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Managing Editor, Dr. Bernhard Haisch
Assistant Editor, Marsha Sims

ERL 306
Stanford University
Stanford, CA 94305-4055

Editorial Board

Prof. Robert Jahn (chairman)
Princeton University
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Book Review Editor

Prof. Henry Bauer
Dept. of Chemistry
Virginia Polytechnic Inst. & State Univ.
Blacksburg, VA 24061-0212

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Editorial: More is More!

So what can this possible mean? One year ago (Volume 6, Number 1) I wrote an editorial entitled, "Less is More!" to convey in a (some say) clever way that JSE was embarking on an ambitious course to increase in size and circulation as an independently published journal. At that time our ambitions for the journal were clear, our plans for achieving them were pretty clear, and our prospects were partly cloudy. I can now report that JSE is doing well and is delivering more value to more readers than ever before.

(1) We made a successful transition to quarterly publication. After some delay in the publication schedule following our changeover from Pergamon Press, we were able to catch up by year's end: Volume 6, No. 4 was mailed in late December.

(2) We published 402 pages last year; when we began in 1987 JSE consisted of 196 pages.

(3) New features have been introduced:

- Book Review section edited by Prof. Henry Bauer
- SSE News section which included substantive reports of the Annual Meeting in Princeton in June, and the Euro-SSE Meeting at the Technical University of Munich in August, along with other items
- The Skeptical Perspective column by Michael Epstein and the Anomalous propagation column by Topher Cooper. We also add a third "Guest Column" beginning with this issue.

(4) Back issue article lists, order forms and subscriber cards (bound in after the last page so as not to be obtrusive) as a nice convenience for readers and a handy way to recruit new subscribers.

All of these things are possible because of the success of the journal and the society in raising money this past year. The Challenge Grant (Volume 6, Number 2) was fully met, and we thank Mr. Robert T. Bigelow for his contribution of \$15000. A number of other substantive gifts were also received. These donations, together with revenues from journal sales, met our budget needs in 1992.

As for 1993, we have an active promotion program in place to increase our subscriber base, but there remains a significant gap between our operating expenses and our sales. For that reason I urge you to make a donation to the journal this year, purchase a gift subscription for a friend, colleague or library, and by all means spread the word about the better-than-ever Journal of Scientific Exploration. (Use the Promotional Copy and Gift Subscription Form at the back of the journal.)

Bernhard Haisch
Managing Editor, JSE

SSE NEWS ITEMS

1993 Annual SSE Meeting

The Twelfth Annual Meeting of the Society for Scientific Exploration will be held at the Best Western Inn at Loretto in Santa Fe, New Mexico June 24 - 26, 1993. Local arrangements have been made by SSE members Dr. John Alexander and Dr. Larry Dossey. Any Associate or Member of SSE is entitled to present a paper at this meeting.

The Program Committee for this year's meeting consists of Henry Bauer, Brenda Dunne, Michael Epstein, Beverly Rubik, and Michael Swords (chairman). The Committee has structured this meeting to be both educational and entertaining. We plan to cover two areas of great current interest. One is the so-called Mind-Body-Consciousness interactions with implications for health, placebo effects, psychoneuroimmunology, and bio-electromagnetism. The other comprises UFO phenomena, especially concrete evidence, physical effects, and abductions. These subjects will be the twin themes of the conference, with talks on these topics dominating two of the three days. A tentative list of invited speakers includes: Robert Ader, Larry Dossey, Beverly Rubik, Harry Rubin, and Jan Walleczek for mind-body topics, and John Alexander, Richard Haines, Donald Johnson, Mark Rodeghier, and Don Schmitt for UFO topics.

To encourage in-depth discussion, the talks and panel discussions will have significant amounts of question time. To allow other SSE members to present papers on their own research, the Program Committee has decided to create what they hope will prove an attractive format for a poster session, to be held in concert with a wine-and-cheese reception.

An excursion to an "exotic" setting will be scheduled: either the Los Alamos Supercomputer facility or the ancient Anasazi Pueblo ruins near Santa Fe. Cocktails and a banquet are scheduled for Friday evening. SSE Associates and Members are encouraged to contribute papers for possible oral or poster presentation. Please submit an abstract (of 200 words or less, to be received by April 24) to the Program Committee Chair: Dr. Michael Swords; Department of Science Studies; Western Michigan University, Kalamazoo, MI 49008.

To obtain your registration material please contact Professor Larry Frederick at Society for Scientific Exploration, P. O. Box 3818, University of Virginia, Charlottesville, VA 22903-0818. Please note that there will be a late registration fee after May 15.

A block of rooms has been reserved at the Inn at Loretto at the special conferences rates of \$98 per night for a single room, and \$108 per night for a double

room. The address is: Inn at Loretto, 211 Old Santa Fe Trail, Santa Fe, New Mexico, 87504, USA, Telephone: 505-988-5531, FAX 505-984-7988. *Please make your own reservations directly with the hotel.* We look forward to seeing you in New Mexico!

SSE President Sturrock Named Honorary Fellow

Society president Prof. Peter Sturrock of Stanford University's Center for Space Science and Astrophysics, has been elected an honorary fellow of the Ukrainian Research Institute on Anomalous Phenomena (RIAP). The institute was recently established in Kharkov, Ukraine to further the scientific study of various anomalies, especially the UFO problem. An English-language *RIAP Bulletin* and a Russian language scientific journal are scheduled to begin publication. RIAP director Vladimir V. Rubtsov is also interested in establishing contacts with researchers and scholars outside the Ukraine. RIAP may be contacted at P.O. Box 4684, 310022 Karkhov-22, Ukraine.

In Memory of Charles Honorton

We all mourn the recent passing of Mr. Charles Honorton. Much has, and will, be written about Chuck's contribution to the field of parapsychology and his particular skill as a researcher. In this note, however, I would rather relate a few personal memories that will reveal Chuck as multifaceted.

I met Chuck at a meeting of the Fundamental Fisiks Group (sic) at the Lawrence Berkeley Laboratory in 1972. At that time, I was just becoming aware of the field, and I was then active in nuclear physics research. To a room filled with 50, or so, skeptical physicists, Chuck handled himself with good humor, appropriate assertiveness, and, of course, great professional skill. This guy, I had to meet! That began a 20-year professional relationship and personal friendship.

While I was conducting field research in India in 1974, I wrote Chuck a 10-page letter outlining a large number of options for the fall: Should I come to Maimonides Medical Center for awhile? Should I bring my hardware random number generator? . . . etc. To illustrate Chuck's willingness to risk, I will quote verbatim his entire response to my lengthy letter and list of questions, "YES!" After all, I was completely unknown to the field, yet he was willing to invite me into his laboratory for an extended stay.

During our early days together at Maimonides, Chuck began his migration to becoming a "techno-wiz." As he said, the most complicated gear he had then was a ping pong ball, and the most complicated technical task was to correctly cut one in half. . . er, excuse me! What I mean to say is create ellipsoidal hemispheres. During a relative short period of time, his lab could boast of a personal computer (long before Apples or IBM PCs) and PSIFI, a state-of-the-art random number generator (RNG), which was set to monitor physiology. For the last few years, we reversed our roles and I often sought his advice on PC matters.

Chuck was brave. He submitted an abstract on our RNG studies to the annual meeting of the American Physical Society (APS). After his presentation, he asked me how come the audience was so uncritical? I hardly had the heart to tell him that the APS never reviews abstracts for meetings, and they always reserve a session for the "nut cases." Sadly, that is where we were! Besides bravery, Chuck could thoroughly enjoy a good belly-laugh, even at himself. It was hours after a beer or two before the tears of amusement dried. During our last meeting at the Las Vegas Parapsychology Convention we argued over who was mentor to whom. In my mind there is no discussion; I have always considered him as my mentor. So, Chuck, unless you are willing to participate in yet another experiment together, I get the last word. "Thanks. I miss you. I love you."

*Edwin C. May, Director
The Cognitive Sciences Laboratory
Science Applications International Corporation
1010 El Camino Real, Suite 330, Menlo Park, CA*

In Memory of Michel Gauquelin

Michel Gauquelin, a member of the Society of Scientific Exploration and the discoverer of planetary effects which have proven to be the hardest rocks among the anomalies of science, passed away May 20, 1991. He decided the day of his death. His friends had noticed increasing signs of distress and physical disturbance, a state of strain that seemed to overpower him.

The root of the Gauquelin tragedy might be found in his struggle of many years, to no effect, for acknowledgement of his discovery in mainstream science. Great efforts at defending his empirical observations against successive attacks from three skeptical organizations had worn him out. In each of them he became entangled with ill-will and strategies so dubious some members of the adversary camps even left their organizations in protest. Michel Gauquelin stayed on the battlefield for nearly three decades, but toward the end of his life he repeatedly complained that the strain of those decades of combat had used up his physical resources.

Gauquelin did not leave behind any document explaining his decision, but there is one deplorable hint. By his last will he demanded that all empirical data amassed through his lifetime, more than 30,000 birth documents on file in perfect order, must be destroyed. His will was put into effect, and it is almost inevitable to understand his act as a charge, not only directed at those who had not played fair with him but directed at all scientists not serving and suffering as much as he did in the pursuit of scientific truth.

Gauquelin was born in Paris on 13 November, 1928, the son of a dentist. From his father, an amateur astrologer, he gained his first interest in "the stars," making himself knowledgeable in the field by reading popular books on the subject found on his father's shelves. Psychology courses at Sorbonne University and the accompanying necessity to learn scientific principles and methodology, however,

generated skepticism and a strong sense for the crucial role empirical evidence must have in establishing and improving human knowledge.

Still a student, he subjected astrological hypotheses to empirical tests. The results were negative and he found severe methodological flaws in earlier statistical studies purporting to show positive relationships between astrology and various facets of human life. But then, by using the birth data of famous physicians, members of the Academie de Medecine, he discovered considerable relationships between the frequencies of births and certain planetary positions, of a type not predicted by astrology, discoveries which have since become known, collectively, as the Gauquelin effect. Immediate replications furnished confirmation of his first observations. By gradually extending his sample categories to more professions (eleven in all) and to other nationalities (five in all), he ended up finding an entire pattern of relationships among professions and the planets Mars, Jupiter, Saturn, Venus, and the Moon.

Gauquelin occasionally noted that at that time he was liable for one grave fault: He had published his planetary results under his own name instead of a pseudonym. The reactions of the Sorbonne authorities to his two pioneering works, published in 1955 and 1960, precluded any academic career after his graduation. Gauquelin thus had to earn his living by administrative counseling and by writing popular psychological guidebooks. His vocation, however, was science, and under those unprosperous conditions he would certainly not have achieved the ends of his research without his wife, Francoise Schneider-Gauquelin, who helped him to collect and analyze the masses of data needed to buttress his empirical bridgehead.

Further work went in two directions interwoven by theory: The first was toward what was called planetary heredity. Do planets at the birthtimes of children prefer the positions they had when their parents were born? Gauquelin reported that they do. The second objective was a search for proximal links: Do the positions of planets at the birth times of professionals indicate future careers or else - Gauquelin's hypothesis - innate character dispositions? Gauquelin, assisted by his wife, extracted 15,000 character traits from biographies. Statistical comparisons of several kinds which he set forth provided support.

Anyone interested in Gauquelin's tremendous work spread over more than 200 technical papers could well keep overviews on its progress by just reading the monographs: *The Cosmic Clocks* (1967), *Cosmic Influences on Human Behavior* (1973), *The Truth about Astrology* (1982). His early French works turned up late in abbreviated English editions: *Written in the Stars* (1988/1960), *Planetary Heredity* (1988/1966). In his last book (*Neo-astrology - a Copernican Revolution*), Gauquelin not having found any ordinary place for his life's work in the present world tracked it down to the past. His ancestress, he confessed, was astrology whose system of beliefs, even though abounding in error, had sensed and preserved certain bits of truth. In his view "neo-astrology" - a derogatory label coined by his adversaries which he eventually adopted with irony - was for

him the inception of revolutionary changes, of Copernican proportions, for science no less than for astrology.

Gauquelin's work has been called Herculean sometimes, but was it Sisyphian at the same time, i.e., in vain? Apparently not, as a revival of the old Mars effect debate is starting off with greatly improved prospects. Gauquelin's claim has received fresh support from new data and analyses generated by new confederates as well as by new doubters. Still, some seemingly sparkling evidence among findings that he deemed established had bluntly been challenged of late: The heredity hypothesis, as he himself reported, had dwindled away under computer analysis of newly-collected data. His cherished character trait hypothesis was also questioned by some. Gauquelin may have, at long last, lacked the vigor required to continue playing his revolutionary role in the next act of his planetary drama. He left the responsibility for its denouement to posterity.

*Suibert Ertel
Institut für Psychologie
Gossler Strasse 14, 03400 Göttingen, Germany*

ANOMALOUS PROPAGATION

by Topher Cooper

(This column is dedicated to the late Charles Honorton. Though parapsychology will sorely miss his continued work, he himself will be missed much more.)

The publication schedule for JSE is such that I'm finishing up writing this second column just as subscribers are receiving their copies of the first column. There has not, therefore, been much time for me to receive any questions or comments. I will say again, therefore, that I do want to hear from you. Especially welcome are items which you think may be appropriate for this column: there is only so much that I can read myself looking for material. The more contributions, the better this column can be. The first person who sends me each item that I use will be acknowledged in the column. The addresses to which you can send your contributions, comments and questions appears at the end of this column.

No Evidence for Health Effects of E-M Fields

An expert panel concluded that there is no convincing evidence that there are any negative health effects from low-frequency electric and magnetic fields. The panel was asked to conduct a review by the Committee on Interagency Radiation Research and Policy Coordination, which is a subcommittee of the (US) Federal Coordination Council on Science, Engineering and Technology. They reviewed over a thousand articles to reach their conclusions. They did feel, however, that there was some evidence of biological effects and that more research should be conducted on these, even if no health consequences had yet been demonstrated.

SOURCE: UPI; November 13, 1992, "Electromagnetic fields pose no health hazard, panel says"

Strong Evidence for Health Effects of E-M Fields

The Swedish National Board for Industrial and Technological Development has decided, on the basis of two Swedish studies, that the evidence for cancer risk from power line exposure is sufficient to justify regulations intended to minimize exposure. Jaak Noeu, director of the board's electrical safety department, describes the link as 80% certain.

The two studies, one by Maria Feychting and Anders Ahlbom of the Karolinska Institute in Stockholm and the other by Birgitta Floderus of the Swedish National Institute of Occupational Health in Solna, were able to take advantage of the very complete and accurate records available in Sweden to assemble strong cases. Feychting and Ahlbom demonstrated a dose/effect relationship for the first time. Children who were younger than 15 years old and who had an esti-

mated average exposure of more than 0.2 microteslas were 2.7 times as likely to contract leukemia, while an average exposure of over 0.3 microteslas was correlated with a risk of 3.8 times the average rate. Although the dosages used in this study are estimates rather than direct measurements, they are considerably more accurate estimates than used in any previous studies. This is because of the availability of detailed records of actual power line current for each line in the grid.

SOURCES: New Scientist; October 31, 1992; (Vol. 136, #1845), p. 4; "Swedish Studies Pinpoint Power Line Cancer Link", by Andy Coghlan.

Intellectual Courage

David Bohm, a major figure in the development of quantum mechanics, died on October 27, 1992 of a heart attack. The obituaries in the popular press, such as the one cited below, tended to emphasize a single incident which gave him national prominence outside the world of science: he was acquitted of charges of contempt for refusing to answer the questions of the infamous House Un-American Activities Committee. He was subsequently black-balled and was forced to leave Princeton University for Brazil and Israel.

Bohm discovered a number of highly counter-intuitive predictions of quantum mechanics and, in 1951, wrote the book "Quantum Theory", which remained the standard text for decades. Later, he attempted to address what many feel are weaknesses in the standard interpretation of quantum mechanics (the Copenhagen Interpretation) with his own "realist" interpretation. Bohm felt that his interpretation showed that there was room in quantum mechanics – perhaps even a necessity – for psi phenomena, and was not shy about saying so.

SOURCE: New York Times; October 29, 1992; p B16 (Obituaries) "David J. Bohm, 74, Physicist Acquitted in McCarthy Years", by Eric Pace

Nervous Plants

Plants may perceive their environment in a way which is more like that of animals – especially primitive animals – than was previously thought. It has been known for about two decades that wounding one leaf of a plant can result in triggering defensive reactions in others. It has generally been thought that this was due to a chemical message traveling with the flow of fluids in the phloem (the circulatory tissue of the plant). No one, however, has been able to unambiguously identify the chemical responsible.

Now it appears that, at least for the one particular example studied, there is no such chemical. A very clever series of experiments has established that the information is transmitted via an electrical "action potential", just like information is transmitted through animal neurons. The plant cells that take part in this communication are not specialized for the purpose, though, and the signal apparently moves between them by electrical conduction, rather than by the chemical signaling that takes place at neural synapses in complex animals. The plant system, therefore, more closely resembles the nervous systems of simple animals like jelly fish.

SOURCE: Nature; November 5, 1992; (Vol. 360, no. 6399) pp62-65, "Electrical signaling and systemic proteinase inhibitor induction in the wounded plant", by D.C. Wildon, J.F. Thain, P.E.H. Minchin, I.R. Gubb, A.J. Reilly, Y.D. Skipper, H.M. Doherty, P.J. O'Donnell, and D.J. Bowles; Nature; November 5, 1992; (Vol. 360, no. 6399) pp14-15, "Potential awareness of plants" by Keith Roberts.

Galileo Cleared . . . Pictures at 11

Galileo has finally been officially cleared of charges of heresy by the Roman Catholic Church. This is the result of the conclusions reached by a commission of historic, scientific and theological inquiry after thirteen years of study on the matter. Contrary to widespread assumptions, however, the commission was not charged with ruling on whether Galileo had been correct in his heliocentric beliefs – that has been accepted by the Church for a long time. Rather they set out to determine whether or not the judges who convicted Galileo had acted correctly in light of the theological and scientific evidence available to them. The commission ruled that the judges had made the conviction in error. On the other hand, the commission also ruled that the errors had been made in good faith rather than in willful disregard for the evidence or theological law. They stated, for example, the scientific evidence that Galileo provided in his defense did not unambiguously prove his heliocentric theories.

SOURCE: Los Angeles Times; October 13, 1992; "Catholic Church Clears Galileo of Heresy Charges by Vatican" by William D. Montalbano.

Biomagnetic Bibliography Available

Reader Michael Levin, a graduate student in developmental biology at Harvard University, has a long term interest in the interactions of electromagnetic fields and living systems. He has assembled a bibliography of scientific papers which covers all aspects of this kind of research. He encourages anyone who is interested in getting a copy of this bibliography or who has questions about the topic to get in touch with him.

SOURCE: by Internet email – mlevin@husc8.harvard.edu; by postal mail – Michael Levin; 43 Worcester Ave.; Swampscott, MA 01907

Transtemporal Logic

Q: Why did the tachyon cross the road?

A: Because it was already on the other side.

SOURCE: R. J. Davies on the USENET sci.physics newsgroup.

Cosmological Anomaly Resolved

In 1988 the galaxy B2J0902+34P in the constellation Lynx was studied with rather surprising results: stars within the galaxy were found to be much older than the galaxy itself. A new study by Peter Eisenhardt and Mark Dickinson,

using improved sensors and analysis algorithms, shows that the stars in the galaxy are much bluer, and thus younger, than the previous study indicated.

SOURCE: NASA News, October 30, 1992; "Astronomers resolve mystery associated with extremely distant galaxy" (Contributed by Bruce R. Kepes)

All The Anomalies Fit to Print

Wouldn't it be nice if someone had worked for years to assemble a multivolume catalog of "all phenomena that cannot be readily explained by prevailing scientific theories?" What if the intermediate products of this immense effort were made available – each anomaly briefly but clearly described, classified, the source (primarily scientific journals) carefully cited, and, in some cases "scores" for reliability and degree of anomalousness added? What if, as an extra, a selection of relevant books judged worthwhile by the compiler could be ordered from the same source?

As you've probably guessed, there is such a resource. It's William R. Corliss's "The Sourcebook Project". You can write for information to the address given below. The newsletter, *Science Frontiers*, is a particularly good bargain: it's free if you buy something else once in a while, or can be had separately for a reasonable fee.

SOURCE: The Sourcebook Project, PO Box 107, Glen Arm, MD 21057

Traditional Medicine Degrees for Foreigners

Foreigners may now obtain advanced degrees in traditional Chinese medicine at the recently opened "International College of Chinese Traditional Medicine" in the city of Tianjin. Master's degrees will be available in eight specialties as well as doctorates in acupuncture.

SOURCE: *Nature*; November 19, 1992 (Vol. 360, #6401), p. 201, "News – China in Brief", by You Qin Li

Windows of the Mind

When part of an area of the brain known as the primary visual cortex is damaged, the corresponding areas of the visual field disappear from consciousness. Generally that is the end of the story – but some affected people are able to guess about simple properties of objects in the blind areas with much higher accuracies than can be explained by chance (frequently protesting all the time that the guessing is a waste of time since they are unable to see a thing). This is the phenomenon of "blindsight."

It has traditionally been assumed that blindsight is due to visual information being routed around the primary visual cortex through auxiliary visual processing centers. This assumption has been called into question, for at least one instance of blindsight, by a new study. In the patient under study, new high precision techniques for mapping the affected area showed that there was a tiny island of spared neural tissue within the general devastation, and that, furthermore, the

blindsight phenomenon only occurred for images presented to that tiny island. In this patient, at least, the blindsight information appears to be routed directly through patches of undamaged tissue within the primary visual cortex.

Sources: Science; November 27, 1992 (Vol. 258, #5087), pp 1489-1491, "Residual Vision in a Scotoma: Implications for Blindsight", by Robert Fendrich, C. Mark Wessinger, and Michael S. Gazzaniga; Science; 27 November, 1992 (Vol. 258, #5087), pp 1438-1439, "Unraveling the Dark Paradox of 'Blindsight'", by Marcia Barinaga.

Opinions expressed in this column are those of the author and do not necessarily represent those of JSE. Comments may be directed to the author by electronic mail at "cooper@cadsys.enet.dec.com" or US post at Topher Cooper, Digital Equipment Corporation, 77 Reed Road (HL02-3/G13), Hudson, MA 01749.

THE SKEPTICAL PERSPECTIVE

by Michael Epstein, National Capital Area Skeptics

Local Skeptic Group Activities

Rather than their sharp focus on religious visions and miracles which characterized my last report, local skeptic groups have broadened investigations to cover a somewhat wider variety of phenomena and claims. The Tampa Bay Skeptics hit the streets in search of a "real" psychic (Anonymous, 1992), with the author, who suffered from a medical condition, remaining undercover to prevent any information being transmitted to prospective subjects. They also investigated a claim of the miraculous appearance of the "Holy Spirit" in the form of a dove on the glove of a surgeon performing a triple bypass operation. A photograph and videotape of the apparition showed it to be simply a combination of light and shadow, random layering and bunching of cloth under a surgical glove, and possibly a spot of blood (Johnson, 1992). Besides hosting the 1992 CSICOP conference, the North Texas Skeptics are still waiting for dowser Bette Epstein (no relation) or her daughter Cassie to meet their \$6,000 challenge and prove their powers (Sullivan, 1992). Joe Nickell, well-known skeptical author was present for the alleged visit of the Virgin Mary to a Catholic Church in Cold Spring, KY. Nickell saw no miracles and said most of the individual experiences could be naturally explained (Peterson, 1992). The New Zealand Skeptics awarded their "Bent Spoon" Award to Consumer magazine for an article that "virtually endorsed questionable medical therapies" such as homeopathy, aromatherapy, and acupuncture (Hyde, 1992). The magazine replied that they might present the skeptics with an award of their own: a magnifying glass so that they could read the article in detail. The Australian Skeptics presented their Bent Spoon for 1992 to Allen Roberts, archaeological research consultant for Ark Search, who claimed that a formation in the mountains of Turkey was the remains of Noah's Ark (Williams, 1992). The award committee decided on the award when a vision appeared before them and a voice spoke unto them saying that the formation was not the Ark or a syncline, but a giant, fossilized bent spoon. A member of the Bay Area Skeptics related his experiences visiting the recently opened Institute of Creation Research Museum of Creation and Earth History (Linke, 1992) and several members (including me) of the National Capital Area Skeptics (NCAS) attended a recent "Back to Genesis" seminar that drew over 7,000. Other NCAS projects were an examination of the Natural Law Party, which had close ties to the Transcendental Meditation movement, and an expose of chain letter scams (Denman, 1992).

Sex and the Skeptic

Perhaps in an attempt to reach a new market, the January 1993 issue of *Hustler* magazine brought us the skeptical "Spaced Out: Starstruck UFO Freaks and the Company They Keep", a fringe group profile by Doug Vincent. No, I didn't discover this through my normal reading habits . . . I even gave up my *Playboy* subscription a few years ago. I was alerted by the firestorm created on the Bitnet Skeptic discussion group when the article first appeared. The volume of electronic mail grew so large that the host computer halted all traffic and the board was down for over a week. The article, while not particularly deep or revealing, was a well-written expose of the author's experiences at the National New Age and Alien Agenda Conference in Phoenix, Arizona, and the International Symposium on UFO Research in Denver, Colorado. Among those mentioned prominently are Dr. Edith Fiore, Linda Moulton Howe, and Daniel Drasin, with Phil Klass getting a ringing endorsement at the end of the article. And by either coincidence, synchronicity, or because of the editor's bizarre sense of humor, there was a pictorial titled "Randi: Home Wrecker" in the middle of the skeptical UFO article. At least her name wasn't Phil.

Skeptic Conferences

The recent CSICOP conference (Oct 16-18, 1992) in Dallas, TX was profiled by James Lippard of the Arizona Skeptics. His excellent coverage is planned for publication in the Nov/Dec 92 and Jan/Feb 93 issues of *The Arizona Skeptic* and was also electronically posted to the Bitnet Skeptic discussion group. The EuroSkeptics meeting (July 17-19, 1992) held in St. Vincent, Italy was also reported in several articles (Alcock, 1993; Denman, 1992; Kurtz, 1992).

More Than You Ever Wanted to Know about CSICOP

Recently posted in the library of the UFO/Paranormal Issues (National Issues) section of CompuServe is a discussion thread dealing with CSICOP. A good deal of the thread is a lively debate between Rick Moen of the Bay Area Skeptics and Richard Broughton of the Parapsychological Association, started when Richard provided information concerning George Hansen's comprehensive article on CSICOP (Hansen, 1992). The discussion, which took place in March of 1992 was placed in the library in September and October of 1992.

When is a Skeptic Not a Skeptic?

When he's a dogmatist and his name is Nicholas Humphrey, the recently appointed Cambridge University research fellow in parapsychology. "I want to show not only that these things don't happen, but that they are logically impossible, that the paranormal is all in the mind" spoke Dr. Humphrey, when interviewed by the *Times of London* and quoted in the leadoff article of *News and Comment* in the Winter 1993 issue of the *Skeptical Inquirer* (SI) (Jones, 1993). Dr. Humphrey is (according to SI) a distinguished theoretical psychologist,

author of "A History of the Mind," and a leading expert on the evolution of the brain. Unfortunately, he also appears to be one of those "experts" who throughout the centuries has proclaimed the anomalous to be either fraud or explainable by current scientific knowledge. Such "experts" trace their lineage through such as Francisco Sizzi (Professor of Astronomy) who debunked Galileo's claim to see moons of Jupiter as follows: "Jupiter's moons are invisible to the naked eye and therefore can have no influence on the earth, and therefore would be useless, and therefore do not exist" (Cerf, 1984).

I would support Dr. Humphrey's approach to parapsychological research if it was limited to proving his thesis experimentally or by the careful examination of the evidence, as the noted parapsychologist and skeptic Dr. Susan Blackmore has done. However, I'm skeptical about Dr. Humphrey's real agenda when he continues to be quoted (more appropriate to Free Inquiry than SI . . . did this manuscript slip into the wrong pile in the Institute for Inquiry?) that "Roman Catholicism without the paranormal would be nothing; it needs its miracles. Praying has no paranormal benefits – statistically, it is not going to help." Does that mean that there are "normal" benefits . . . that is what most people pray for! And let me assure Dr. Humphrey that, despite the faults of that religion, the social outreach programs of Roman Catholicism make it much more than "nothing" without the paranormal.

Skepticism and Theism

The Chairman's Corner in the Tampa Bay Skeptics Report (Smiljanich, 1992) focused on the author's concepts of skepticism and theism, and generated some heated responses from the reading audience. I found the discussion, which implied that skepticism and theism are not mutually exclusive, to be logical and well presented. Defining religion as general theological questions of God, absolutes, morals and values, rather than any particular organization, Terry produces some memorable quotes:

"Skepticism and rationalism are themselves grounded on a trust (a 'faith') in their ability to provide true knowledge."

" . . . all belief systems are based ultimately on a form of faith."

" . . . only falsifiable claims can be accepted as potentially contributing to true knowledge."

"Science and religion are not at war . . . the enemy of knowledge is irrationalism, not religion"

"Science is not a revealed or discoverable body of absolute truth. It is our attempt to organize and understand the manifestations of the reality that we encounter, and is forever subject to revision"

"It is proper to be skeptical about extraordinary claims that are outside the generally accepted beliefs and theories of science. That is the only way to maintain progress in scientific knowledge . . ."

"Many religious claims (e.g. a young earth, or the power of prayer) are in conflict with science, and we should not shy away from battle merely because they are labeled 'religion'. Belief in the existence of God, however, is . . . perfectly compatible with being a skeptic about worldly claims . . ."

Last Words

It would be fitting to end with the last words of Galileo Galilei, skeptic and believer, who, as noted in *Anomalous Propagation*, has been rehabilitated. On his death bed, the valiant Italian astronomer declared: "Yet it still moves." (Brandreth, 1989)

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Opinions expressed in this column are those of the author and do not necessarily represent those of the National Capital Area Skeptics, JSE, or any other organization. Comments may be directed to the author by electronic mail at CompuServe: 76640,1540 or Internet: mse@enh.nist.gov or by U.S. post at Michael Epstein, B-222 Chemistry, NIST, Gaithersburg, MD 20899. Copies of some articles are available on electronic media and others may be available as hard copy.

GUEST COLUMN: MAJOR MEETING ON NEW COSMOLOGIES

By Tom Van Flandern

Meta Research, 6327 Western Ave, NW, Washington, DC 20015

The dominant model in cosmology today is the Big Bang theory in which the entire universe originated in an explosion of space and time about 10-15 billion years ago. But the interpretation of the observations on which the model is based has been called into question by recent observations. For example:

1. Some objects have discrepant redshifts that apparently are not due to velocity. [Arp, H. and Sulentic, J.W., "Analysis of groups of galaxies with accurate redshifts," *Astrophys. J.* 291, 88-111 (1985).] Moreover, it has now been confirmed that redshifts of nearby galaxies are apparently somewhat quantized, and possibly time variable. [Tifft, W.G., "Properties of the redshift. III. Temporal variation," *Astrophys. J.* 382, 396-415 (1991).] This has raised doubts about whether the cosmological redshift of distant galaxies is actually due to expansion of the universe, as is assumed in the Big Bang.
2. The microwave blackbody radiation with a 3-degree absolute temperature is usually assumed to be a remnant of the Big Bang fireball. But a comparison of the intensities of galaxies at infrared and radio wavelengths shows that there is considerable intergalactic absorption. Such absorption would prevent us from being able to see microwave radiation from beyond all the galaxies, suggesting that the blackbody radiation must have a more local origin. [Lemer, E.J., "Radio absorption in the intergalactic medium," *Astrophys. J.* 361, 63-68 (1990).]
3. Four "pencil-beam surveys" of galaxies show large-scale structure out to distances of over a billion parsecs. These appear to be a succession of features at roughly 150-million-parsec intervals, all similar to the nearest one called "The Great Wall". Such giant structures are incompatible with galaxy formation mechanisms in the Big Bang model. [Kurki-Suonio, H., "Galactic beads on a cosmic string," *Sci. News* 137, 287 (1990).]
4. Many galaxies in our part of the universe appear to be streaming in one direction. At first, the existence of this flow was thought to be due to a "Great Attractor" pulling all these galaxies in its direction. However, this has not been confirmed by recent studies, which failed to show the expected backside infall. But if the 3-degree microwave blackbody radiation, used as a standard of rest for measuring these galaxy peculiar velocities, were actually closer and somewhat asymmetric, the apparent galaxy-streaming effect would vanish. [Mathewson, D.S., Ford, V.L., and Buchhorn, M., "No back-side infall into the Great Attractor," *Astrophys. J.* 389,

L5-L8 (1992); Lindley, D., "Not so Great Attractor," *Nature* 356, 657 (1992).]

But if the Big Bang (BB) model is not fundamentally correct, then what do we really know about the nature and origin of our universe? Many of the world's expert cosmologists who dispute the Big Bang theory met in Lodz, Poland during the week of September 7-12, 1992. The conference theme was "Progress with new cosmologies". The North American organizer and coordinator was Roy Keys. The following is a brief report on the highlights of the meeting. About 60 scientists and students were in attendance.

Jack Sulentic, University of Alabama, gave a review talk about compact groups (CG) of galaxies. These are small groups of typically 4-5 close galaxies which are grouped more densely on the sky than the general field by a factor of a hundred or more, yet are not part of any recognized cluster of galaxies. The most famous example is Stephan's quintet. But these CG's present many challenges to cosmologists. For example, their calculated stability is low, with predicted collapse in just a few hundred million years. There is an excess of discordant redshift members (defined as a velocity difference of more than 2000 km/s), with far more of them present in CG's than chance will allow. Various lines of argument can effectively rule out the possibilities that most CG's are: a) chance unrelated optical configurations; b) chance loose optical associations; c) transient unbound cores in loose groups; or d) recently collapsed from larger groups. Present data suggest that CG's were either always compact, or were recently formed as is. Neither possibility is easily reconciled with Big Bang expectations. It was noted that CG's would be more stable (have longer predicted lifetimes) if gravity were proportional to $1/r$ instead of $1/r^2$ on the large scale. Three other talks presented various theoretical models implying or requiring just that. Such a change in the behavior of large-scale gravitation would also completely eliminate the need for the invention of "dark matter" as the dominant ingredient of the universe.

Halton Arp, Max-Planck Institute for Astrophysics, spoke about his "Variable Mass" cosmology. He pointed out the need for cosmologies to explain why quasar redshifts cluster near 0.3, 0.6, etc., with the grouping just below $z = 1.2$ dominating all others; and why certain classes of stars have significant excess redshifts. He also pointed out the inconsistency that local galaxy groups seem to have velocity dispersions of less than 100 km/s, while distant groups seem to have members with dispersions up to 1000 km/s.

Eric Lerner, author of "The Big Bang Never Happened, discussed BB element abundance predictions. He pointed out that, in the case of each prediction, an observational contradiction exists. He then discussed the various ways in which BB cosmologists "fix" these problems. One "fix" is increased deuterium destruction. But this overproduces energy (the galaxy gets too bright), as well as He^4 , He^3 , and CNO. Another "fix" is an inhomogeneous Big Bang; but among other problems, the lithium abundance is still not right. Moreover large scale structure implies an age for the universe of greater than 63 Gyr (billion years). Invoking a large cosmological constant to solve this problem is in conflict with

the observation that gravitational lenses are rare. Another "fix" is to invoke matter-antimatter annihilation at about two seconds after the BB. But this limits the baryon density to a small fraction of what is observed. Finally, the angular size versus redshift relationship is far below the BB predicted lower limit for galaxies, clusters, and radio lobes. Lerner then discussed how PC (plasma cosmology) might deal with some of these problems. His book contains an extensive discussion of this alternate cosmology as well.

William Napier, Royal Observatory, presented an independent verification of Tiff's puzzling periodicity in ordinary galaxy redshifts. A completely new sample was chosen, with careful anti-biasing considerations. The goal of the team was to show that Tiff's "impossible" anomaly would vanish. But their results showed a strong, definite periodicity in galaxy redshifts out to the sample limit of 2000 km/s. Redshifts occur preferentially at integer multiples of about 37.5 km/s after corrections for the Sun's galactocentric motion. This was taken as confirmation of Tiff's results from a decade ago, which have been largely ignored because they are so difficult to explain. Among many other problems, this result implies that intrinsic, motion-based redshifts of galaxies must be quite small compared to 37.5 km/s. This would help explain how galactic clusters stay bound, but introduces many new intractable problems, especially for the Big Bang.

In his second talk, Jack Sulentic spoke about multiple redshifts seen in some quasars and AGN's. Line profiles come in all types: symmetric, double-peaked, and asymmetric. Relative shifts are both toward the red and the blue. Arguments against an accretion-disk/black-hole model were reviewed. Apparently a non-Doppler redshift-blueshift mechanism is needed. For example, one broad H-line (in Markarian 668) shifts back and forth by 1000 km/s relative to another narrow H-line, with an average offset of 2000 km/s. These shifts correlate perfectly with intensity.

Amitabha Ghosh, Indian Institute of Technology, presented data on white dwarf redshifts. The gravitational redshift portion is measurable from line profile widths, and can be separated from any Doppler shift. Based on masses from binary star cases, and radii from the Boltzmann law, the implied surface gravity tells us that typical white dwarf gravitational redshifts should be 60-70 km/s. But profiles are about 40 km/s wider than that. This is a known but unsolved problem for those working on redshift mechanisms.

Andre Assis, State University of Campinas, noted that Eddington (1926; reprinted 1986; "Internal Constitution of Stars", Ch. 13, "temperature of space = 3 degrees"), Nernst (1937), and Regener (1933) all predicted a 3-degree microwave radiation based on the equilibrium temperature of interstellar and intergalactic photons. But Peebles' (incorrect) prediction that the Big Bang fireball would leave a background remnant at 30 degrees Kelvin got all the press when the microwave radiation was discovered in 1964.

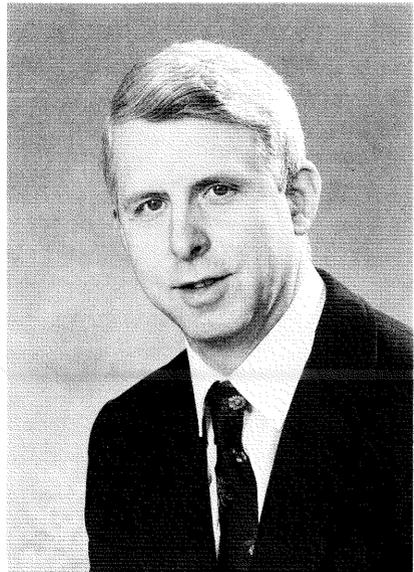
In my own talk I presented the "Meta Model", a new cosmology derived deductively from first principles that provides alternative explanations for the

cosmological redshift, the microwave blackbody radiation, the hypothetical "dark matter," and many other anomalies in standard cosmology. Details are in my new book, "Dark Matter, Missing Planets and New Comets," North Atlantic Books (1993). The book also contains intuitive and understandable models never before presented for gravity, relativity, and many other fundamental concepts of physics. Astronomy-interested readers will find much original material to ponder in the book's pages, including discussions of the origins and nature of solar system bodies, prospects for a "Planet X", etc.

The conference was information-intensive, and the conferees quickly bonded despite their diversity of views and the many alternative cosmologies discussed. There was near-unanimity of agreement that many standard-model advocates have compromised scientific objectivity when considering fundamental objections to the Big Bang model as a whole, or when evaluating alternative cosmologies.

Conference proceedings are expected to be published during 1993. Plans are also being made for a follow up conference during the summer of 1993, probably at an Eastern United States location.

Tom received his Ph.D. degree in Astronomy from Yale University in 1969, specializing in celestial mechanics. He spent 20 years at the U.S. Naval Observatory, where he became the Chief of the Celestial Mechanics Branch. Tom has now formed his own Washington, DC-based company, Meta Research, to foster research into ideas not otherwise supported because they conflict with mainstream theories in Astronomy. One of his recent papers may be of interest in connection with the meeting review in this issue: Halton Arp and Tom Van Flandern co-authored "The Case Against the Big Bang" in *Phys. Lett. A* 164, 263-273 (1992).



Acculturated Topographical Effects of Shamanic Trance Consciousness in Archaic and Medieval Sacred Landscapes

PAUL DEVEREUX

Penzance, Cornwall, United Kingdom

Abstract — Various linear enigmas exist in ancient sacred landscapes worldwide. These include examples of Native American landscape lines, such as the Chacoan "roads", New Mexico, and the Nazca "lines", Peru; Neolithic linear earthworks, called "cursuses", in Britain; stone rows in Europe, Malaysia and elsewhere; temple alignments in Indonesia. There is also the archaeologically heretical idea of "leys" (alignments of ancient sites), put forward by Englishman Alfred Watkins in 1921. Although the ley theory has long been derided by mainstream scholarship, new German and Dutch findings show that there was a medieval tradition of straight "Doodwegen" (death roads) or "Geisterwege" (ghost paths). It seems Watkins may have unwittingly uncovered vestiges of these features. Certainly Watkins had no concept of current "New Age" notions of "energy ley lines", which are modern fantasies. It is argued that such medieval features arise out of a deep-seated, universal conceptual complex associating "spirit ways" with straight lines: straight cords and threads in ancient traditional healing practices as well as straight tracks and other ceremonial landscape markings. It is suggested that these ideas have their roots in archaic shamanism, which, throughout Eurasia, influenced later, ceremonial aspects of monarchy. A proto-Indo-European language vestige is cited.

Preliminary evidence is presented indicating that the spirit – line association derived from the ecstatic "journey" experienced during the shamanic trance. This gave rise to images of "flying shamans" in tribal societies throughout the world, and, ultimately, to the "magical flight of the sovereign" in proto-state and state societies. It is this "flight of the soul" that seems to have been translated onto ancient sacred landscapes as straight lines, which later became variously acculturated as sacred ways, spirit and fairy paths, roads of the dead or of ghosts, or Royal Routes. The neurological aspects of the so-called out-of-body state, and its possible association with modern psychological epidemics such as "UFO abductions", is alluded to.

Introduction

Because the origins of landscape lines seem to relate to a universal experience provided by the human central nervous system (i.e., the out-of-body experience), the basic nature of landscape lines worldwide can be studied cross-culturally.

Enigmatic straight lines of various kinds exist in many ancient ceremonial landscapes worldwide, a famous example being the complex of lines on the pampa near Nazca, Peru. No one has hitherto been able to provide an explanation for such curious features, but now an exciting and somewhat unexpected rationale is coming into view. But first, I'll describe some of these landscape lines.

American Indian Lines

Nowhere has more surviving or recorded examples of these lines than the Americas. The Californian sierras contain the remnants of straight tracks worn deep by the now-extinct Miwok Indians. The tracks were up to 40 miles in length (Barrett and Gifford 1933; Sample 1950). In New Mexico, straight, so-called "roads" radiate out for tens of miles around Chaco Canyon (Kincaid et al 1983; Frazier 1986; Lekson et al 1988). These were built about a thousand years ago by the now-lost Anasazi, the "Ancient Ones", and Chaco Canyon is currently seen as having been one of their key ceremonial centers. The roads are engineered features, not mere trails, being a fairly constant nine meters wide, with spur roads about half that width. They are now difficult to see at ground level. They change direction only occasionally and seem associated with pueblo complexes called Great Houses — several roads meet at Pueblo Alto, for example, on the northern rim of Chaco Canyon. Where the roads come to the top of the canyon walls, what appear to be ceremonial stairs were carved out of the living rock of the canyon sides.

But why did a people without the horse or the wheel need such strange "roads"? The mystery has been compounded by recent computer-enhanced infra-red air surveys by NASA, which reveal multiple parallel sections to the roads which are no longer visible to the eye or the normal airborne camera (Sever 1990). Most researchers now think these roads were ceremonial or sacred ways (Kincaid et al 1983; Sever 1990; Lekson et al 1988). Broken pottery, probably resulting from votive activity, has been found in patches along some of the roads.

Moving southwards, evidence of pre-Hispanic straight track systems in Mexico survives in the region around the archaeological site of La Quemada. About 100 miles of Native American roads have been identified there, and dated to around 700 AD (Trombold 1991). In the Yucatan Peninsula of south-eastern Mexico, we enter the domain of the ancient Mayan culture. The Mayans interconnected their sacred cities with straight roads (sacbeob), the longest-known one being the 100-kilometer length that links Coba with Yaxuna. Explorer Thomas Gann encountered this feature in the 1920s and marvelled that it was "straight as an arrow, and almost flat as a rule." (Robertson 1983).

In South America, the Kogi Indians of northern Colombia have networks of paved tracks which extend over their territory (see below). The Inca had 41 ceques that radiated out from the Coricancha, the so-called "Temple of the Sun", in Cuzco, Peru, that linked sacred places or wakas. Investigation has shown

that these lines were variously used for sun watching and multifarious ritual purposes, including straight-line pilgrimages. Indeed, the lines had considerable socio-religious importance within Inca society, involving *mit'a* or shared labour, kinship groupings, ancestor worship and water supply (Hadingham 1987; Aveni 1990 [2]). Probably the most famous of American Indian straight lines are the markings on the pampas around Nazca, also in Peru. Formed by removal of the desert varnish revealing a lighter subsoil, the Nazca lines are of various widths, ranging from large trapezoid areas to track-width features. They can be up to several miles long, and pass without deviation over ridges. They criss-cross the pampa in bewildering array and are interspersed with a variety of unicursal ground drawings of animals and geometric forms (Reiche 1968; Reiche 1974; Morrison 1978; Morrison 1987). The lines have been noted by Western researchers for several decades, but the most significant work on them has been done only as recently as the 1980s by an inter-disciplinary team co-ordinated by Anthony F. Aveni of Colgate University (Aveni et al 1990). No strongly significant astronomical orientations have been found in the lines, but Aveni and colleagues have confirmed that a network pattern is embedded within them: there are over 60 star-like "ray centers", from which lines radiate like the spokes from the hub of a wheel, and at least one line from each center links with another. Amongst other things, Aveni's team also discovered:

- a) that well-worn trails were engraved within the pure geometry of some of the lines — it is thought this was caused by ritual walking, perhaps similar to the straight-line pilgrimages of Cuzco (above);
- b) that some pampa lines seemed directed at the Nascan ritual center of Cahuachi, from where archaeologist Helaine Silverman suspects "the Nasca priest-scientists observed the natural and supernatural world"; and
- c) various types of stone structures exist amongst the lines.

Silverman and anthropologist Gary Urton also suspect that the lines were ritually swept and cleaned during their period of use. Urton witnessed this ritual cleaning still being practised in a Christian context in an Andean community: before a saint's image was paraded around the plaza in front of the church, each local kinship group cleaned and swept a strip of the square to symbolically convert it to sacred space (Silverman 1990; Urton, 1990). It is not a big leap from these plaza strips to the Nazca lines. (This apparently minor point about ritual sweeping is more important than it seems, as will be seen.)

South of Peru, we come to the altiplano of western Bolivia, where old straight Indian tracks also can be found — some of them longer than anything at Nazca. They seem to have been first brought to Western attention by French anthropologist Alfred Mâtraux in the early 1930s. He found shrines set out along pathways that were "absolutely straight, regardless of the irregularities in the ground".

Knowledge of these paths re-emerged in the 1960s during air photography for a new map of Bolivia. Tony Morrison and colleagues investigated some of the lines on the ground. They found that maintenance of the lines is in some cases dying out with the older inhabitants of the altiplano communities; consequently, many of the lines, which are formed merely by the clearing of bushes and stones, are growing back into the landscape. Some of the holy places on these old straight paths have been Christianized, and even some churches have been built on the lines, as at Sajama (Morrison 1978).

Other kinds of landscape lines of antiquity survive elsewhere in the world. In Britain, for example, there are earthen lines called cursuses. In 1723, antiquarian William Stukeley stumbled on what is now known as the Stonehenge Cursus, a linear earthwork about a mile-and-a-half long, linking two mounds just to the north of Stonehenge itself. He thought it was a Romano-British running track, and so he gave the Latin word "cursus" to it, meaning racecourse. But we now know it dates to around 3000 BC and its purpose is a mystery. About 50 further cursuses have since been found, mainly by air photography because these mystery earth lines are now mostly so eroded that they show up only as vegetation marks from the air. Cursuses typically consist of parallel ditches, usually linking prehistoric burial mounds or barrows, and can extend for several kilometers. Excavations within them have revealed very little.

Most cursuses are fairly straight features, like the one at Aston-upon-Trent in Derbyshire, which had one ditch aligned for a mile towards a (now eroded) mound (Gibson and Loveday 1989), and another at Scorton, in Yorkshire similarly aligned to a hilltop site (Topping 1982). A dead straight, two-mile-long segment of a crop-mark cursus was found to the west of Heathrow Airport, London, dwarfing the runways designed to handle Jumbo jets! Some cursuses, though, are straight only in sections — perhaps dating from different periods — while others have irregular linking sections.

Another alignment aspect to cursuses was first noted in 1947 during archaeological investigation of the Stonehenge Cursus: the line of the straight northern ditch could be extended almost a mile to pass through a standing stone remnant known as the Cuckoo Stone and a Neolithic ritual site named "Woodhenge" (Stone 1947).

I have studied about half of the known cursuses, and found that 64 per cent of them pointed to either a prehistoric site or an ancient church (presumably on an earlier sacred site) up to three miles beyond one of their ends (Pennick and Devereux 1989). In one case, the old church in the Suffolk village of Fornham All Saints actually stands on one length of the cursus there. Furthermore, another segment of the same cursus aligns to the major ancient site of Bury St Edmunds Abbey.

Another prehistoric linear enigma is that of the stone rows, found in various landscapes around the world. In Britain, they are particularly numerous on the wild, prehistoric landscape of Dartmoor. Like cursuses, no one understands what these rows were for. They occur in single and multiple lines, and invari-

ably pass through burial cysts. At their ends are larger stones set at right angles to the line of the row. These are called "blocking stones" by archaeologists.

To pluck another example of a landscape line at random, there is the alignment of temples on Java, Indonesia. There, the famous Borobudur shares an alignment with other temples that is still recognized in ceremonial terms.

There are many other kinds of such lines around the world.

Ley Lines

One cannot discuss landscape lines, especially alignments, for long, without mentioning the archaeological heresy of "leys" (or "leylines").

In 1921, Alfred Watkins noticed that prehistoric burial mounds, standing stones, hilltop earthworks and other ancient sites in his Welsh border homeland fell into straight, cross-country lines. He considered that these had developed along old traders' tracks laid down perhaps 4,000 years ago. The tracks were straight, he reasoned, because they had been laid out by line-of-sight (Watkins 1925). Watkins called his alignments "leys" (a Saxon term meaning "cleared strips of land") because he felt that the old straight waymarked tracks had originally passed through virgin forests, and strip-clearance had eventually occurred at points along them. He considered that the old tracks had gone through various changes in the Bronze and Iron Ages, gradually becoming forgotten in the early historical era. Watkins explained the fact that ancient churches recurred on these supposed prehistoric lines by saying they had evolved on earlier pagan sites.

Although mainstream archaeology at the time dismissed the whole idea of leys, neither Watkins nor his detractors seemed properly aware of cursuses or the American Indian lines, and their potential relevance to the whole issue.

The split between orthodox and fringe views about leys and landscape alignments started to widen shortly after Watkins' death in 1935. Occultists and dowsers began to fantasize about leys as "lines of power", and in the psychedelic Sixties, UFOs, dowsable energy lines, planetary grids and suchlike were added to the potpourri of notions surrounding the ley issue. This all got caught up with the expansion of the New Age movement in the 1970s, so that now there is a populist fantasy version of "leylines" as "energy lines".

There is, however, a much more sober, if much less known, research-based approach to the whole question of archaic landscape lines, and below is outlined the most recent developments to have occurred in that research-based understanding of mystery landscape lines.

Spirit Ways

In *Lines on the Landscape* (with Nigel Pennick, Hale, 1989) I suggested that we were dealing with archaic spirit lines rather than "energy lines". Shortly after the book was published in 1989, further material began to emerge to support that idea.

First, John Palmer, a British artist living in Holland, came up with remarkable data on medieval Dutch doodwegen or death roads — perfectly straight roads, some, near Hilversum, still visible, leading to cemeteries (Palmer 1989, 1990, 1991). These roads had a legally-determined width of six feet, and Palmer even traced a medieval oath relating to the straight carriage of corpses. This echoes an earlier, Viking practice of carrying a dead chieftain to rest in a ceremonial wagon along a straight sacred road, as has been uncovered at Rosaring, Sweden (Saward 1986).

Death and straightness seemed linked in earlier centuries, but why? Why "dead straight"? The connection seems to be with the spirits of the dead, for German researcher, Ulrich Magin, has recently found an old reference to Geisterwege, or "ghost paths" (Magin 1992). "These paths," says the German source, "always run in a straight line over mountains and valleys... In towns they pass the houses closely or go right through them. The paths end or originate at a cemetery." The spirits of the dead "thrive" on these paths, and "one meets with ghosts quite often" on them.

It now begins to look as if Watkins' "leys" could well have been alignment remnants of such medieval spirit tracks, and it seems archaeologists and the "energy ley" dowsers alike will have to reappraise their views.

These medieval deathghost paths may have developed from a core concept that had an earlier form in the Neolithic cursuses (above), linking, as they do, burial mounds. The Celtic idea of "fairy paths" running in straight lines from one prehistoric earthworked hill to another must also be closely related to this core concept. In Ireland it was considered bad luck to build one's house on a fairy path (Mac Manus 1959/1973), a startlingly similar idea to that enshrined in the old Chinese geomantic system of Feng shui, in which it is said that bad spirits travel in straight lines and one should not build in their way (Eitel 1873/1973; Lip 1979/1987).

The concept of lines for spirit use is also incorporated into the American Indian sweat lodge tradition, in which a straight earthen ridge is built to connect the firepit with the lodge entrance for spirits to use to enter there-in (Deer and Erdoes 1972/1980). Traditional Amerindian sand paintings also have spirit path depictions, which also occur in the Peyote and other ceremonial activities. These images almost certainly derive from a matrix of ideas that probably also informed the large-scale American Indian landscape lines, as the already-noted Andean Indian practice of ritually sweeping or cleaning a line suggests. Interestingly, a similar concept of spiritual sweeping seems to have prevailed in northern European folklore, which describes a special spirit flail used to sweep unwanted sprites from old pathways (Pennick 1989 [2]). The same idea is also expressed in association with British morris dance and folk gatherings, such as Yorkshire's "Plough Stots", in which a "Betty" (a man dressed as a woman) follows the sword dancers along an ancient country road, carrying a broom and miming a sweeping action.

Thread Lore

I have found that the strange, ancient association between spirits and straight lines could also extend to threads and cords. These also are "lines" — we talk of a "clothes line" for instance, or a "fishing line". So an Australian Aboriginal healer would fix the filament produced by a certain insect to the head of a sick person, and run it to a nearby bush where the patient's soul was ensnared (Dobkin de Rios 1990). The spirit would be coaxed down the insect's thread back into the person's body. Again, during a healing, Siberian Buryat shamans would lay an arrow next to a sick person, and run a red thread in a straight line out from the arrow-point to a tree outside the tent, so that the patient's spirit could be brought back along the "road" formed by the thread (Eliade 1951:1964). The Kalahari !Kung "climb threads" when their souls go out-of-body during trance dancing (Campbell 1988) and the Rigo people of Papua New Guinea leave a taut "fishing line" behind them when they go on out-of-body flights (McIntosh 1980). And so on — there are many examples.

Spirit Baffles

There is a kind of logic in the old traditions in that while straight lines supposedly facilitated spirit movement, curved or twisted ones hampered it. For example, Baltic fishermen would run around a stone labyrinth on the shoreline to entrap any trolls following them, before leaping aboard their boats. Chinese Feng shui practitioners use curved or blocked lines to deflect spirits. Following the same basic principle, Indonesian temples have low walls inside their gates to prevent straight-moving spirits from entering. It is probable that essentially the same idea was behind the blocking stones on the Dartmoor stone rows (above).

Threads also figure in this aspect of the story. For instance, in northern European lore, "spirit traps" were made by stretching red threads across a hoop, and placing it on a staff on the path the spirit was supposed to haunt (Pennick 1989 [2]). Also, some medieval witches wore knotted string shawls so their spirits could not be taken.

Spirit Flight and The Shaman's Journey

That ancient landscape lines had to be thought of as spirit ways was in itself a breakthrough, but what lies behind this ancient and obviously deep-rooted conceptual complex linking spirits (in general) and lines? The main clue is surely the cross-cultural nature of the various associated themes. This indicates that we are dealing with some universal factor to do with human consciousness — and the obvious place to look for that is in archaic shamanism, itself a universal expression of human consciousness.

The shaman, of course, was the person who used trance-inducing methods in order enter the spirit worlds on behalf of the tribe. This "shaman's journey" was what we would call today an "out-of-body" experience, which was often

envisaged as magical spirit flight. Could it be that the lines of shamanic spirit flight became translated onto the landscape as straight lines of various kinds in certain societies? Flight is, after all, the straight way over the land — "as the crow flies", we say. Another phrase with a similar meaning is "as straight as an arrow", and arrow symbolism was strongly associated with shamanic flight. The Koryak shaman of Siberia, for instance, was said to leave his body along a path traced by an arrow (Eliade 1951/1964), and we have already noted hints of this association, too, with the Buryat healing procedure of running threads from arrow-points. Did some ancient peoples symbolically mark the lines of spirit flight onto their ceremonial landscapes, as physical traces of something happening "in spirit"?

This may seem an odd idea to Western minds, but, then, we are not dealing with our kind of thinking. Yet even we are familiar with the concept of a "Sacred Way", and what is that if not some specialized, sanctified route?

There is a surviving tradition in a few isolated Australian Aboriginal tribes where annual Dream Journeys are undertaken along specified routes through the Outback (Mountford 1968; Cowan 1989). These were originally trodden by the mythic Dreamtime beings. Where these spirit beings had been become inherently holy.

The Dream Journey routes are conceptual lines in the landscape, much like fairy paths in Celtic tradition, that are passed on verbally and by use from one generation to the next. But when we look at the Chaco Canyon mesa country, the wastes of Dartmoor, or the Nazca pampa, we can see that in some cultures spirit lines were given concrete symbolic expression as physical markings, and were doubtlessly also eventually endowed with acculturated socio-religious meaning and functions.

Lines in the form of threads have also traditionally been associated with the out-of-body state, as noted above with the Rigo and the !Kung for examples. The Aborigines have various other beliefs about a thread issuing from the penis or the mouth linking body and spirit during miriru, the out-of-body state, and the karadji, or holy men followed "aerial ropes" during their trance journeys (Cowan 1989).

Southern African San (or Bushman) rock art repeatedly displays a curious figure known to archaeologists as a "flying buck". Recent research (Lewis-Williams and Dowson 1988), confirmed by the San themselves, and long suspected by the late Joseph Campbell (Campbell 1988), shows that this creature, with lines trailing out behind it, represents the out-of-body shaman who transforms into an antelope and then flies. This image is now sometimes referred to as a "trance buck" by archaeologists because of this.

As my efforts to synthesize all this material progressed, I came across a 1977 paper by American anthropologist, Marlene Dobkin de Rios (Dobkin de Rios 1977). I found she had already suggested a link between the out-of-body "aerial journey" of the shaman during trance with American landscape markings like the lines and also effigy mounds depicting animals, birds and winged

human beings, such as are found in Ohio, Wisconsin and elsewhere in the Americas. She realized these markings had been produced by shamanic peoples who were known to have taken hallucinogenic plants as part of their religious cults. Many of these native drugs give the specific sensation of "spirit flight" (La Barre 1975).

From the Heart of the World

That the mystery landscape lines, in the Americas at least, were related to spirit travel has recently been effectively confirmed by the Kogi Indians of northern Colombia. These isolated people have the best-preserved pre-Columbian society of any Native American people. They live in an isolated mountain fastness that contains most of the ecological niches to be found on Earth, and they see their territory as the "Heart of the World". They are ruled by shaman-priests called Mamas, "enlightened ones", who are able to see the spirit world—which they call *aluna*—interposed with the physical surroundings. The Kogi also have mysterious pathways that criss-cross their territory, linking ancient stone-built towns.

Alan Ereira has recently produced a TV documentary (Ereira 1990) and book (Ereira 1990 [2]) on the Kogi, and was, indeed, the first filmmaker allowed into the Kogi's territory. His film was shown on TV when I was in the midst of assembling the data being outlined here. Various things were shown and cryptic commentary used, that made me immediately contact Ereira and ask him if the Mamas had mentioned that their tracks related to their out-of-body travels. He confirmed that they had told him this, and referred me to a sequence in his film which showed tribal folk cleaning one of the paved pathways under the guidance of the Mamas. Ereira said that the pathway ran up from a river in a straight line and disappeared beneath an ancient sacred building in a Kogi town. The Mamas had carefully explained to him that the pathway was the physical trace of a spirit path which continued on in a straight line beyond the building, but in *aluna* only (Ereira pers. comm.). (Ereira admitted to me that it had required some adjustment on his part to see road cleaning as an aspect of the out-of-body experience! Obviously, this cleaning must relate to the traditional practice of symbolically creating sacred space by ritual sweeping, noted above.)

Ereira also photographed a Kogi standing stone covered with incised lines. He was told that the physical paths partly related to the stone's lines, which were, in effect, a map of spirit ways, the routes the Mamas took during their out-of-body shamanic journeys.

Bird Symbolism

The effigy mounds (above) depicting birds and winged humans were typical of Amerindian shamanic symbolism. The eagle feather, especially attached to a stick, was a sign of magical flight (Eliade 1951/1964). The Mississippian

people (c. 900–1500 AD) left many examples of pottery and shells decorated with human-bird figures classed by scholars as the "flying shaman" motif (Fagan 1991). A tablet of stone bearing the image of a man in bird costume was found in Monk's Mound, North America's tallest prehistoric earthwork, at the center of the Mississippian ceremonial complex of Cahokia (Fowler 1989).

The antiquity of this bird imagery in shamanism is shown in the Palaeolithic cave painting at Lascaux, France, of an apparently entranced man wearing a bird-mask. Near him is a bird-headed stick, and this was a documented symbol of shamanic trance in Siberia up until recent centuries (Eliade 1951/1964). In addition, the Siberian shaman might wear bird-claw shoes and a metal representation of a bird's skeleton on his ritual garb, similar to the way a Hopewell Indian shaman would hang bird claw shapes cut out of mica on his robes, as have been found in the Hopewell necropolis known as "Mound City", Chillicothe, Ohio, along with wooden effigies of hallucinogenic mushrooms.

Chinese Taoist priests were known as "feather scholars", denoting their shamanic origins (the idea of spirit lines in Feng shui probably came from archaic shamanism via Taoism), while at the other end of the Eurasian landmass, Celtic Druidism, too, was associated with the ability of magical flight. For example, the powerful Druid Mog Ruith is described as wearing an enchennach or "bird dress", and rising up "into the air and the heavens". Again, the father of the pagan Celtic Irish king, Conaire, was said to be a supernatural birdman (Ross 1970/1986). The Vedic Upanishads refer to the out-of-body spirit as "the lone wild gander" (in the Brihadaranyaka Upanishad: see Campbell 1988) and geese figure prominently in the symbolism of shamanic magical flight worldwide (Halifax 1982).

Modern Western Spirit Flight Symbolism

Surprisingly, even modern Western societies still retain acculturated images of shamanic spirit flight. It has been suggested that the image of Father Christmas flying in his reindeer-drawn sleigh derives from the shamanism of Arctic Europe and Siberia, where the tribes were reindeer herders. Their experience of spirit flight was aided by the use of the Fly Agaric (*Amanita Muscaria*) hallucinogenic mushroom which has a distinctive red and white cap, the color of Santa Claus' robes! (Taylor 1980.)

Another familiar Western image of spirit flight is the witch on her broomstick. This relates to the use of "flying ointments", prepared by medieval "wise women" (known before their Satanization by the Church as "Night Travellers" — *qveldriga*, "night rider" or *Myrkrida*, "rider in the dark") from herbs containing hallucinogenic alkaloids which specifically generate out-of-body sensations and, often, the feeling of body-image transformation into animal forms. Broomhandles were sometimes used to apply the ointments to vaginal tissues (Harner 1973).

From Shamanism to Monarchy, from Tribe to State

The American Indian peoples came over the Bering land bridge at least 12,000 years ago from Asia, the heartland of "classical" archaic shamanism. They therefore brought shamanic traditions with them which survived because the Americas were not subjected to the same intensive cultural changes as was Eurasia. Even so, marked Amerindian spirit flight lines became very complex socio-religious features, with all kinds of beliefs and dogmas attached to them, as has already been noted above, regarding the ceques around Cuzco.

This was even more the case in the Old World, where there was much greater social flux. Tribal societies developed into more complex proto-state and state cultures, and great religions developed across Eurasia, absorbing or marginalizing the earlier shamanic practices. Shamans became priests, and they in turn became theocracies or divine chieftains and kings. Throughout Eurasia, there are myths of "flying sovereigns". In his classic work on shamanism, Mircea Eliade wrote that "The 'magical flight' of sovereigns manifests the same autonomy and the same victory over death" as did the shamanic journey (Eliade 1951|1964).

Most of the Eurasian myths of flying sovereigns belong to the linguistic group we call Indo-European, and a proto-Indo-European word, *reg, seems to relate to a priest-chieftain function, and means "movement in a straight line" (Partridge 1958|1961). It has become the root of many European words to do with kingship and governmental, spatial, moral and figurative straightness. The English word "ruler" derives from it, for example, and means both a leader and a straightedge. In Sanskrit, *reg can also be translated as mana or supernatural power, and, possibly, "protector". So we have the image of a shaman-chieftain, a protector figure, with charisma or special power (Mallory 1989). In his *The Golden Bough* (1922), Sir James Frazer pictured the divine king from whom "lines of force radiate to all quarters". Such lines of kingly power were surely another, more sophisticated development of the idea of spirit lines.

Eventually, in state societies, the ecstatic, shamanic origins of the straight line-spirit connection became forgotten and the Straight Way became rote ceremonial ways, boundaries, royal routes, imperial avenues, and so on. A vague sense that the Straight Way was somehow sacred or represented power associated with rulership survived, however, as we see with features such as the avenues of Versailles, the Mall leading out from Buckingham Palace, and even the Masonic lines of roads radiating from the White House in Washington DC.

A Common Origin

We can study landscape lines cross-culturally because the so-called out-of-body experience is a universal product of the functioning of the human central nervous system in trance conditions, and thus transcends cultures.

The neurological aspects of trance conditions, especially of the "out-of-body" or metachoric (Green and McCreery 1975, 1989) state, actually explain the recurrence of the straightness element noteworthy in ceremonial landscape lines. Conceptually, this is, as suggested above, related to the idea of flight over the land in spirit form, as in the "shaman's journey", but its basis is actually neurological. Work by various neurophysiologists (Siegel and Jarvik et al 1975) has shown the occurrence of entoptic ("within vision") phenomena in the early stages of trance states. These visual phenomena are grouped into specific "form constants" which include grids, nets, dots, zig-zags, spirals, and other semi-geometrical forms. Such images have been found in ancient rock art in various places around the world, and are now being associated with shamanic practice and trance conditions (Lewis-Williams and Dowson 1988). Moreover, in at least one tribal society, that of the South American Tukano Indians, the deliberate manipulation of entoptic imagery has reached a sophisticated level and is used as a basis for its decorative art (Reichel-Dolmatoff 1978, 1978 [2]).

The precise mechanisms producing the entoptic imagery are not yet fully understood, though it is thought that several factors may be involved, such as phosphene effects within the retina, and characteristic neuronal firing within the visual cortex. As trance deepens, these semi-geometric entoptic images—which appear as if in front of the eyes, and sometimes as if projected onto surfaces in the actual environment — become "construal". In other words, the geometric patterning becomes the matrix for the incorporation of memory-based imagery, much as the dot screening of a newspaper photograph "carries" the image. The billowing entoptic patterns trigger associations within memory function, and they "take on" representational form. Curious hybrid forms, perhaps half-human, half-animal, can appear, and may account for therianthrope imagery present in rock art and in ritual costumes (especially in the totemic ambience of a tribal society). Ultimately, the experient's locus of consciousness is absorbed into the interplay of the flickering, construal hallucinatory material, and participation occurs.

A key entoptic is the "tunnel" ("alley", "cone") form constant. This seems to herald the shift of the early trance state into the deeper condition of participation. This entoptic is, if I may so term it, the fundamental blueprint of the "straight way". It reportedly occurs frequently in Near Death conditions, in which the experient finds him or herself seemingly floating down a dark tunnel towards a light (Moody, 1975). On emergence at the "far end" of the tunnel, the person often sees a paradisaic "summerland", and perhaps encounters a radiant being of great sacral power. This Near-Death "tunnel" is strongly reminiscent of the UFO "traction beam", often mentioned in so-called "UFO abductions". It is now thought that these "abduction" experiences may be the spontaneous (and, I feel, the possibly pathological) modern version of the "shaman's journey" (Devereux, 1989; Ring, 1992), occurring in a secular society that has no shamanic or spirit tradition, but rather is imbued with the impersonal ("alien") and depersonalizing motif of the machine ("UFO").

The suggestion being offered here is that the directness inherent in ancient landscape lines is underpinned by this entoptic straightness, the "tunnel" of the "exteriorizing" spirit. Conceptually, the soul, like the crow or arrow, flies straight. The terrestrial linear markings are spirit lines, an environmental expression of the trance tunnel effect.

Modern opinion is divided as to whether this out-of-body experience involves an actual shift in the locus of consciousness, or is "merely" hallucinatory (see the differing viewpoints of Blackmore 1982/1983 and Kalweit 1984/1988, for instance), but the effect is the same as far as the experient is concerned. Ancient peoples clearly believed in the reality of spirit flight, and that belief has left its imprint as straight line and effigy markings on what can only be called shamanic landscapes. These lines varied from culture to culture and age to age in their form and meaning, but their underlying source was the common canvas of the human mind in metachoric trance conditions. These particular mind states have clearly left a deep imprint in many earlier societies; if our own culture was to reacquaint itself with these realms of consciousness in an open, scientific manner, the potential would exist for the emergence of fresh understandings about consciousness and its relationship to the environment that could ultimately affect our whole worldview.

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The entire range of material outlined in this paper is dealt with in depth, and in slightly differing contexts, in two new works by Paul Devereux: *Symbolic Landscapes*, Gothic Image, July 1992, and *Shamanism and the Mystery Lines*, Quantum (UK), September 1992, and Llewellyn (USA), April, 1993. Paul Devereux can be contacted via: P O Box 92, Penzance, Cornwall TR18 2XL, UK.

Mainstream Sciences vs. Parasciences: Toward an Old Dualism?

GERALD L. EBERLEIN

Sociology Dept., Technical University, Munich, Germany

Abstract— The Observatory for New Spiritual Movements has been operating at the Technical University of Munich since 1980. It is concerned with the study of cults, New Age ideologies and with the parasciences from anthropology to cryptozoology. The parasciences are being analyzed by the science of sciences, i.e., by the philosophy of science, psychology and sociology of science, as well as by the history of science.

The following ten hypotheses, characteristics and questions are discussed:

- (1) Mainstream sciences may make neither monopolistic nor absolutistic claims.
- (2) The six main criteria of the academic sciences are compared to the six main criteria of the parasciences.
- (3) The two types of science also differ in their value orientations.
- (4) Social processes and structures are characteristics of both.
- (5) What are the characteristic motivations of parascientists?
- (6) What is the historical background of the mainstream sciences?
- (7) What are the difference between the metaparadigms of mainstream science and those of the parasciences?
- (8) What are the social functions of mainstream sciences and parasciences?
- (9) Do the functions of the parasciences transcend those of mainstream science?
- (10) What are the characteristic differences between practitioners of mainstream science and the parasciences?

Introduction

There are good reasons for the choice of this topic in the setting of our conference subject "the challenge of anomalistic observation". First of all, there is the fact that science is the particular field for academic researchers being its own object of critical investigation.

This is taking place in the context of the new disciplines "philosophy and sociology of science" which will be discussed later in more detail. The discussion about the so-called parasciences has been carried on for some years by our "Observatory for New Spiritual Movements", founded in 1980. This observation post in the Institute for Social Sciences of the Technical University Munich that has been able to operate independently of commissions, especially of

ideological institutions, was primarily concerned with "cults", on which there were at that time hardly any German-language publications.' For this purpose, original documents and publications on the new religious movements were gathered and analyzed; this was also done for students preparing to teach at vocational schools, in order to give them material for use in class discussions.

Toward the end of the 1980's the fashionable "New Age" phenomenon caught the attention of the public and of researchers. All the media reported on it, some of them up to the present day; a variety of products and publications offering supposed or actual "Heilswissen" (salvation knowledge) were available commercially, and were consumed by an eager and heterogeneous public. Lectures and newspaper articles resulted from the observatory's attention to "cults", and the sociological study of "New Age", with a two-year research project at the Institute for Social Sciences leading to the first German empirical research report which enjoyed and still is enjoying a surprisingly widely circulated response.'

Also since the end of the 1980's, the "Observatory for New Spiritual Movements" has been more intensely occupied with parasciences, resulting in, among other things, the first German-language reader with a comparison of mainstream and parasciences.³ In recent years a veritable inflation of parasciences could be observed in Europe, of which a selection can be named in comparison with mainstream sciences.

Following anthropology, Germans having a higher education have again become interested in anthroposophy, "gnostic anthropology" and other spiritual concepts of man; instead of archeology and prehistory, millions of Germans are reading books on atlantology, bermudology, "paleovisitory" and E. Daniken's "pre-astronautics".⁴ Both the last-named disciplines maintain that extraterrestrial beings have made space voyages and visited our planet in pre-historic times, and we pursue their traces in "magic history". In Hessen at least, there are state-certified astrologers; if the experts may be believed, there are more practising astrologists than clerics in Germany.

While the reader may have only an amused smile for ethnological investigations of cargo cults in the South Seas that are supposed, by way of symbolically laid out airports, to force down the white man's cornucopia, analogous to his military and freight airports, over the impoverished islands — the above-mentioned observatory noticed that aromas, gems and many other objects, as well as spells, supposedly have extraphysical effects on members of our highly modernized society. While the discoveries and research results of geology and geography have led only to an excellent popular science magazine, conferences and publications on geomancy (so-called "magic geography") have an eager audience.' Magic powers and lines of lost cults and cultures are supposedly rediscovered in crop circles, dolmens, menhirs and sacred monuments throughout the world. At our Technical University, institutes for air and space travel are doing research and teaching; scientists and engineers from widely varying fields are working on ufology with so-called lay researchers.⁶ Nu-

merology, i.e. interpretation of destiny based on secret numbers, is studied in place of mathematics.

Conventional medicine—for the most part allopathic—is confronted by unconventional procedures such as acupuncture, spiritual healing, homeopathy, iris-diagnosis etc., which are by no means employed only by “paramedics”.⁷ Philosophy is being overrun by esoteric metaphysical approaches and mystical systems, while increasing numbers of physicists have for decades been attending parapsychological-parapsychological conventions and recently have been more deeply interested in dowsing.⁸

There is a more or less credulous audience for prophecy as opposed to prognosis.⁹ Anti- or parapsychiatry (e.g. the Scientology Church's psychotechniques, the goal of which is to become “clear”, or Catholic exorcism) is struggling against mainstream psychiatry. And parapsychology, along with older mainstream sciences such as graphology (now known as “psychology of handwriting”), is attracting not only people interested in psychology, but also increasingly those spiritualistically inclined.” Recent polls of teachers and counseling services in Germany show an unsettling, rapidly growing interest in experimentation in occult practices on the part of school children.”

In Europe, parasociology (H. Niezing), i.e. explicitly ideologically defined social sciences, is encroaching more and more on sociology. In Germany, there are not only university chairs for Marxist, but also for Christian social science and philosophy of life. Also, there is increasing interest in sociobiology, the theory of “egoistic genes”, the latest fad in social science. Along with publications on statics, there has been an increasing number of “harmonical” publications, e.g. pyramidology, that attempt to prove universal harmonics by way of sacred numbers. Finally, there is cryptozoology, which for thousands of interested readers means “Nessie”, “Yeti” or “Bigfoot” research.

It may be considered as scientifically proven mainstream knowledge that “Bildungsaberglaube” (W. Hellpach) (superstition of intellectuals)¹² such as alchemy, astrology¹³, graphology, phrenology (i.e. 19th century character-reading based on the shape of the skull) and other parasciences have definitely foundered, since they have no empirical foundation. As the above-mentioned list shows, however, geomancy, psychoanalysis (dogmatic Freudianism is meant here), sociobiology and ufology have taken their place alongside methodologically disproven disciplines such as astrology and graphology.

This comparison inevitably brings up the question: What is mainstream science, and in what way does it differ from the parasciences? First of all, I would like to differentiate between the idea of critical science and scientific ideology(-ies). Tolerant interpretation of academic disciplines will view this as one approach to knowledge; in contrast, scientific ideology regards science as the sole valid approach. It is perfectly clear to the critical scientist that scientific cognition is value-conscious to the extent that the independent researcher can and wants to define his personal values.¹⁴ In contrast, scientific science insists on value-free cognition or even believes it can proceed without premises.

A glance at their objectives also shows their diversity. The purpose of critical scientific cognition is always enlightenment regarding man and the world, whereas scientism, unthinkingly naive, mostly considers itself as the instrument of mastery over nature, the world and man. In a word: critical science is "only" objective-scientific science on the other hand claims for itself absolute validity and certainty.

My reflections follow from the methodical perspective of a science that is still young: namely interdisciplinary science of science.¹⁵ It embraces descriptive philosophy of science, i.e. the description of the foundations and prerequisite values, methods and theory formation in university and parasciences, and empirical sociology of science, i.e. the investigation of the social processes and structures of scientific communities, as well as psychology of science, which is concerned with the creative and discovery processes of researchers in their everyday work. These are all synchronic, thus cross-sectional viewpoints; but, in addition, there is the history of science as the diachronic dimension, which attempts to cover the change in theoretical as well as empirical research, whereby the researcher can ascertain only afterward what was "correct" research and teaching, once these have crystallized and achieved recognition.

Interdisciplinary philosophy and sociology of science is obviously a mainstream science, but can make no claim to "orthodoxy". It can thus never be completely sure of itself, it must continually change its subject, research techniques and viewpoints, and, in contrast to the philosophy and sociology of science and the history of science, is not in a position to make evaluations in the sense of "true" and "false" science. Its investigation of the parasciences is thus strictly descriptive, regarding them as "deviant" disciplines, but not pejoratively as "pseudosciences", which the normative philosopher of science can and indeed must.

The scientific researcher, on the other hand, may not assume that there is or may be only one concept, one model, one paradigm of science. Since Kuhn, this is understood to be the model of a scientific community and comprises model concepts, approaches, theories and solutions.¹⁶ Every discipline contains competing paradigms, current as well as historical ones. The disciples of old paradigms do not as a rule "convert" to new ones, but "die out". Strictly speaking, every discipline has dominant paradigm(s). However, as a generalization, the empiric-analytical paradigms of the sciences is meant here. Normally, it takes up no position regarding the preliminary metaphysical decision "What is reality?" or "How real is reality?"¹⁷

Discussion

Monopolistic and Absolutistic Claims

In spite of its universalistic character, mainstream science may lay claim neither to a monopoly nor to absolutism: It may consider itself neither the sole cognitive approach, nor assert the absolute truth of its insights. For the para-

sciences, on the other hand, a sense of conviction and involvement is a prerequisite; they assume the absolute certainty of their subject's existence, employ their own procedures and research approaches (horoscopes, swinging a pendulum, seances); they construct ad hoc explanations from case to case. An example is parapsychology, which attempts to explain supposed or actual extrasensory phenomena in a philosophically inadmissible way with a putative "Psi (ESP) function". In this way, the parasciences consciously deviate from mainstream sciences regarding subject, methodology and explanation to a greater or lesser degree.

Research Criteria

Mainstream sciences are based on other research criteria than are the parasciences. For the mainstream sciences these are: analysis of the whole based on a summing of parts; causalism, i.e. an ideally "hard", deterministic, or at least statistical connection between cause and effect; naturalism, admission solely of inherent explanations; scientism, scientific knowledge as an end in itself for basic research; methodism, namely the objectifying logic of science as a unique value; and technicalism, the application of highly differentiated research techniques as a basic prerequisite.

Therefore, the mainstream concept of man is, in a way, constructed elementaristically; it inquires into causal connections, allows only naturalistic, i.e. empirical explanations, whereby scientific knowledge appears to be an end in itself.

In this concept of man, objectivizing methodology and social research techniques have an essential unique value as prerequisites for objective knowledge. Its goal is the "transparent man".

On the other hand, the parasciences present the following research criteria: holism, i.e. holistic context; analogics, i.e. resemblances in astrology, therefore the principle "wie oben, so unten" ("as above, so below"); esoterism as the quintessence of occult science and knowledge; metaphysical-mystical thought, especially in the speculative form; subjectivism, namely empathy and personal interpretation; finally, crypturgy, which is understood to be the workings of occult powers.¹⁸ The parascientists refer to mainstream scientists, who reject these criteria, as "rigid dogmatists".

The aim of the parascientific concept of man is the holistic essence of man, as yet not only unknown, but perceptible solely by way of empirical science. Only by analogical thinking and esoteric occult science, typically by speculative or even partly irrational thought, through interpretation and empathy, a concept of man is outlined, that is characterized by the asserted working of occult powers. The para-disciple is an "adept", who is "initiated" in occult knowledge.

Value Orientation

Parascientists do not differentiate between scientific and extra-scientific valuation. Mainstream scientists are able to objectify attributed values through

analysis of their prerequisites. Parascientists cannot and do not wish to do this. An example: Air- or space-travel researchers would only attempt to study physical characteristics and possible mechanisms of UFOs; in contrast, most Ufologists are primarily interested in the source of these puzzling, unidentified objects; they attempt to surmise about their supposed occupants, or even to declare with conviction that the "ufones" have a particular message for us, that they come as rescuers, even saviors of humanity and want to keep us from catastrophes of war or of the ecosphere. The supposed ufones are viewed not only as expert space travellers, but also as masters and agents of extraterrestrial initiation; they, and meetings with them, are to an extent advertised. The esoteric teacher thus is not only a competent specialist, but also master of a secret discipline. He is superior to others because he has at his disposal particular, profound insights and wisdom, and embodies them in his personal conduct as well.

Both paradigms, however, also share common values: maintaining and elaborating upon traditions in research; gaining new insights, i.e. achieving progress in research; and thus, ultimately, affecting a change of consciousness of researchers and their society.

Social Processes and Structures

The social processes and structures of the parasciences are comparable to those of the mainstream sciences: Both feature scientific communities, paradigms, congresses, publishing, etc. But there are important differences. In contrast to academic scientists, parascientists are mainly lay-researchers. While the former regard themselves as mere "communities of research", the latter strive to be "communities of creed".

Motivations

An important assumption of the philosophy and sociology of science regards the motivation of parascientists as analogous to that of adepts of the so-called "new cults": They are characterized by a desire to compensate for frustration, by escapism, and by opposition to scientific rationalization of the world. Consequently, parascientists would have to be considered as maintaining, rather than innovating, insights. This is consistent with their role as guardians of occult wisdom. According to psychologists, the existence of a so-called "believer in the occult" has been empirically demonstrated. Esoteric beliefs and modes of behavior can be deduced about this personality: it is comparatively unstable and experiences the world as hostile. Man is seen as being at the mercy of a supra-natural order, ill-fate being its manifestation. To be informed about its intentions, one resorts to oracles and divinations as means for predicting the future. Above the world of reality, one seeks refuge in a spiritual domain. Within its framework, being an occult ideology, all secrets become known. The inter-personal relationships are marked by a conflict between the need for closeness and the increasing fear of rejection, furthering isolation as well as tendencies toward withdrawing.¹⁹

Historical Background

There are noteworthy differences in the development of mainstream and parasciences. Whereas institutionalized disciplines were differentiated into specialized fields by the 20th century to which the humanities and empirical sciences had evolved from philosophy, the parasciences originate in the acceptance and superficial rationalization of systems of creeds and convictions, i.e. ideologies. Hermetic sciences, mythical and mystical mentality, the belief in salvation, etc., are parts of these ideologies. Accordingly, the parasciences are based on residues of previous knowledge. Anthroposophy can be traced back to theosophy; parapsychology to the occultism and spiritism of the 19th century; numerology to the system of magic numbers, which evolved from the Pythagorean philosophy of the secret qualities of numbers as building stones of the cosmos.

Metaparadigms

In mainstream sciences, metaphysical assumptions appear in their philosophical foundation, their meta-paradigm, only.²⁰ Idealistic, realistic, monistic, and dualistic preconceptions are of importance only on the lowest level of assumed presuppositions. In contrast, in the parasciences, metaphysical, axiomatic preconceptions enter the formation of hypotheses and theories.

For example, in the mainstream sciences the researcher's "personal metaphysics" play no decisive part in his work. The scientist's assumptions on the essence of matter, phenomena, man, cosmos, mind, etc. do not significantly influence the formation of theories and methods of research. Parascientists, however, share the conviction that secret spiritual powers underlie the phenomena; that the world is composed of mind and matter; or even, that it is animate (so-called pan-psyche idealism), culminating in the belief that a spiritual entity or spirit realm is immanent in, or guiding, the cosmos (i.e. objective idealism or spiritualism).

The parascientist does not regard his assumptions as presupposed systems of value, exchangeable and open to objective research; rather, they emerge in his hypotheses and theories and he seeks to directly prove them. From the point of view of sciences, this is impossible.

Social Functions of Mainstream Science

Mainstream and parasciences accomplish different cognitive and social objectives. Mainstream theories define concepts, inform about facts, recapitulate empirical regularities, explain and predict phenomena and laws. They convey enlightenment, thus shaping society and the world. Finally, they result in a rational view of the world and man, devoid of contradictions.

Social Functions of Parasciences

To a certain extent, parasciences operate in a similar fashion; in addition, however, their functions reach further. They offer a frame of reference for ori-

entation and understanding, and, finally, attempt to substitute for science, philosophy, and religion. Thereby, they compensate for social deficits, which may be actual or imagined effects of mainstream science. They provide a sense of "enclaves of meaning" (K. Weis) in the shape of alternative ideologies. Thus, the astrological, homeopathical, occult, "UFO-logical" view of man and redeeming wisdom are diametrically opposed to the scientific, positivistic, elitist point of view.

Practitioners

The production and consumption of mainstream sciences are largely determined by an elite of researchers. The parascientists are mainly supported by lower-echelon academic researchers and by lay-researchers outside of the scientific community. The parasciences address, and refer to, deviants: less educated, adhering to a magical-mythical consciousness, ideologists; finally, followers of anti-mainstream movements, which are critical of society and/or its culture (e.g. the Green Parties, anthroposophy, New Age movements).

Recapitulation

The mainstream sciences remain productive and creative by means of continuous and fundamental innovation; including, nowadays, the acceptance of "anomalistics", i.e. the dispassionate and unprejudiced research of the stumbling-blocks in the course of mainstream sciences.²¹

Parasciences, however, often being residues of previous knowledge, are now deviant, sometimes competing, thus alternative modes of knowledge. Despite deviating and being denounced as "pseudo-science", they complement the system of learning and knowledge of modernized societies.²² Based on ancient mentalities and narrative structures, such as epic tales, myths, revelations etc., parascientific propositions often cannot be proven wrong or right, scientifically. Rather, they are experienced as operating, and are believed in; thus, held as personal truths. This applies especially to the so-called methods of divination, e.g. astrology, I Ging, pendulum, Tarot etc.

Notes

1. One of many examples would be: G. Eberlein: "Angst vor der Konkurrenz? Die Jugendreligionen in der Kritik der Kirchen", in: *Evang. Kommentare*, 15. vol., 1982/4, pp. 187-190.
2. M. Schneider: *New Age: Empirische Studien zur New Age-Bewegung — Glaubensspielräume*, München u.d. (1991).
3. G. Eberlein, ed.: *Schulwissenschaft, Parawissenschaft, Pseudowissenschaft*, Stuttgart 1991.
4. Cf. Eberlein loc.cit., "Einleitung", esp. p. 7, note 5.

5. Cf. recently J.M. Moller: *Geomantie* in Mitteleuropa. *Kraftlinien* und *Energiezentren* in Süddeutschland, Freiburg 1988; see also for further references,
6. Eberlein loc.cit., note 6.
7. Eberlein loc.cit., pp. 11 ff. as well as section II.: Homöopathie.
8. As to the convergence of parapsychology and -physics see W.v. Lucadou: *Psyche und Chaos. Neue Ergebnisse der Psychokineseforschung*, Freiburg 1990; for dowsing see Eberlein loc.cit., pp. 10 f. and section I.
9. The only really compulsory monography of the last years was written by A. Gann: *Zukunft des Abendlandes? Eine Untersuchung von Prophezeiungen*, Salzburg 1986 (private publication by the author, Institute of Applied Parapsychology).
10. Eberlein loc.cit., pp. 13 ff. and section III.
11. See J. Mischo: "Okkultpraktiken bei Jugendlichen", in: *Zs. f. Parapsychologie u. Grenzgebiete d. Psychologie*, vol. 30 (1988/1-4), pp. 5-31.
12. To my knowledge, the term comes from one of the founders of German cultural psychology in the 20th century, W. Hellpach: *Grundriss der Religionspsychologie*, Stuttgart 1951, p. 96.
13. Cf. the important positive critique of astrology by H.J. Eysenck, D. Nias: *Astrology: Science or Superstition?* London 1982; refuting this, the most recent methodically based probation control of astrology comes from the astrologist, psychologist and psychotherapist P. Niehenke: *Kritische Astrologie. Zur erkenntnistheoretischen und empirisch-psychologischen Prüfung ihres Anspruchs*, Freiburg 1987. Referring to this, see Eberlein loc.cit., pp. 17 ff. and section IV.
14. For a philosophical-sociological analysis, see G. Eberlein: *Maximierung der Erkenntnisse ohne sozialen Sinn? Für eine wertbewußte Wissenschaft*, Zürich/Osnabrück 1987; for an English-language, easily available contribution, G. Eberlein, O.P. Obermeier: "Value-Free vs. Value-Conscious Social Sciences", in: *Theory and Decision: Essays in Honor of Werner Leinfellner*, G. Eberlein, H. Berghel, eds., Dordrecht 1988, pp. 107-136.
15. In my opinion, the still best interdisciplinary account of science of science is: *Science, Technology and Society: A Cross-Disciplinary Perspective*, I. Spiegel-Rosing, D. de Solla Price, eds., London/Beverly Hills 1977.
16. Term and first account of paradigms of natural sciences comes from T.S. Kuhn: *The Structure of Scientific Revolutions*, Chicago 1962. Since 1979 more than 20 meanings of "paradigms" have come into use (cf. *Criticism and the Growth of Knowledge*, I. Lakatos, A. Musgrave, eds., Cambridge 1970, esp. M. Masterman: "The Nature of a Paradigm", pp. 59-89), today the term "disciplinary matrix" is used.
17. For a generally comprehensible introduction into the problems of reality, based on social sciences rather than metaphysics, see the one by the communication researcher P. Watzlawick: *How Real is Real? Confusion, Disinformation, Communication*, Toronto et al. 1977.

18. For a comparison between mainstream vs. parasciences cf. G. Eberlein: "Wie wissenschaftlich sind Parawissenschaften?" in: *Epistemology and Philosophy of Science, Proc. of the 7th Int. Wittgenstein Symposium 22nd-29th August 1982, Kirchberg/W., Wien 1983*, pp. 288-292.
19. See J. Mischo: "Okkultismus und Seelsorge", in: *New Age—aus christlicher Sicht*, O. Bischofsberger et al., eds., Freiburg—CH/Zürich, 2. ed. 1987, pp. 127-165, esp. pp. 140 ff.
20. For the cognitive metaparadigm cf. note 17, furthermore G. Eberlein: "Wissenschaftstheorie oder Wissenschaftsforschung? Wider eine Dogmatisierung wissenschaftlichen Handelns", in: *Soziale Welt*, vol. 27 (1976/4), pp. 488-503. The term metaparadigm, though, may be used socioculturally as well—cf. Cc. Tart: *Transpersonal Psychologies*, New York 1975, 2. chapter.
21. "Anomalistics", used in German language often as "Paranormology" (A. Resch), explores alleged or real anomalies in natural and social sciences by means of exclusively discipline-related scientific methods. Moving force is the international "Society for Scientific Exploration", founded in 1982, with its *Journal of Scientific Exploration*.
22. The difference between mainstream and parasciences naturally is blurred when private universities are founded that are partially or exclusively devoted to an esoteric-parascientific education. This applies to the Maharishi International University as well as to the recently founded "Cosmology-University" in Switzerland (situated at Vaumarcus/NE). The latter promises not only a "Cosmologist with diploma (KOS. Acad.)", but also the prospect of promotion including doctor's degree and professor.

Existence of Life and Homeostasis in Atmospheric Environment

SHIGERU MORIYAMA

*Physical Science Laboratories, College of Industrial Technology, Nihon University,
2-11-1 Shin'ei, Narashino-shi, Chiba-ken 27.5, Japan*

Abstract— A geophysiological model is used to show how a regulation of the atmospheric CO₂ level could counteract the effect of a gradual increase in solar luminosity. In our model, the biosphere and the atmosphere-ocean system exchange carbon through a biological process which includes the internal and mutual antagonism. It is suggested that as soon as the biologically regulated system had appeared on the early earth, the regulatory aspect of the ecosystem would have been fully operational, and thus, that the earth's environment has been maintained in homeostasis for a long time. One model for the temporal variation of the carbon distribution on the earth, which is in agreement with observed carbon isotopic data, is also suggested. An important result is that our ecosystem left a completely biologically controllable state some six hundred million years ago, and that the current trend is toward destruction of the ecosystem on the earth.

Introduction

Recent carbon isotopic data suggest that, contrary to the generally accepted opinion, our planet's environment may have been strongly controlled by life from a very early stage (Schidlowski, 1988; Holser et al., 1988). Geophysiology is one of the most effective approaches to the question of biological control of the environment. However, the system composed of the biota and the environment is vastly complex; consequently it is very difficult to verify geophysiological evidence. Despite this difficulty, some simplified approaches have recently been applied to this problem (see, for instance, Lovelock and Margulis, 1974; Lovelock, 1986). The Daisyworld model by Watson and Lovelock (1983) is a new approach based on geophysiology. They studied the homeostasis that resulted from the coupling process between albedo due to the color change of Daisy World and the temperature of this world.

It is widely accepted that although solar luminosity has been increasing from early in the earth's history, the earth's average temperature has remained constant and favorable for life. Owen et al.(1979) proposed that the amount of CO₂ was great enough to warm the primitive earth's surface so as to maintain its condition in homeostasis. However, we do not know through what process, or by what mechanism, it has been regulated.

The main objective of this work is to study whether it is simply a coincidence that as solar luminosity has increased, the amount of atmospheric CO₂

was reduced at such a rate as to maintain a quasi-constant temperature. Moreover, we are interested in why the mean surface temperature of the earth is on the cool side of the optimum for the biota, and why the concentration of CO₂ in the earth's atmosphere is anomalously low. In this paper, we argue that a simplified geophysiological model can explain a regulation of the atmospheric CO₂ level.

Walker et al. (1981) proposed that the long-term stabilization of the earth's surface temperature was brought about through a nonbiological mechanism (hereafter called the Walker cycle). This cycle includes a strong negative feedback mechanism in which the rate of weathering of silicate minerals, followed by deposition of carbonate minerals and hence removal of CO, from the atmosphere-ocean reservoir, depends on surface temperature, which in turn, through the greenhouse effect, depends on the partial pressure of CO₂.

However, silicate weathering may be enhanced by biological activity and current weathering of silicate rocks may be biologically controlled (Lovelock and Whitfield, 1982; Schwartzman and Volk, 1989). Lovelock and Whitfield argue that geological input and removal of CO₂ constitute only a few tenths of a percent of the biologically driven fluxes, and that the chemical weathering of rocks is controlled by the biota which maintain the high partial pressure of CO, in the soil through oxidation of organic detritus. The nonbiotic Walker cycle predicts an atmospheric CO₂ concentration 100 to 1000 times higher than the observed levels. Without biota that significantly enhance weathering rates, the earth would be over 30°C warmer than it is today (Schwartzman and Volk, 1989).

Moreover, the nonbiological model has a weak point: it does not explain that despite the gradual increase in solar luminosity, there has been homeostasis in the earth's environment. External disturbances greatly influence its solution. According to the abiotic model, unexpected but common events in the earth's history (such as rapid and vast release of CO₂ by volcanoes, by metamorphism and by a reduction of the land area exposed to weathering) would bring about a new, warmer steady state discontinuous from the previous climate.

To circumvent this weakness and keep constant condition on the earth, the modified Walker cycle model (e.g., Matsui and Tajika, 1989) proposes the existence of a continent as a stable reservoir for huge amounts of CO₂. However, this modified Walker cycle model cannot be valid because of the scarcity and small sizes of continents in the Archaean. Continents seem to have appeared on the earth barely 2.5-3 Gyr ago (e.g., Veizer, 1988). Using the hydrothermal interaction model, Walker (1985) showed that in the early stages of the earth's history, before the emergence of a continent, the atmospheric CO, concentration would reach 1 MPa. The modified Walker cycle model including silicate weathering inevitably comes to the conclusion that our planet would have possessed an atmosphere with a CO₂ partial pressure of above a few hundred kPa during the first half of its history due to the scarcity of continents (Matsui and Tajika, 1989). There is no evidence that supports the existence of an atmos-

phere of this composition. The Walker cycle is more likely to apply to the Mesozoic and Cenozoic than the Archean (Berner et al., 1983; Volk, 1987).

As will be discussed later, the isotopic data ($6^{13}\text{C}/\delta^{12}\text{C}$) imply that the ancient earth was not abiotic but, rather, has been in a state of global biotic saturation since almost 4 Gyr ago. Carbon isotope records in sedimentary rocks indicate that marine microbial photoautotrophs may have been fully operative for almost 4 Gyr, and, strangely enough, that organic carbon would have always made up about one-fifth of the total carbon in the surficial compartment (Schidlowski, 1988). This reveals that our environment has been under the influence of life since its early stage.

We investigate this regulation of the atmospheric CO_2 level, which appears to be linked to the gradual increase of the solar luminosity, and propose a model quite different from the Walker cycle. Our results indicate that when the biosphere and the atmosphere-ocean system exchange carbon through a biological process which includes the internal and mutual antagonism, CO_2 level can be regulated by the biological control so as to be habitable for life. Recent isotopic data, studies on the sources of inorganic carbon and the impact on the carbon cycle of photosynthesis and calcification of algae are discussed. We also present new ideas on fluxes of carbon throughout the earth's history.

The Model

Our model, which consists of three boxes, is presented in Fig.1. Carbon moves between the biosphere and the atmosphere-ocean system through photosynthesis and the formation of organisms (this rate is represented by C in the following equations), and through respiration and decay (the rate is expressed by E). Carbon moves from the biosphere to the lithosphere through burial (D). The atmosphere-ocean system exchanges carbon with the lithosphere by way of volcanism and weathering (G). It is assumed that, within the time step used in our calculation, the dissolution equilibrium of CO_2 is instantaneous between rocks and the ocean, and also between the ocean and the atmosphere. We also assume that the biological activity k is a parabolic function of the globally averaged surface temperature T as shown in eq.(1). T is estimated, as in eq.(2), to include greenhouse effects due to CO_2 and H_2O . Incident solar flux S , which is expressed as the ratio to the current value, has a direct relationship to the effective temperature T_e as in eq.(3).

$$k = C \{ 1 - a(T_1 - T)^2 \} \quad (1)$$

$$T = 285 + 2(T_e - T_{e,0}) + 4.6(p/p_0)^{0.364} - 4.6 \quad (2)$$

$$T_e = S^{1/4} T_{e,0} \quad (3)$$

In the above equations, p is the partial pressure of CO_2 in the atmosphere, a and T_1 are constants, and suffix 0 stands for the conditions at present. Eq.(1), in which biological activity k shows a maximum versus temperature, is a universal property of living things.

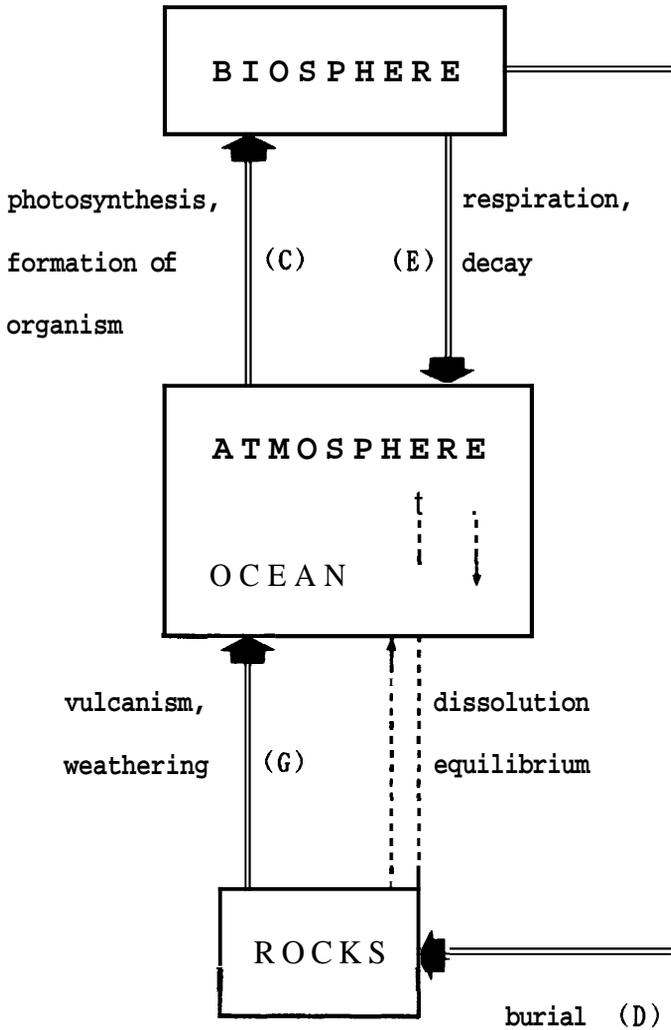


Fig.1. Model diagram of the bio-related system.

The biological processes governing the prosperity and decline of the biosphere can be expressed using an equation which includes what is called the internal and mutual antagonism (Nicolis and Prigogine, 1977). This antagonism expresses the interdependence of resources and consumers, and reflects the intrinsic property of life to proliferate exponentially to limits ultimately set by the availability of critical resources. That is, it reflects the fact that biota blooms to limits determined by resources. Because the total amount of carbon

on the earth is limited and, as a consequence, the amount of carbon available to biota is also limited, development of a biosphere is thought to be under a strong control of the internal and mutual antagonism.

Carbon can be regarded as a resource offered to the biosphere for its development, because an increase in biota is contingent on a consumption of carbon stored in the atmosphere-ocean system. In other words, the activity of biota is closely related to the scale of photosynthesis. On the other hand, death of biota implies an increase in the amount of carbon available to the other two spheres. These processes, which exemplify the internal and mutual antagonism, are shown in the first term on the right-hand side of eq.(4) or (5). Here, the resource and the consumers correspond to carbon and biota, respectively.

Biological activity is determined by many factors. For example, phosphorus input from the continents may be a determinant of the maximum size of the earth's biomass in the present ocean. However, the scarcity and small size of continental areas during the early earth's stage suggest that this common idea about the phosphate cycling should not be applied throughout the whole earth's history (Schidlowski, 1988). Thus, although biological activity is dependent on many other factors, e.g., temperature, phosphorus and so on, the development of a biosphere is thought to be primarily controlled by the internal and mutual antagonism.

Our proposed system constructs a mathematical model composed of a set of non-linear equations as follows:

$$dx_b/dt = kx_a x_b - Dx_b - Ex, \quad (4)$$

$$dx_a/dt = -kx_a x_b + Gx_r + Ex, \quad (5)$$

$$dx_r/dt = Dx_b - Gx_r, \quad (6)$$

where t is time, and x_b , x_a , and x_r indicate the proportion of the earth's carbon which is in the biosphere, atmosphere-ocean and rocks, respectively.

The procedure used to obtain the solutions was as follows: for a fixed value of S , initial values of x_a , x_b , and x_r were set at the previous steady state values; the equations were integrated forward in time until a steady state was reached; the value of S was changed incrementally and the procedure was repeated. Calculations were performed for various values of the parameters. Some of the results are shown in Figs.2-5. In these figures, the distribution of carbon (%) for each sphere of influence or temperature is plotted for S . In the following figures, P_0 gives one measure of the ratio of the amount of CO_2 circulating in the cycle shown in Fig.1 to the total CO_2 existing on the present earth's crust. This P_0 is expressed with percentage of the current amount of atmospheric CO_2 (0.03 kPa) to the total CO_2 cycling through the system shown in Fig.1. For instance, $P_0 = 0.001(\%)$ means that 3 MPa of CO_2 , i.e., about one half of the total CO_2 existing on the earth's crust, is assumed to be circulating within the cycle shown in Fig.1. Similarly, $P_0 = 0.01(\%)$ corresponds to 0.3 MPa, and $P_0 = 0.1(\%)$ to 0.03 MPa.

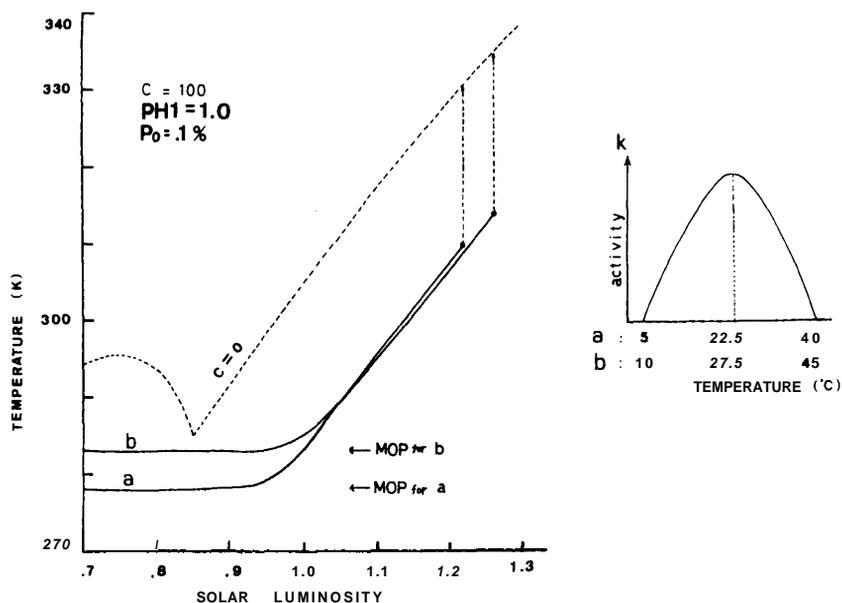


Fig.2. Temperature change of the earth for the gradual increase in solar luminosity S , for variation in temperature dependence of biological activity k . In the case a minimum activities appear both at 5°C and at 40°C, and in the case b at 10°C and 45°C. $C=0$ represents abiotic conditions. At the dot mark drawn at the end of each solid line, our ecosystem is suddenly destroyed and the temperature jumps toward that which prevails at $C=0$. Case b can survive longer than Case a, owing to a shift of the biological activity toward higher temperature range. Where, $G=1$, $D=E=10$. A minimum operating point (MOP), i.e., a minimum temperature for both the biota and its environment to coexist is indicated for each Case a and b.

Results

Although the partitioning of CO_2 between the atmosphere and the ocean is thought to be mainly determined by acidity of the ocean, historical records of the changes in this acidity are still lacking. So, we have tentatively parameterized the partitioning of CO_2 , by introducing the partition rate PHI which expresses the proportion of carbon present as CO_2 in the atmosphere at the initial time, as follows: from the initial time corresponding to $S=0.7$ (the factor 0.7 of the current solar flux) to the time of $S=0.85$ (the factor 0.85 of the current one), i.e., from 4.5 Gyr ago to 2.25 Gyr ago, and assuming linear increase in solar luminosity, the proportion of total carbon present as CO_2 in the atmosphere decreases linearly with time from PHI toward the current value, i.e., one tenth of the total amount of carbon existing as CO_2 within the ocean-atmosphere system because of the change in acidity of the ocean. As the standard case, $\text{PHI}=1.0$ is used in the following calculations.

After the time of $S=0.85$, the partitioning of CO_2 between the atmosphere and the ocean is assumed to become constant. Discontinuities appearing at

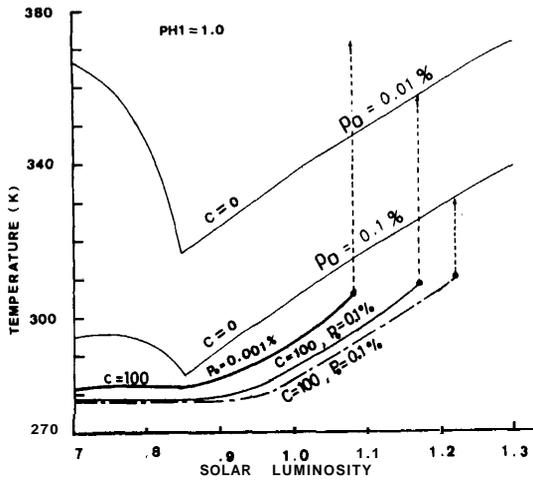


Fig.3. Same as Fig.2 but for the variation in P_0 .

$S = 0.85$ in the following figures are due to this assumption. However, the assumption about the change in acidity of the ocean does not affect our conclusion at all.

Fig.2 shows that, within the solar flux range, $S < 0.96$, in which biota can control freely the amount of the atmospheric CO_2 , there is a minimum operating point (MOP), i.e., a minimum temperature for the biota to coexist with the earth's environment. And Fig.2 reveals that the earth's environment is always set up to the MOP state if there is a sufficient amount of the atmospheric CO_2 for biota to control freely. The existence of MOP is independent, despite variation in temperature dependence of the biological activity (the Case a and the b). The reason that this MOP state is set up is described in (A) in Conclusions and its conceptual explanation is given in Fig.7. In the following, we use the Case a as a standard one.

Fig.3 indicates that, independent of the magnitude of P_0 , the earth's surface temperature is set up close to the MOP condition. As discussed later, the independence of the results on P_0 plays a very important role in the establishment of the bio-regulated system on the earth. Fig.3 also shows that the more P_0 increases, i.e., the more CO_2 the bio-system must control at a given level of activity, the more precarious becomes survival of the ecosystem.

Fig.4 shows the existence of a unique solution (AO in this figure) for CO_2 level in the atmosphere-ocean system, independent of various values of transportation rates. Thus, the regulation of CO_2 level in the atmosphere-ocean system is a universal phenomenon. Even if S exceeds 0.96, the atmospheric CO_2 level remains low and the amount of carbon in the lithosphere and the biosphere remains high, although, because of very small values of these variations, there seems nothing in this figure that shows any continued lowering of the atmospheric CO_2 level, or increasing levels in rocks and biosphere.

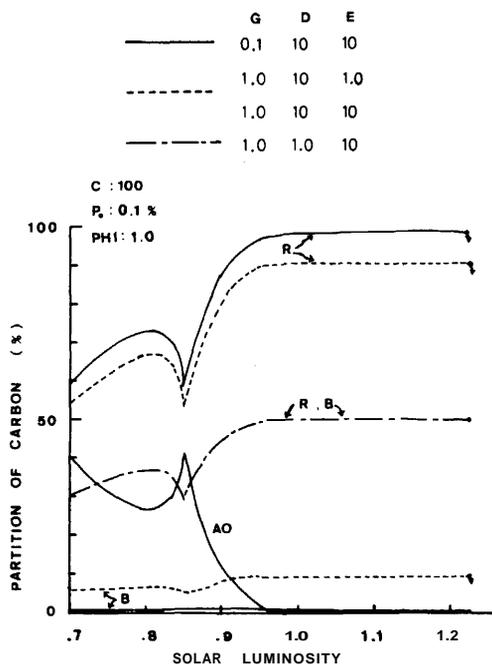


Fig.4. Distribution of carbon in each sphere for an increase in solar luminosity S . Calculations are carried out for various changes in transportation rates G , D and E in eqs.(4)-(6). Despite the various values given to these rates, the distribution of carbon in each sphere is regulated for all the cases, so as to maintain the same homeostasis as in the standard case a of Fig.2. Consequently, a unique solution, AO in Fig.4, exists, independent of various values of transportation rates. Where, B, AO and R indicate the biosphere, atmosphere-ocean and rocks shown in Fig.1, respectively.

Fig.4 explains why there was less biomass on the earth in the past than there is at present. This is surely due to the regulation of the atmospheric CO_2 level to counteract an increase in solar luminosity, and not only to the problem of biotic evolution. Strangely enough, the beginning of the breakdown of the MOP state coincides with the start of the Cambrian, when life suddenly flourished on the earth's surface. This implies that, by adding carbon to the biomass, the earth's ecosystem may have been opposing an increase in solar luminosity.

We also investigate the effects of the temporal development of biological activity on regulation. In this case, the biological activity C' which varies with time, is defined as follows and replaces C in eq.(1):

$$C' = C \times 10 \frac{S_L - S}{S_L - S_0} \log r \quad (7)$$

with this C' , the following k is used instead of eq.(1):

$$k = C' \{ 1 - a(T_1 - T)^2 \} \quad (8)$$

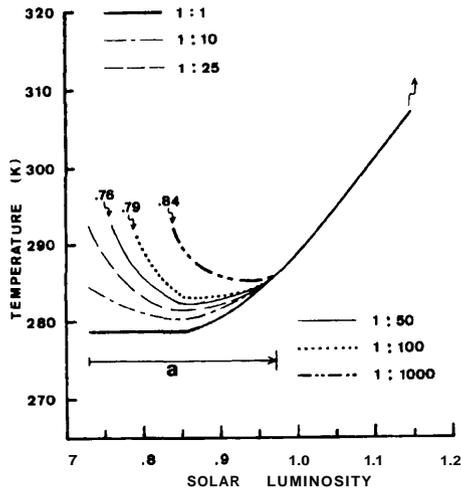


Fig.5. The averaged temperatures of the earth's surface adjusted to the dim Sun, when biological activity increases by degrees with ratios described in the figure within a certain period of time, from 4 Gyr to 0.4 Gyr, which is shown by an arrow head marked with a.

Here, S_0 and S_L are constants about S . We assume that at $S=S_0=0.74$ biota start to be active and after $S=S_L=0.97$, C' is fixed to be equal to C . The parameter r gives the ratio of potential biological activity at $S=S_0$ to that at $S=S_L$. By r , we can parameterize tentatively the evolution over time of the ability of the biota to regulate CO_2 .

As indicated by the $r=1:1$ case, (the solid line in Fig.5) wherein the potential biological ability for regulation has been constant, our earth's environment has been controlled by biota so as to be in homeostasis from the early ages of the earth. However, as the $r=1:1000$ case shows, if the biological ability to regulate CO_2 was very weak in the early ages, the earth's environment is outside biological control until S reaches 0.84. We can see from Fig.5 that the greater the biological ability at $S=S_0$, the longer the earth's environment can maintain the MOP state. Fig.5 is in good agreement with some data which show that the early earth might have been rather warmer than at present (e.g., Knauth and Epstein, 1976). Our results suggest that evolution of the ability of the biota to regulate CO_2 may greatly have affected the earth's early environment.

Discussions

It should be stressed that, unlike the Walker cycle, our model does not assume any explicit feedback mechanism. The only assumption is that temperature influences the biological activity (cf. eq.(1), Fig.7), which is a universal property of life.

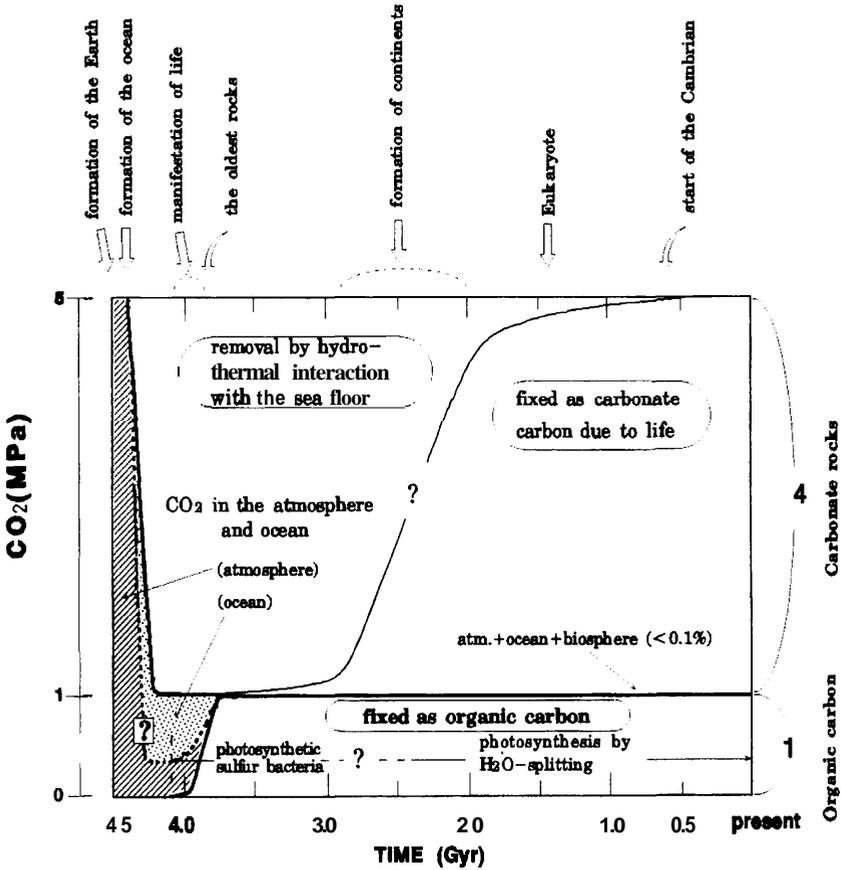


Fig.6. Time-dependent distribution model for carbon on the earth.

The existence of our proposed bio-controlled system on earth from the early stage, not long after the earth's formation, is not mere speculation. There is sufficient evidence that photosynthesis has existed as a biochemical process for almost 4 Gyr. The ¹³C/¹²C ratio in sedimentary rocks is an indicator of the principal carbon-fixing reaction of photosynthesis. Based on a 3.8 Gyr isotopic record, Schidlowski (1988) and Holser et al.(1988) recently pointed out that biological control of the terrestrial carbon cycle may have been established very early and that it was in full operation when the oldest sediments were formed. These authors also point out that photosynthesis has been an important agent in the geochemical transformations of the earth's surface for almost 4 Gyr. Moreover, their results that the ¹³C/¹²C ratio has been nearly constant suggest that about one-fifth of the total carbon in the surficial compartment has been in organic form throughout the ages. As Schidlowski says, this means that the ancient earth could have been in a state of global biot-

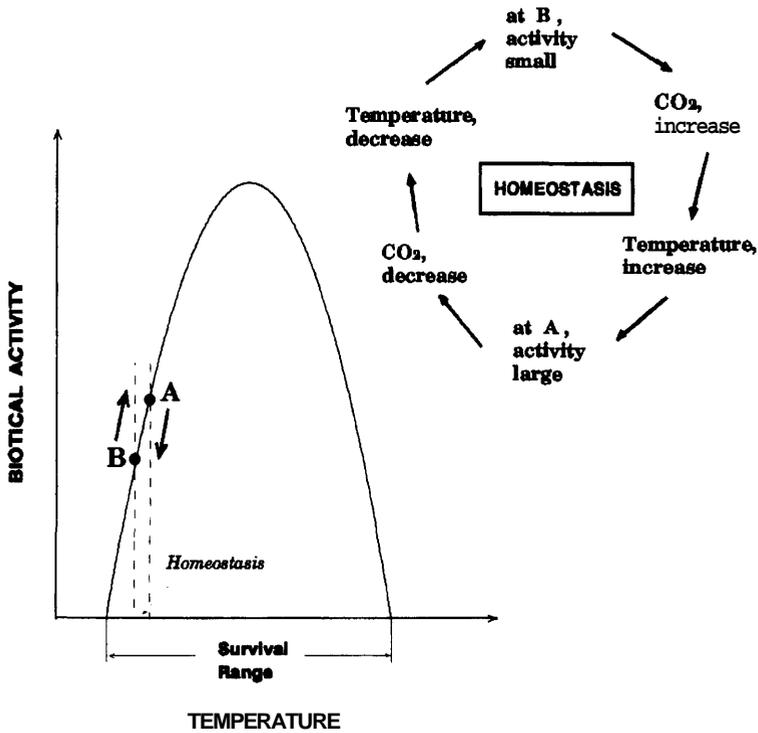


Fig.7. A conceptual diagram on a mechanism for life to make homeostasis of the earth's environment.

ic saturation. Prolific prokaryotic communities probably bloomed to limits ultimately set by the availability of critical resources.

Bio-regulation of carbon cycling may have been much more effective than our results suggest. CaCO_3 in the sea must have been produced mostly by marine biota, in the past just as in the present (Broecker, 1974). Marine biota would have controlled the atmospheric CO_2 levels from very early in the earth's history by controlling the production of CaCO_3 . As for the changes in the atmospheric CO_2 concentration which occurred during the ice age, Knox and McElroy (1984) propose a model in which the production of CaCO_3 is closely linked with the activity of the marine biota. Thus, considering an important role of marine biota upon the production of CaCO_3 , regulation due to biota would have been stronger than that in the model of organic production only. Introduction of such an effect will be put into our model in the future since our results are obtained on the basis of the bio-regulation relating to organic carbon production only.

The carbon excluded from our proposed cycle would, through hydrothermal interaction at the sea floor (Walker, 1985) produce large amounts of inorganic

carbonate rocks. However, it should be emphasized that as soon as carbon appears to the atmosphere-ocean system in a form available to biota, it falls under the control of our bio-regulated system. Thus, again, carbon has been redistributed into each reservoir so as to maintain homeostasis in the earth's environment, i.e., so as to keep the amount of the atmospheric CO_2 nearly invariant in order to hold temperature constant. Whatever disturbances may occur in our system, our proposed mechanism still keeps our environment in homeostasis, because, as indicated in Fig.3, homeostasis is not disturbed by variations in the magnitude of P_0 .

For instance, let us suppose that there is 1 MPa of CO_2 in the atmosphere-ocean system in the early Archaean, before our bio-controlled system appears. Most of the rest, i.e., about 4 MPa of CO_2 , was in the form of inorganic carbonate rocks. Most of the CO_2 in the atmosphere-ocean system (1 MPa) would be present in the air, due to the acidity of the early earth's ocean until our bio-regulated system for the carbon cycling had appeared about 4 Gyr ago. The abiotic model, i.e., the hydrothermal interaction cycle described above, estimates a very thick atmosphere composed of about 1MPa of CO_2 due to the scarcity of continents (Walker, 1985). However, once our proposed system was established on our planet, the MOP state would appear on the earth before long.

It is by no means difficult for biota to bring the system under control in a short time, because biota sustain a formidable rate of fixation of atmospheric carbon. One should note that microbial communities (specifically cyanobacteria), which are among the most productive ecosystems we know of, have rates of CO_2 fixation that render them capable of producing all the organic carbon currently on the earth within 10^4 - 10^5 yr.

Thus, about one-fifth of the carbon within the earth crust would be in circulation in the organic carbon cycle proposed, so as to regulate the amount of the atmospheric CO_2 to maintain our environment in homeostasis. This inference is in accord with the records of carbon isotopes indicating that organic carbon has comprised about one-fifth of the total carbon in the surficial compartment for almost 4 Gyr.

One should recall that, as previously mentioned, the ancient earth was never abiotic but, rather, was in a state of global biotic saturation from an early stage. It should also be noted that the rate of carbon fixation in microbial mat communities, stromatolites, would have been higher at the greater levels of atmospheric CO_2 that were present in the past (Rothschild and Mancinelli, 1990).

In Fig.6, we propose one model for distribution of carbon (or CO_2) on the earth, which is in agreement with the carbon isotopic data. Before the ocean was created, all the carbon was in the atmosphere in the form of CO_2 gas. Once the ocean had been formed, the atmospheric CO_2 would gradually have dissolved into the ocean, with an increase of pH of the sea due to release of cations supplied by dissolution of basalt into the ocean. Deposition of carbonate rocks followed, due to hydrothermal interaction at the sea floor (Walker, 1985). The partitioning of CO_2 between the atmosphere and the ocean is strongly dependent upon the pH of the ancient ocean. However, as soon as life

had appeared on our planet about 4 Gyr ago, the bio-regulated system for the carbon cycle that we suggest in this paper would have been in full operation. Thus, from the early stage up to now, the ratio of organic carbon to carbonate carbon has been fixed at 1:4, as indicated by the isotopic data. Life would have taken charge of removal of about 1 MPa of CO_2 from the atmosphere, and have deposited it as organic carbon. If life had been absent, this CO_2 would have remained in the atmosphere. Fig.6 shows that this CO_2 fixation was at first accomplished through bacterial photosynthesis by e.g. sulfur bacteria. Bacterial photosynthesis preceded the evolution of the H_2O -splitting reaction (e.g., Holser et al., 1988).

In the early stage, carbonate rocks would have been produced by the abiotic mechanism of hydrothermal interaction at the sea floor, as pointed out by Walker. However, more carbon would gradually have been fixed by marine micro-organisms as CaCO_3 (Omori, 1988; Okazaki and Setoguchi, 1988). As the process of photosynthesis intensified, this increase would have accelerated because the rate of calcification in micro-organisms such as algae increases linearly with photosynthesis (Borowitzka and Larkum, 1976). The increase in continental area would also have accelerated the CO_2 fixation into CaCO_3 . As the supply of cations increased through weathering of the land masses which emerged some 25 Gyr ago, marine biota would gradually have become the predominant source of CaCO_3 . After the Cambrian, marine life had developed such calcification mechanisms as formation of coral skeleton and mineralization of mollusk shell. The so called silicate weathering mechanism for deposition of carbonate minerals would have been highly accelerated by the emergence of these marine biota. Today, above 95% of carbonate rocks are of organic origin. This is also shown in Fig.6.

Conclusions

In summary, we conclude as follows:

- (A) Until the incident solar flux reaches the value point $S = 0.96$, biota attempt coexistence with the earth's environment by regulating the atmospheric CO_2 level so that the average temperature coincides with their MOP. On the other hand, if the average temperature were to be set at higher level, atmospheric CO_2 would decrease in time, due to the higher activity of biota. In consequence, the temperature would decrease to a point which biota would not tolerate. This conflict between CO_2 level and biological activity is the reason why biota prefer to keep the MOP on the cool side of the optimum for the biota and to keep the earth's surface temperature in homeostasis. As soon as the biosphere had appeared on the early earth, biota and the earth's environment started to coevolve as a holistic system. Or, we can say as follows: life cannot coexist with an environment which is unfavorable for life. This "Principle of Existence" has kept the earth's environment in homeostasis for a long time. A conceptual diagram of this is given in Fig.7.

(B) Our bio-regulated system must resist the increase in solar flux by lowering the atmospheric CO₂ level. According to our result, when the solar flux S reaches 0.96, even an atmospheric CO₂ level nearly equal to zero is too large to maintain the MOP condition. Today, only a few hundred ppm of CO₂ remain in the atmosphere for biota to regulate. Although biota do their best to lower the CO₂ level in the atmosphere and to survive as long as possible by transporting carbon into the biosphere and the rocks, the increasing solar flux will eventually overcome the effort of the biota. This will bring about the end of our bio-regulated system. This prediction is in good accordance with that of Lovelock and Watson (1982). According to the present model, our ecosystem is now on the way to ruin, having already left the MOP state some six hundred million years ago, which strangely enough, coincides with the start of the Cambrian, when life suddenly flourished on the earth's surface.

Acknowledgments

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A Guide to UFO Research

MICHAEL D. SWORDS

Department of Science Studies, Western Michigan University, Kalamazoo, MI 49008

Abstract— The very natural query of the interested intellectual: what should I read to understand the status of the UFO Phenomenon?, has no simple answer. This review article briefly examines the characteristics of the "UFO Problem" and relates those characteristics to the problem of ignorance in the academic community. An "inside look" at the appropriate library for the "working UFOlogist" is then explored.

The UFO Research Problem

When a serious person decides foolishly to throw all of his or her extra time into finding out about UFOs, there begins an intellectual avalanche of connections and demands which is, essentially, endless. This field is forever rich, but that richness betrays a multidisciplinary which defies any amount of training. This subject is, in fact, the most multidisciplinary one that this author has ever experienced (and that includes degrees and teaching in areas like History of Science and Technology, Science-Technology - and - Society, and Environmental Studies).

The naive academic shuffles onto the queer streets of UFOlogy with the confidence that the world makes sense, order will be quickly restored, and logic will suffice. What our intrepid ivory-tower dweller finds are chaotic beehives of reports which sting the intuition into felt perceptions of partial order, but which resist simplification under any useful scheme. Who are these people? What are they like? What motivates them? Are they normal? Abnormal? Supranormal? What stimulated these "stories"? Have they been going on for a long time? Are there patterns in this stuff? Easy to explain patterns? Repeatable patterns? What caused the photos? The traces? The field effects? The radiation effects? Is any of this related to government activities? Military technologies? Extraterrestrials? The Paranormal? Could it even be?

There are many more questions than these. These are just the first "general" waves of the avalanche which attack you (if you have any intellectual honesty). I have made an incomplete matrix of the disciplinary demands upon the serious UFOlogist in Figure 1. Its purpose is not completeness or absolute interconnected accuracy. It is just to illustrate a point: there can be no properly trained UFOlogist. Anyone external to the field should take all pronouncement about UFOs from individuals with a grain of salt. The more absolutist those pronouncements are the more they should be buried in sodium chloride. And as for a "quick reading list" to suddenly become deeply knowledgeable in

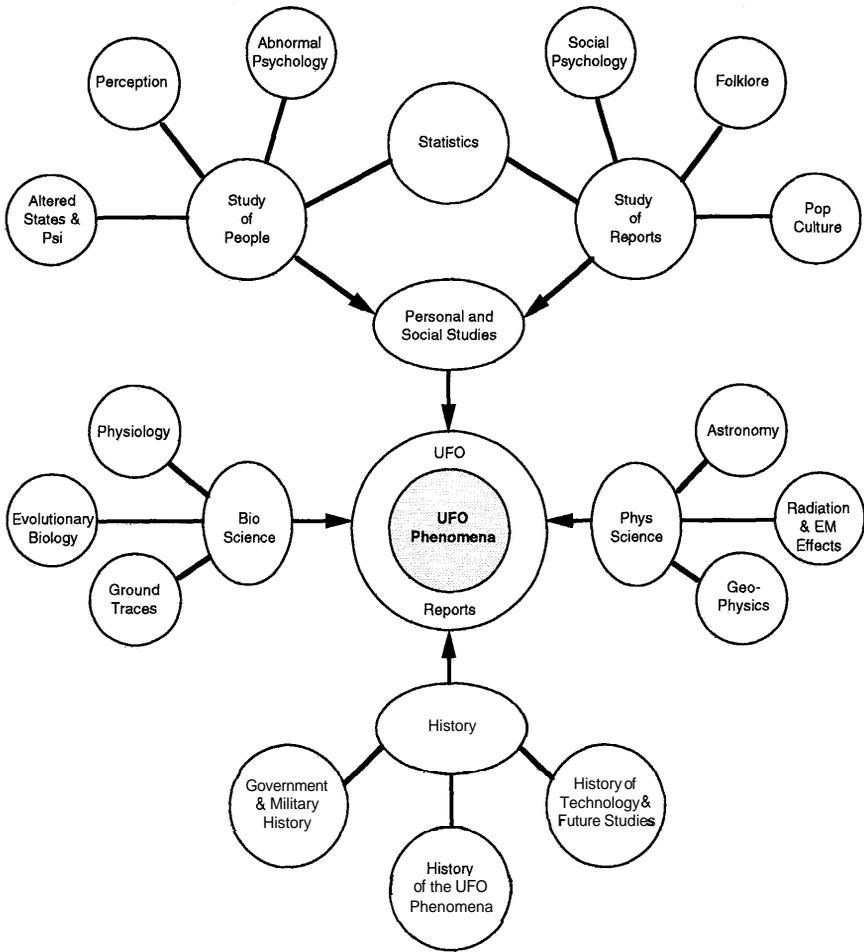


Fig. 1

the "problem": (as Alexander the Great may have been told) there is no royal road to UFOlogy.

A few further words about the multidisciplinary matrix before going on with another serious problem vis-a-vis "reading about UFOs". We highly trained intellectuals, all proud of our mental abilities and acquired knowledge, have tended not to behave very well in the face of this daunting array of demands. Some individuals, having barely paddled their toes in these strange waters, have actually had colossal enough egos to write "authoritatively" on the subject (Irving Langmuir and certain astronomers come immediately to mind). Such individuals have immediately (though no one knows on what grounds) shrunk the matrix into one or two narrow disciplinary areas, and then applied wholesale ignorance of detail to cram the phenomena into some pseudoscientific simplistic hypothesis. These forms of "Universal Theories" are almost always

ridiculous (as far as explaining anything profound about the phenomena) to anyone who has spend serious time in the field. They are also almost always consciously described as debunking the "extraterrestrial hypothesis", and they almost always sound reasonable to the UFOlogically ignorant inhabiting the corridors of academia.

A similar form of inadequate reductionism comes from the other end of the enthusiasm scale. Some UFO proponents (whether in the field extensively or not) react to the complexity by creating a theoretical tar-pit of such osmotic omnipotence that it can absorb anything that falls into it. (Certain "New Age" and Parallel Realities speculators come to mind.) Also, however, there are extraterrestrial theorists who make the ETs so powerful and inscrutable that there are no restrictions on their behavior or technology. This doesn't mean that such concepts are necessarily false, just that they are intellectually dangerous and untestable, and should be voiced with reservation and humility. Long-term involvement in the field is no protection against this latter form of reductionism: in fact it often seems to encourage it. This is only human. We want to see the "Truth". If we have expended so much lifetime in its pursuit, perhaps we even need to see it. In my twenty years of fairly intense interest in this subject, I have rarely found individuals who have continually managed to keep their balance. I have found no "debunkers" who have even attempted to do so. This sadly bodes ill for one looking for good books to read.

The Universal Theory is almost surely doomed before it is even birthed, As you immerse yourself in the richness of the history and the reports, it becomes almost obvious that you are dealing with many sorts of causal agents. The demands of the matrix are probably (in part) a reflection of the multiplicity of causes. Is there some important new social psychology buried in there? Probably. Is there some important new personal psychology buried in there? Probably. Military-government activity? Geophysics? Meteorology? Probably, probably, and probably. And is there some extra-terrestriality (or other equally astonishing reality) buried in there? I say: probably ... maybe "both". People anxious for a simplistic world (whether romantic or mundane) seem driven to devise a reductionist concept. It has never worked. Such views persist at embarrassing costs to the details of the case histories.

"UFOs" (or their reports) seem likely to have many causes, even though nearly all of us seem drawn to only one. We seem unable to think in several other ways which cause us endless troubles as well. I will mention two. Surprisingly, the typical academic seems as incapable of thinking in historical perspectives as our oft-accused students. Our minds seem to become continually locked into what we're experiencing right at this moment, and neither the weight of the history of the phenomenon nor the "arrows" or lessons of our larger history (of arrogances and successes) seem to play their proper roles in producing measured and wise thinking on any of these matters. A good historian is necessary to make a good futurist. And a good historical matrix is the cool solid foundation from which creativity and wisdom may spring. Have we instead become the impatient children that we decry in our educational writ-

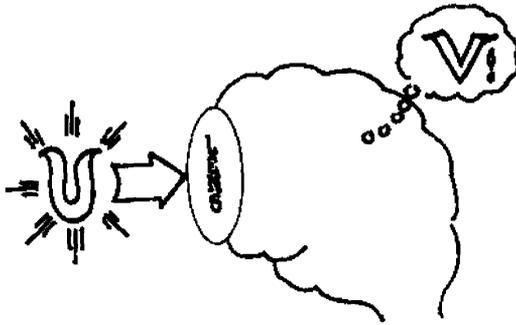


Fig. 2

ings? Are we the Intellectual Hot-and-Now generation? The greater the heat, the greater the increase in entropy, I believe.

A second, more understandable, blind-spot involves "intelligence". This intelligence is the type of thinking in the shadow-world of agents and people concerned with military and national security. This was a major blind-spot for me with my naive country boy's ways of thinking that people and events were generally what they seemed. Scientific training accentuated that "what you see is what you get" simple mindedness. History training fooled me into believing that there is always a clear "paper trail", and the clever scholar can rather quickly uncover it. This was true despite my cynical understanding that the histories are written by the victors, and that the messy intuitions and serendipities of "science" are always polished and straightened before they reach the journals. The depths of the misdirections and distortions of the shadow-world of national security intelligence have only incompletely penetrated my sheltered brain. But I believe that I know this much at least: the first priority in the interaction between two potentially hostile organizations is not science nor any "normal" form of overtness, it is security. Research is entirely subverted to security concerns. And no one "lands on anyone's White House Lawn". The foolishness of some of our academic statements about "proper UFO behavior" have been the cause of much derisive giggling by former intelligence officers that I have chanced to be around while one or the other of us (the UFOlogists) has voiced naive opinions. And, when it comes to the upper echelons, don't kid yourself: there is nothing oxymoronic about military intelligence.

This UFO business, as you see, is incredibly complex. It is an intellectual vortex which can easily addict you, draw you in, and still tell you very little about the fundamentals which lie below. It may be a Black Hole which has no bottom, and which defies all academic "laws of nature" inside its event horizon. Part of its impenetrability is its dependence upon people, and that is the last point that I want to make before moving on to the library.

Anyone interested in "Anomalous Reports" of any kind realizes that the initial research problem is the accuracy of the relationship between the descrip-

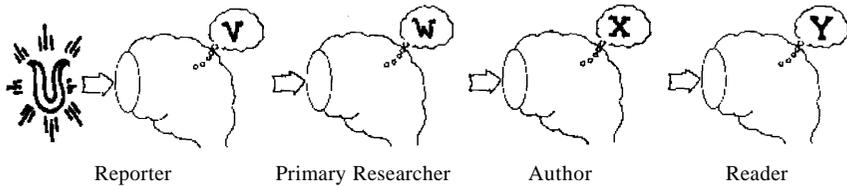


Fig. 3

tors in the report and the actual stimulus for the report. None of us, either in UFOlogy or elsewhere, are so naive as to assume that reports transmit to us the precise characteristics of the external reality experienced by the reporter (if in fact there was any external reality at all). Every reporter has an "interpretive lens" through which experiences pass on their way to those associational areas of consciousness which attempt to integrate them with that already known. This "lens" includes the physical and social environments in which we find ourselves, plus our own perceptual and mental idiosyncrasies (temporary or chronic). Our lens may profoundly or insignificantly distort the experience, but it is almost a certainty that some change occurs between the event and its report.

There are ways in which we attempt to deal with these problems of "subjectivity", as we all know. But a simple point for the reader-of-books may be worth making. "Events" don't only occur to the reporters of anomalies. When those reporters report, this is an "event" for the primary researcher listening to the report. That researcher must deal with his or her own "lens" and write that report up accordingly with some added distortion. UFO catalogers and synthesizers gather these reports using their own distorting lenses, and perhaps, write the books which you will read. Note that you, too, have your own lens. (Figure 3).

The "great UFO books" that you will read contain materials probably thrice-distorted. But doesn't that happen in all fields of academic study, and we seem to survive? Perhaps. But in academia there is some form of truly interacting, critiquing, and challenging community. It doesn't work very efficiently, but in some fields at least it seems to ultimately sift wheat from chaff. There has been no such stable UFO intellectual community. There are few proper fora, and until recently there were none at all. There are arguments a plenty, but they occur independently of any proper mode of lasting evaluation. There are signs that this is changing as we speak with the emergence of the SSE, the Journal of UFO Studies, and groupings of "Invisible Collegians" coming out of the closet stimulated by the so-called "Abduction" phenomenon. The point of the lenses is that their existence, critically unchecked by a larger community, makes the UFO reader's job harder yet.

The UFO Research Library

A typical history of a serious UFO researcher begins with the reading of some stimulating book (for an astonishing number of us old timers it was the

same book: Captain Edward J. Ruppelt's *The Report on Unidentified Flying Objects*. For youngsters it might be something by Hynek or the early Vallee, or (horrendously) Whitley Streiber's *Communion*). These were read naively but with the intuition that there was something important here. As one submerges more deeply, the clearest thought which arises is that one must cease reading these sorts of books (at least exclusively) and get closer to the raw data. Even heroes have their lenses and their limitations.

The serious UFOlogist's research library, thereby, maximizes closeness to the data. Since UFOs, UFOnuts, and UFO pieces are not readily available, "closeness to the data" means primary case investigation reports (both written and aural). Secondly, it means direct extractions from the primary reports to be used in catalog and pattern-finding forms of compilations. Thirdly, it consists of declassified and FOIA'd documents (largely from the U. S. military and "civilian" intelligence agencies). Fourthly, it is the direct test-data available from so-called "second-level" case research (ex. physical and chemical lab analyses, psychometric examination of CEIII witnesses, medical records et al.). The processing of this information takes a serious commitment on the part of the reader, and so is unlikely to interest an individual looking for "a few good books".

My personal files contain the primary case documents for only about 200 cases. There are also probably fifty or so audio tapes interviews with primary witnesses. This is a puny primary data base compared with the real long-term case researchers in the field. Some of them own or have ready access to the USAF Project Bluebook microfilms containing all available project investigative documents from 1947 - 1969 (13,000 sightings). Casual readers would be unlikely to run into the opportunity to read such document files even if they wished to. The closest that such a reader can get to such information is to read either an in-depth study of a specific case (compiled by a primary case researcher) or a "data-collection". Some examples of the "classic case study" are:

1. Jennie Zeidman's *A Helicopter-UFO Encounter Over Ohio*;
2. Norman Cruttwell's *Flying Saucers Over Papua*;
3. Isabel Davis and Ted Bloecher's *Close Encounter at Kelly*;
4. Kevin Randle and Don Schmitt's *UFO Crash at Roswell*;
5. Raymond Fowler's *The Allagash Affair*.

Occasionally there are books aimed at the popular market which are still reasonably accurate and close to the data (John Fuller's *The Interrupted Journey* and *The Exeter Incident* are two). Other superb studies like Walter Webb's *The Buff Ledge Incident* languish in unpublished manuscript form. If one could read French, and had the opportunity available, the acquiring of the Gegan case series (especially vis-a-vis Trans-en-Provence) would afford a further data closeness from reputable researchers. The Center for UFO Studies is currently beginning to mine its tremendous store of primary documents to pro-

duce packets on historical classic cases for interested researchers. Those available will include Betty and Barney Hill, Kenneth Arnold, Captain E. J. Smith, Levelland, and the Portage County Police Chase. Certain historical audio tapes will also be made available, and, of course, any interested party could always research the complete files in Chicago.

The second more-or-less readily available data-sources are the primary "collections". Some of these are:

1. Mark Rodeghier's *UFO Reports Involving Vehicle Interference*;
2. Ted Phillips' *Physical Traces Associated with UFO Sightings*;
3. Dick Hall's *The UFO Evidence*;
4. Ted Bloecher's *Report on the UFO Wave of 1947*;
5. A splendid set of compilations by the Australian researchers Keith Basterfield and William Chalker.

Other data bases exist in various forms, of course. The extensive CUFOS case catalog "UFOCAT" is currently on computer disk and available to researchers. The Ted Bloecher-David Webb catalog of humanoid cases, HUM-CAT, is close to being so. Other data bases involving EM effects, physical traces, airplane encounters, paranormal involvements, even Big Foot have been gathered and published in paper or small monograph form.

Declassified and FOIA'd documents now abound and provide a real eye-opener for the naive academic assuming that only non-serious individuals spend any time on this topic. The Blue Book microfilm of twenty-two years and 13,000 cases has been mentioned. FOIA'd materials occur in the hundreds of pages from the USAF, FBI and the CIA. For those genuinely interested, many of these have been made available by the FUND for UFO Research, and by other organizations (ex. CUFOS) and individuals. A particularly rich source of mystery are the materials concerning the Green Fireballs phenomenon of 1949-51 or so. These documents were collected and made available by William Moore. If one wished to read "a real book" on the materials, one which does a fairly objective job of presenting some of them is Barry Greenwood and Larry Fawcett's *Clear Intent*. Keeping up to date on these government releases requires reading the periodical, *Just Cause*.

The last category of "primary data" mentioned involved the second-level or depth research on cases or with witnesses. This is almost never available in any way other than serious research articles. These articles require the researcher to own or have ready access to a variety of periodicals or similar sources. Such articles appear in Symposia proceedings (esp. the two run by CUFOS, but also scattered in MUFON proceedings among a mixed-bag of serious and embarrassing presentations), the *Journal of UFO Studies*, some issues of the *British Flying Saucer Review*, GEPAN materials, the European journals UPIAR and *Cuadernos*, and a wide scatter or appearances elsewhere. New developments, such as TREAT and the MIT conference on Abductions, produce collections of higher quality writing, but as yet rarely containing primary data. These sorts

of resources, mentioned above, form the working core of the serious researcher's library. There are other elements of nearly equal importance, and slowly we will approach the materials the non-UFOlogist actually wants to read (whether they will do them any real good or not).

Every field of any maturity has at least a few standard works which support the learning and locating process. UFOlogy has a few. Far and away the most important single supporting resource in UFOlogy is George Eberhart's colossal two-volume bibliographic effort: *UFOs and the Extraterrestrial Contact Movement*. Covering all areas of UFO-related material and all types of media, it is endlessly useful to the scholar. There are two well-done standard encyclopedias for the field: Margaret Sachs' *The UFO Encyclopedia* and Ronald Story's *The Encyclopedia of UFOs*. Both of these works are structured mainly in the short informative newsclipping style of encyclopedic list-and-learn-a-little philosophy. Sachs' book is entirely in this mode. Story's employs many different writers and tends to have longer commentaries. Both are well-researched and worth owning. A third encyclopedia in multi-volumes is being produced piecemeal at this moment: Jerry Clark's *UFO Encyclopedia* slated for three volumes. Two have appeared as of 1992: *UFOs in the 1980s* and *The Emergence of a Phenomenon*. The third volume will fall in the intervening time period. Clark's writing takes advantage of his great literary skills and long-time involvement in the field. The entries tend to be idiosyncratic to his interests, but his interests have been so wide as to allow good coverage anyway. The pieces tend to be in the manner of historical research essays stacked together with biographical notes in an encyclopedic alphabetical array. Clark is an excellent historical researcher and the encyclopedia will become a UFO classic.

The standard history of the field is David Jacobs' *The UFO Controversy in America*, one of the few books on our subject published by a scholarly press.. It is the fruit of Dr. Jacobs' Ph.D. thesis and the research depth and excellence shows it. It is now being, and will someday be even more so, supplemented by a rather different sort of "history": a many-volumed year-by-year collation of UFO events by Loren Gross. Digging heroically into the minutiae of press coverages, government documents, and whatever else is findable, Gross is slowly producing a documentary retelling of the phenomenon. *UFOs: A History* has laboriously worked its way into the mid-50's with over a dozen volumes passing the assembly line. A last "support work" type of volume which deserves mentioning is the "How to Do UFO Research" book. There are many investigator manuals which have been offered, but the classic remains Allan Hendry's *The UFO Handbook*. A second very useful aid is Richard Haines' *Observing UFOs*.

These constitute the major working elements of the UFOlogical research library: primary data documents, primary government documents, data-collections, depth research publications, and important support resources. Almost none of this rates high in readability by the layman. These materials are but-

tressed in the library by several other forms of literature of more or less value: UFO journals of the magazine or newsletter variety, writings by intelligent persons (whether overly knowledgeable about what they're writing about or not), general UFO books, miscellaneous "strange stuff", and materials from non-UFO fields which are applicable to elements of the subject. Let's pick around at these topics and see if we can find anything worth our time.

History is very important in this field, and because of that it is important for the researcher to maintain a healthy storage of older journals and newsletters to refer to. These journals are prophylactic against the errors of reading the polished final versions of the later books. They remind us that many classic situations were not anywhere near so clear when one gets closer to their time. Distance is important. So is proximity. The intellectually honest researcher oscillates between them to find the points of focus. To allow the proximity certain journal-newsletters of the "classical era" seem more important than others: *Flying Saucer Review* is obvious; so are the *APRO Bulletin*, NICAP's *UFO Investigator*, and many smaller newsletters by the researchers CSI (Los Angeles), CSI (New York), Leonard Stringfield, Lee Munsick, and several non-US publications such as *UFO Critical Bulletin* (Brazil), *Australian Flying Saucer Magazine*, *Flying Saucers* (New Zealand). Of course if you read languages other than English there are more. As one moves out of the 1950's and 60's, these journals offer less "historical perspective" obviously. They become more numerous and available and one needs to decide on where to spend one's money. There have been excellent journals in Britain and Australia over the years, but most American researchers subscribe to one or both of the prominent U. S. productions. They are, as Dennis Stacy (editor of the *MUFON Journal*) has observed, the "magazine" of the field (The *International UFO Reporter* of CUFOS) and the "newspaper" (The *MUFON Journal* of the Mutual UFO Network). *IUR* tends to be more selective, scholarly, and critical. *MJ* tends to be more adventurous, populist, and entertaining. The journals accurately reflect the roles of their organizations as pursued by their leaderships. Now there is finally a true academic journal, the *Journal of UFO Studies*, dedicated solely to scholarly writing. It is an annual in its fourth volume.

Writings by intelligent persons: well there has been a surprising lot of that. By far not all of it makes any sense, despite the intelligence behind it. This is because as we have seen, intelligence is no proof of knowledge nor open-mindedness. These "intelligent writings" come in the forms of books, article collections, and loose articles. In the latter category are many useful things but impossible to list usefully here. The key authors to locate would be persons such as Allen Hynek, Frank Salisbury, and James McDonald. McDonald is especially significant in this form since his writings are not available in books. Abductions research has spawned several isolated articles worth noting: Leo Sprinkle's "Hypnotic Time Regression Procedures in the Investigation of UFO Experiences", Terence Dickinson's "The Zeta Reticuli Incident" (vis-a-vis the Hill Star-Map controversy), Bloecher, Clamar, and Hopkins' "Summary Re-

port on the Psychological Testing of Nine Individuals Reporting UFO Abduction Experiences", Alvin Lawson's "Perinatal Imagery in UFO Abduction Reports", and John Mack's "The Abduction Syndrome: A Preliminary Report". An unending chain of other such materials could be listed. I'll stop with one further: Ron Westrum's "Social Intelligence about Anomalies: the Case of UFOs". These writings, when useful, tend to be by seriously committed people trying to push forward the research process. When not useful (as with Irving Langmuir's "Pathological Science"), they tend to be by arrogant egos attempting to stop it.

Intelligent "article" writing in collected forms has also happened and is worth keeping on the UFO library shelf. Symposia Proceedings are the common venue for this, and five examples are particularly worth listing:

1. *The Symposium on Unidentified Flying Objects* (1968) held before the U. S. House of Representatives (one of the more readily available ways of reading the thoughts of James McDonald; Hynek, Sagan, Robert Hall, Harder, and Baker were the main speakers);
2. *UFOs- A Scientific Debate* (1972), the Sagan-Page edited papers of the 1969 AAAS convention symposium. Prominent participants included Hynek, Menzel, McDonald, Hartman, Baker, Roach and others;
3. *Proceedings of the 1976 CUFOS Conference* with 35 speakers (including several non-U. S.);
4. *Proceedings of the First International UFO Congress*; twenty-one entries from the meeting sponsored in 1977 by FATE magazine (despite the attitude toward anomalies typified by the sponsor, the great majority of the presentations were properly academic);
5. *The Spectrum of UFO Research*, the 1988 compilation of the papers from the second CUFOS conference of 1981 containing several classic papers in the field.

Two further symposia proceedings are appearing as this is written, and will be worth reading: the TREAT II proceedings from the abductions research meeting at Virginia Polytechnic Institute, and the Abductions Study Conference held at MIT. The former has just appeared (late 1992) and includes nineteen offerings (including John Wilson, Kenneth Ring, and Eddie Bullard). The latter will appear (probably) in 1993 and (again probably) contain works by Mack, Jacobs, David Gotlib, Budd Hopkins, Michael Papagiannis, Mark Rodeghier, Paul Horowitz, David Pritchard et al.

Collections exist for other reasons as well as memorializing conferences, and editors have collected works by their colleagues for our benefit in UFOlogy several times. Here are five of those "times" which remain worthwhile:

1. *The Scientific Study of Unidentified Flying Objects*, Daniel Gillmor, editor. This is the practically unreadable but historically vital report of the notorious Condon Project. For the cognoscente it is highly intriguing, but it should be read by no one in isolation. David Saunders' and R.

Roger Harkins' *UFOs? Yes!* is an appropriate antidote to the misrepresentations (even more so are the internal project files, a few of which are deposited at CUFOS);

2. *UFO Phenomena and the Behavioral Scientist*, Richard Haines, editor. A very intelligent group of articles by people like Westrum, Sprinkle, Phillis Fox, and Michael Persinger.
3. *The Humanoids*, Charles Bowen (editor). This and the following two entries are more mixed as regards to scholarship, but contain useful essays.
4. *UFOs 1947-1987*, Hilary Evans and John Spencer, editors. A modern tour-de-force of the field from an international perspective. This international perspective is vitally important for U. S. researchers in danger of becoming too mired in peculiarities of American pop culture;
5. *Phenomenon*, John Spencer and Hilary Evans, editors. Similar sort of book as #4. It is almost as if Spencer and Evans had a bunch of essays and topics left over, flipped their names, and edited a second book.

A new collection due to emerge soon, will be a collection of exceptional multiple abduction cases, edited by Dick Haines.

On to "general UFO books"... here, finally, is the category wherein interested outsiders expect to find their salvation to the problem of UFO ignorance. As we have seen, the common problem of dependency on the individual author is especially acute in the field of UFOlogy, so that anything I recommend here must be viewed in that light. There are, however, a limited set of such books which might suffice enlighten the reader if read as a group with full crap detectors up and working.

The Top Ten: (in alphabetical order)

1. Bullard, T. E. *Abductions: The Measure of a Mystery* (the deepest collection and analysis of CEIV's available);
2. Hopkins, Budd *Missing Time* (wherein the pattern of the modern abduction scenario was first [partially] revealed. If one has time which is not missing, one should also read the follow-up, *Intruders*);
3. Hynek, J. Allen *The UFO Experience: a Scientific Inquiry* (the defining volume of scientific UFO research);
4. Jacobs, David *Secret Life* (the attempt to micro-describe the pattern of CEIV activities as originally sketched out by Budd Hopkins);
5. Keyhoe, Donald *The Flying Saucers are Real*, and *Flying Saucers from Outer Space* (the best book sources to recapture the atmosphere of the 1950's UFO community);
6. Menzel, Donald *Flying Saucers*, and (Menzel and Boyd) *The World of Flying Saucers* (the most scholarly of the skeptical books, yet the easiest to see through);
7. Michel, Aime *The Truth About Flying Saucers* and *Flying Saucers and the Straight Line Mystery* (the best for internationalizing your consciousness about early UFOlogy);

8. Persinger, Michael and G. LaFreniere *Space Time Transients and Unusual Events* (from which comes all the hullabaloo about the Tectonic Stress Theory and electromagnetic field effects on the observer). To maintain full awareness of Dr. Persinger's evolving thought and research, one should monitor his ongoing series of papers in *Perceptual and Motor Skills*.
9. Ruppelt, Edward *The Report on Unidentified Flying Objects* (the eternally fascinating inside view of the USAF secret project);
10. Vallee, Jacques *The Anatomy of a Phenomenon* and *Challenge to Science, the UFO Enigma* (the lasting contribution of the early Vallee, which with Hynek's volume brought many academics into the field).

Note that several other highly suggested works have been previously mentioned in this review (perhaps most importantly for the "book reader", Jacobs' History). After one has assimilated the above, here are another dozen:

- 1a. Adamski, George (and Desmond Leslie) *Flying Saucers have Landed* (although obviously a hoax, no one claiming to understand UFOlogy should be ignorant of Adamski's tale and message, and how he ruled Flying Saucerdom in the '50's and '60's);
- 2a. Barker, Gray *They Knew Too Much About Flying Saucers* (the real beginnings of the conspiratorial-paranoia element in UFO work written around a real but still-unexplained early incident);
- 3a. Hall, Richard *Uninvited Guests* (a good modern review of the field);
- 4a. Jung, C. G. *Flying Saucers* (the origins of the UFOs as Archetypes of human hopes and fears theory);
- 5a. Klass, Philip *UFOs Identified* and *UFOs Explained* (the first two of Klass' books are probably sufficient to get the flavor of his mental approach to cases, and to judge the validity of the debunker view point. Some would say that *UFO Abductions, A Dangerous Game* should be included as well, but I disagree. To comment upon abductions research one should be knowledgeable either in case research or in some relevant established field – probably both. This author is neither. CEIV research has been best critiqued by responsible members within the UFO community itself);
- 6a. Lorenzon, Coral *The Great Flying Saucer Hoax* (the best book to appreciate the work and views of APRO and its worldwide civilian network);
- 7a. McCampbell, James *UFOlogy* (a professional engineer takes a technical look at the phenomena);
- 8a. Maney, Charles and Richard Hall *The Challenge of Unidentified Flying Objects* (intelligent early work by a physicist and a NICAP insider);
- 9a. Randles, Jenny *UFO Reality* and (with Peter Warrington) *Science and UFOs* (the best two works by the prolific British expert);
- 10a. Ring, Kenneth *The Omega Project* (wherein Sociologist Ring tests the hypothesis that experiencers of certain spectacularly anomalous phenomena may have measurable mental traits in common);

- 11a. Steiger, Brad *Project Blue Book* (although it pains me to even write the name down of such a "who cares about truth?" writer, this is the exception to the rule of no confidence. Here is a reasonable, descriptive review of the USAF Project);
- 12a. Vallee, Jacques *Messengers of Deception* and *Passport to Magonia* (the two "middle years" works which encouraged much of the speculation vis-a-vis conspiratorial paranoia and New Age Parallel Reality opinions, respectively... two emotional elements of UFOlogy still very much with us today).

Such is a short list idiosyncratically but not arbitrarily picked, which, if read, would make one much more knowledgeable but not necessarily wiser about what has gone on in UFOlogy. Some of these writers, through their lenses, try to bring you closer to the data. At this, in my opinion, Hynek and the early Vallee are the best. Some of these writers do not at all look closely at the primary data. They are off hoaxing, gossiping, speculating or debunking. However, to obtain a rich enough understanding of the data, one needs to be wise to the "Sociology" and lens discolorations which have surrounded it.

"Strange Stuff": UFOlogy is like flypaper for the bizarre. Weirdness accretes almost instantly to mainline UFO research and causes endless problems in public relations with the academic community. The typical serious UFOlogist attempts to strip the field of all other extraordinary claims in the hopes of leading a more peaceful intellectual life. "Multiplying one's anomalies" multiplies one's troubles with one's peers. Because the removal of "other" strangeness proceeds on emotional grounds, it may or may not be proper; i.e. it may not be "other" at all. Nevertheless more UFOlogists than not relegate several forms of strangeness to the unadvertised "grey-basket", at least in public.

The contents of the excised "grey basket" include among other things: cattle mutilations, crop circles, Bigfoot, the Face on Mars, Ancient Astronauts, Mothman, Blessed Virgin Mary apparitions, the Bermuda Triangle, Atlantis, Tunguska, Leylines, Hollow Earth, Angels/Demons, Messianic messages, Channeling, Reincarnations on other planets, Perpetual Motion machines, Psychotronics and Tesla machines, Dowsing, and the tenets of the Nevada School of Total Paranoia (this latter label will suffice to designate a loosely associated "school" of "thinkers" (?) who believe in a long-consummated dirty deal between the government and the UFOnuts, which has gone horribly wrong and has involved Kennedy assassinations, world-wide drug control, AIDS invention, and the using of homo sapiens as the main ingredient in the Extraterrestrial cook book). Some of these excisions are obviously proper; some may not be. All are defensive in nature.

The point of this as regards the UFO research library is that all these things have been written about and often in connection with UFOs. If only to protect oneself, the UFOlogist needs to be aware of the best thinking on the subjects. Life is short; time is in demand, and it is a rare person who has the freedom or inclination to become smart on all these issues. When we decide to not make

the effort, we should probably just shut up about them – even the seemingly craziest. However it is in many of these areas that the popular journals (IUR and MUFON J.) do their best services. Occasionally a rational individual will decide to review and critique one of these alleged UFO connections, and the rest of us can become a little smarter. Occasionally an article even appears in CSICOP's *Skeptical Inquirer*, which I monitor continually in hopes of locating the odd sensible thing which appears there.

The other point that should be made is that few serious UFOlogists believe that there is any good data for UFO connections to any of the above-named concepts (if, indeed, there is any good data for them simply on their own.) It may surprise you that major UFO researchers know that cattle mutes and crop circles exist, but are 90+% convinced that UFOs have nothing to do with them. "We" have no belief in the mysteriousness of the Bermuda Triangle, and are aware that the Tunguska Event was a carbonaceous comet. The Face on Mars is, as of now, just an interesting figure on a NASA picture, which should be explored without prejudice in the future. Ancient astronauts made none of the great buildings of our high cultures of the past; whether they inspired any of our legends is interesting but unsupported speculation. Atlantis didn't exist as an ancient hi-tech golden world, and we weren't there in past lives. Dowsing may or may not find water and broken electrical connections, but its relationship to UFOs seems to be zero. And Jesus and the Buddha surely do not live on Venus.

UFOlogists continually have to protect themselves from bizarreness right across the "seriousness" spectrum. On the one end the *Weekly World News* may locate Elvis living with Allen Hynek on Pluto, but on the other Louis Farrakhan of the Nation of Islam tells his followers that he receives insight and the backing of UFO weapon power from UFO Motherships in near-Earth orbit. For your sanity I am not recommending that you read any of this stuff unless you are a professional folklorist, a student of cults, or a CIA-employee. Many of us in UFOlogy find our shelves cluttered with this material, because it is there, it is not just fun and games, and it often interferes with our work.

An exception arises when something becomes so big and so real that it is worth spending some time with it. This has been the case with the English Crop Circles. Although there is very poor documentation of circles being related to "flying objects", the resemblance of the simpler ones to alleged UFO landing traces dating back to the Australia "Tully nests" of the '60's, made the connection inevitable. To date (ex. the summer of 1992) crop circle "research" has either been disorganized and undisciplined, or it has been unpublished. Many claims are made, but little documentation or detective-work ever appears. The status of the phenomenon is simply: it exists and no one yet honestly knows who or what is doing it. All options from weird meteorology to weird Britishers and on to weird aliens are still open. Dr. Terence Meaden's *The Circles Effect and Its Mysteries* is amazingly already out-dated, but it at least seems intelligently researched. The best overview seems to be Ralph Noyes'

edited collection *The Crop Circle Enigma*, which allows various views to try to make their cases. A cottage industry of Crop Circle journals has appeared, but whether any of them are particularly trustworthy it is too soon to say. Where is the proper presentation of data? Where are the proper publications of alleged lab tests? The situation almost appears to be mired in the sort of rollicking amateurism that is afraid to really probe into something which would spoil the fun. Until someone gets serious, the UFOlogist merely looks at the pretty pictures and, after smiling at the artistry, sets the whole business aside in the grey basket.

Materials from non-UFO fields: UFOlogy is always bumping into other areas of knowledge and forcing its intelligentsia to learn something about them. My major personal task in the community has been to serve as a sort of "general science educator" for busy researchers who haven't the time to absorb all that stuff themselves. These subjects often turn out to be of lasting influence on UFO thinking, whether by skeptics or proponents. Certain basic points of information regarding Cosmology, Astronomy, Planetology, and the whole of Biological Evolution come up regularly, usually with enough ignorance added in by the speaker to create embarrassment or disgust in the mind of any listener in-the-know. The whole literature of SETI and the "Drake Equation Debate" has thereby insinuated itself into the business of UFOlogy whether anyone wanted it there or not. The research UFOlogist could use some depth familiarity with the material and have it ready on the shelves. Ongoing journals are not much use here, unless you are a fanatic and wealthy enough to subscribe to *ICARUS*. Short of that the *Journal of the British Interplanetary Society* would be nice. Most scholars would settle for a few of the best references in the field. A selection would be:

1. Ashpole, Edward *The Search For Extraterrestrial Intelligence*;
2. Billingham, John (ed.) *Life in the Universe*;
3. Goldsmith, Donald and Tobias Owen *The Search For Life in the Universe*;
4. McDonough, Thomas *Search for Extraterrestrial Intelligence*;
5. MacGowan, Roger and Frederick Ordway *Intelligence in the Universe*;
6. Ridpath, Ian *Worlds Beyond*;
7. Shklovskii, I. S. and Carl Sagan *Intelligent Life in the Universe*;
8. Shneour, E. A. and Eric Ottesen (eds) *Extraterrestrial Life*.

It would be impossible to list readings for all the fields relevant to UFOlogy, but such a list appears here for the SETI material since it has grown together so intimately with the UFO subject. My research requires much deeper references both for SETI and the individual areas of science verging on the parameters in the Drake Equation, and the argument over the Fermi Paradox. Other UFOlogists require supplementary library materials in other areas: hypnosis, parapsychology, psychiatry, human reproduction, genetics, geophysics, meteorology, plant biology... etc. etc. etc. Our inability to create a true academic-style

community with regular and ample fora for information exchange places great pressures on the individual to be a "Renaissance Man". The SSE and JUFOS and the emerging professional communities gathering about the abductions phenomenon may finally change all that.

The field of UFO research has continued to defy being brought into any reasonably-sized intellectual boundaries. As its mysteries resist solution, its boundaries move dynamically with every new claimed characteristic and every new external concept applied. Thus this library tour can never end. But it will end in the same way we finite humans put an end to any path of complex questioning... we arbitrarily draw a line and quit. Now.

Novice or veteran researchers wishing to accrue to themselves outstanding library and research materials should be aware of UFOlogy's finest book and documents source: Arcturus Book Service, 1443 S. E. Port St. Lucie Blvd. Port St. Lucie, FL 34952.

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This list was selected on two grounds: to illustrate "interesting" cases, and, more importantly, to point out the variety of types of "primary" case materials and their difference from the "typical UFO books" occupying collection shelves. The only "normal-appearing" books (Fuller, Randle-Schmitt, Hynek-Imbrogno, and, perhaps, Salisbury) were allowed on the list because they were written by authors close to the primary investigations, concern themselves with a precisely definable incident or set of incidents, and stay close to the less-polished primary investigative documents.

Selected Data - Collections

(Australian Catalogues)... the Australian researchers have done, perhaps, the best job of data-collection and publication of any country's group.

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3. Basterfield, Keith. An In depth review of Australasian UFO related entity reports. *ACUFOS Report D-3*, 1980.
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- Schopick, Andrew. (Untitled manuscript of E-m cases). *CUFOS archives*, c.1965.
- Webb, David and Ted Bloecher. *HUMCAT* (Humanoid-case catalogue). *CUFOS* archives, currently unpublished (being updated by D. Webb).

. . . again, list selected so as to indicate desirability of data-closeness as a check against idiosyncratic interpretations of reported phenomena.

Selected U. S. Government Documents

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. . . other governments' documents can be interesting as well, most prominently the released Australian Air Documents, courtesy of researchers Edgar Jarrold and William Chalker; and the French government project GEPAN documents-studies, which are (unfortunately for linguistic ignoramuses like myself) not in English.

Brief Listing of Important Journals. (English language, as usual).

- APRO Bulletin*. 1952 - 85.
Flying Saucer Review. 1955 - present.
International UFO Reporter. 1976 - present.
Journal of UFO Studies. 1981 - 3. n.s. 1989 - present.
UFO Investigator (NICAP). 1957 - 80.
 And, if you have the means:
 Australian:*ACOS Bulletin*. 1975 - 79.
Australian Flying Causer Bulletin. 1953 - 55.
Journal Australian Center UFO Studies. 1980 - present.
TUFOIC Newsletter. 1969 - 78.
UFORA Newsletter. 1980 - present.
 Brazilian:*The Flying Saucer*. 1956 - 58.
UFO Critical Bulletin. 1957 - 8.
 Canadian:*Canadian UFO Report (Journal UFO)*. 1969 - 81.
Swamp Gas Journal. 1978 - present.
 Great Britain:*Journal of Transient Aerial Phenomena*. 1979 - present.
Magonia. 1979 - present.
Northern UFO News. 1974 - present.
 New Zealand:*Flying Saucers*. 1953 - 59.
 General European: (Italian published): *UPIAR*. 1976 - ?
 Other U.S.A.:*CSI Quarterly Bulletin*. 1952 - 54.
CSI (NY) Newsletter. 1954 - 59.
CRIFO Orbit. 1954 - 57.
Just Cause. 1978 - 80; 1984 - present.
MUFON Journal. 1967 - present.
Saucer news. 1955 - earlier years.
The Saucerian. 1953 - 62.
Saucers. 1953 - 59.
Space Review. 1951 - 53.
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. . . other journals offer occasional items of interest, or course (ex. *Second Look*, *Pursuit*, *Fortean Times*, *Zetetic Scholar*, *INFO Journal*, William Corliss' *Science Frontiers*, and many professional journals. One journal which should be quite valuable but, surprisingly, almost never has any well-researched or otherwise useful material, is the *Skeptical Inquirer*. I recommend its subscription for certain other topics, but the quality of the UFO pieces is usually not worth the time of any knowledgeable researcher).

Proceedings and Essay Collections

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- Hynek, Mimi (editor). *The spectrum of UFO research*. Chicago, IL: CUFOS, 1988. (Proceedings of the 2nd CUFOS conference, 1981).
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- Spencer, John and Hilary Evans (editors). *Phenomenon*. New York, Avon, 1988.
- UFORA. *UFO research in Australia and New Zealand*. Richmond, Aus: Dynamo House, 1992 (a computer disk "book").
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Major Support Materials

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- Gross, Loren. *UFOs: A history* (and other similar titles). (multi-volumed). Fremont, CA, Gross, 1980 - present. These private volumes are being reproduced now by Bob Girard of Arcturus Book Service.
- Haines, Richard. *Observing UFOs*. Chicago, Nelson-Hall, 1980.
- Hendry, Allan. *The UFO handbook*. Garden City, New York, Doubleday, 1979.
- Jacobs, David. *The UFO controversy in America*. Bloomington, Indiana University, 1975.
- Sachs, Margaret. *The UFO encyclopedia*. New York, G. P. Putnam, 1980.
- Story, Ronald (editor). *The encyclopedia of UFOs*. New York, Doubleday, 1980.

Valuable Books, A Selection Calculated to Give an Overview of the Significant Elements of the Field.

The Top Tier:

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- Hopkins, Budd. *Intruders*. New York, Ballantine, 1988.

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- Jacobs, David. *Secret Life*. New York, Simon and Schuster, 1992.
- Keyhoe, Donald. *The Flying saucers are real..* New York, Fawcett, 1950.
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- Shklovskii, I. S. and Carl Sagan. *Intelligent life in the universe*. San Francisco, Holden Day, 1966.
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. . . these materials give the overview of the "debate-discussion", but, as usual, miss the importance of the details. I have appended much more extensive bibliographies on these subjects to three of my papers: "Science and the Extraterrestrial Hypothesis in UFOlogy." *Journal of UFO Studies* n.s. 1, 67 - 102, 1989; "ETI and the Third Option: an exchange." *International UFO Reporter* 12 (4), 22 - 24, 1987; and "Modern Biology and the Extraterrestrial Hypothesis." *MUFON 19th international UFO Symposium Proceedings*. Ver-

sailles, MO, *MUFON*, 50 - 78, 1991. As mentioned, the detailed reading should be taken up in the journals *ICARUS*, *Journal of the British Interplanetary Society*, and *Origins of Life*.

Important Miscellani

General Anomalistics and Crop Circles

- Corliss, William (editor). The handbooks and publications of the Sourcebook Project, especially *Handbook of Unusual Natural Phenomena* (1977); *Lightning, Auroras, Nocturnal Lights and Related Luminous Phenomena* (1982); and *Rare Halos, Mirages, Anomalous Rainbows and Related Electromagnetic Phenomena* (1984). Glen Arm, MD.
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- Krupp, Edward C. *In search of ancient astronomies*. Garden City, New York, Doubleday, 1978.
- Krupp, Edward C. *Echoes of Ancient Skies*. New York, Harper and Row, 1983.
- Kusche, Lawrence. *The Bermuda triangle mystery solved*. New York, Harper and Row, 1975.
- Morrison, Tony. *Pathways to the gods*. New York, Harper and Row, 1978.
- Ramage, Edwin (editor). *Atlantis: fact or fiction?* Bloomington, Indiana, 1978.

Selected Works on the Paranormal

(UFOlogists are constantly confronted by elements in their field which are potentially characterized as "paranormal", yet few researchers actually know anything about the field. Proper research would be to regularly consult journals like *J. American Society of Psychical Research*, *J. of Parapsychology*, *Research in Parapsychology* (the abstracts of the Parapsychological Association conventions), the Princeton PEAR publications, the *Zetetic Scholar* debates, and the *Journal of Scientific Exploration*. Rhea White's *Exceptional Human Experience*, the *Journal of Near-Death Studies*, and Marilyn Ferguson's *Brain/Mind Bulletin* are also potentially relevant. Since this level of time expenditure "outside the field" is possibly too much to expect, a few book selections follow).

- Alvarado, Carlos S. Research on spontaneous out-of-body experiences in Shapin, Betty and Lisette Coly (editors). *Current Trends in Psi Research*. New York, Parapsychology Foundation, 1986.
- Blackmore, Susan. *Beyond the body*. London, Heinemann, 1982.
- Davidson, H. R. E. and W. M. S. Russell (editors). *The folklore of ghosts*. Cambridge, D. S. Brewer, 1982.

- Gackenbush, Jayne and Stephen LaBerge (editors). *Conscious mind, sleeping brain: perspectives on lucid dreaming*. New York, Plenum, 1988.
- Hardy, Christine. *Science and altered states of consciousness*. Paris, Editions de Rocher, 1988.
- Hufford, David. *The terror that comes in the night*. Philadelphia, University of Pennsylvania, 1982.
- Krippner, Stanley (editor). *Advances in parapsychological research* (multi volumes continuing). New York, Plenum, 1977 - present.
- Plank, Robert. *The emotional significance of imaginary beings*. Springfield, IL, Charles Thomas, 1968.
- Rao, K. Ramakrishna (editor). *Case studies in parapsychology*. Jefferson, North Carolina, McFarland, 1986.
- Ring, Kenneth. *Life at Death*. New York, Coward, McCann, and Geoghegan, 1980.
- Rogo, Scott (editor). *Mind beyond the body*. New York, Penguin, 1980.
- Sabom, Michael. *Recollections of death*. New York, Harper and Row, 1982.
- Tyrrell, G. N. M. *Apparitions*. London, Duckworth, 1953.
- Wolman, Benjamin (editor). *Handbook of parapsychology*. New York, Van Nostrand, 1977.
- Zaleski, Carol. *Otherworldjourneys*. New York, Oxford, 1987.
- Selected works on Frontier Science Thinking (for UFOlogists with impoverished vision vis-a-vis the possible)*.
- Bohm, David. The enfolding-unfolding universe. *Re Vision*, (summer) fall 1978), 24 - 51.
- Bohm, David. The implicate order: a new approach to the nature of reality. *Beyond Mechanism* (editor David L. Schindler), New York, University Press, 1986.
- Davies, Paul. *God and the new physics*. New York, Simon and Schuster, 1983.
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- Moms, Michael and Kip Thorne. Worm-holes in space time and their use for interstellar travel. *American Journal of Physics*, 56,395 - 412, 1988.
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- Waldrop, Mitchell. String as a theory of everything. *Science*, 229, 1251- 53, 1985.

. . . and having experienced the above perhaps one could risk reading "dangerous" authors like Fritjof Capra, Nick Herbert, Fred Wolf, and Michael Talbot.

LETTERS TO THE EDITOR

Comments on "Better Blood Through Chemistry"

Epstein and Garlaschelli (1992, henceforth "E&G") present a very plausible hypothesis about the supposed liquefying blood of Saint Januarius – a strong counter argument to the claim that since no conventional explanation suffices to explain the phenomenon it must be anomalous (paranormal or supernatural). They quite convincingly flesh out the bare bones of the hypothesis presented by Garlaschelli, et. al. (1991) in a letter to Nature. There it is proposed that this "blood", and by extension other supposed miraculously liquefying blood relics, are actually thixotropic gels – seemingly solid substances which liquefy upon being shaken.

One part of the hypothesis, however, which I feel can be improved upon, is the part concerning the identity of the hoax's perpetrator. While not absolutely crucial to the overall hypothesis, it 'is' important – the plausibility of the argument rests in part on the plausibility that there would be someone with both the knowledge and the motivation necessary to perpetrate the hoax. Unfortunately, the little "just-so story" (E&G, pp. 238-239) of an alchemist-priest who believes (on analogy with the miraculous conversion of the communion wafer and wine to the body and blood of Christ) that his alchemical creation has been miraculously converted to the blood of his patron saint, is, though possible, a bit too contrived to be completely plausible.

We do not, however, really need such contrivance. Although I am frequently critical of self-proclaimed skeptics who are overly willing to assume venal fraud unnecessarily, this does seem to be a case where venal fraud seems likely. As I understand it, there was a large, active market in relics at the time, and a relic which would visibly produce a miracle on command would have brought a high price. Almost any alchemist discovering the process and with a need to finance his or her further researches (and/or personal comforts) would have been tempted to engage in fraud. The buyer might have been the church itself or a wealthy merchant or noble anxious to curry favor in Heaven (or in the Church) by means of a donation of an appropriate relic.

A more serious question for the hypothesis – to a non-chemist like myself in any case – is the required stability of the hypothesized thixotropic gel. I would be startled at a household jam which remained in its gel state after six centuries and a thixotropic gel would seem to be in a much more critically-balanced state. Adding to my concerns are the reports, mentioned in E&G, of the "blood" either not liquefying or of taking much longer to liquefy at times, without any reported correlation to external factors like the weather. While variability and stability are not in outright contradiction to each other they are a bit difficult to reconcile. Until the issue of long-term stability of thixotropic mixtures – particularly those

produced with relatively crude ingredients and techniques – is resolved, I have to consider the hypothesis as less plausible than could be hoped for.

My last point is a minor one from the viewpoint of the thesis of this article. It is important, however, because of its relevance in general to such attempts at conventional explanations to purported anomalous phenomena.

E&G say (p. 244) in their "Conclusions" section, "The evidence is very strong that the blood of [Saint Januarius], as well as other liquefying miracle blood, is actually a thixotropic gel." This misrepresents and exaggerates E&G's very real accomplishment. Little or no evidence is presented that the blood of Saint Januarius is a thixotropic gel.

What E&G do accomplish is to present a conventional hypothesis which appears consistent with many of the features of the phenomenon. Furthermore, they successfully defend that hypothesis against a number of anticipated challenges. By doing this, they remove the major argument for the phenomenon's anomalousness.

Simple consistency with observations, however, does not constitute evidence for a hypothesis. Frequently when anomalists of various kinds make this same logical error, it is cited by skeptics as evidence of irrationality or pseudo-science.

For a fact to be evidence for a hypothesis it must be more consistent with that hypothesis than with the alternative hypotheses. There are no facts, however, presented in E&G or in Garlaschelli *et al.* which are unambiguously more consistent with the thixotropic gel hypothesis (TGH) than with the alternative anomalistic hypotheses. Rational preference for TGH lies in its greater 'a priori' likelihood, not in its better fit to the available evidence. That TGH may seem to explain the facts "better" or "more reasonably" than anomalistic hypotheses is a reflection of this 'a priori' likelihood rather than a greater consistency with the facts.

Topher Cooper
Digital Equipment Corporation
77 Reed Road (HL02-3/G13)
Hudson, MA 01749

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Reply to Cooper

In his comments on our recent article, Topher Cooper raises several interesting questions that we wish to address:

- 1) *That venal fraud is likely to have been the origin of the blood relic:* This is of course a minor point, and different scenarios can be imagined. While it is certainly possible that deliberate deception (venal fraud) was involved, it seems to us that self-deception is also a very likely hypothesis. This would be similar to when, for example, a red-colored bacterial growth

named *Serratia marcescens* that is known to grow on bread slices, could have contaminated a holy host, giving rise to some of the "bleeding host" miracles. The hypothesis of an alchemist knowing what he was trying to do is a bit stretched in this case. It would be more plausible if he obtained the thixotropic mixture by chance, although he may have been well aware that his mixture yielded a very strange behavior starting from natural compounds and standard (or nearly so) procedures.

It is obviously difficult to determine if the deception was deliberate. An argument against venal fraud can be made based on the role of the church in 14th-century life. The church and more importantly, the beliefs it espoused were inexorably linked to everyday life. Indeed, if it was common belief that a communion host would bleed and scream out if nailed (and thousands of dead Jews could testify to that belief), the scenario of a priest-alchemist believing his concoction to be saintly blood would be more realistic than the perpetrator risking eternal damnation by selling a fake relic. Now if that relic appeared today, we would tend toward the more venal explanation.

On the other hand, there is considerable evidence for deliberate deception with other relics that appeared on the scene in medieval times. Some of them are so blatantly false as to be grotesque: a feather from Angel Gabriel, two corpses of the same saint (the ultimate bilocation?), the finger that Saint Thomas stuck into Christ's ribcage, milk from the Virgin Mary, several Christ's foreskins, the holy sponge, the holy lance, the holy nails, the crown of thorns, and enough pieces of the holy cross to build Noah's Ark (Bentley, 1985 and Cruz, 1984). Obviously, many of these relics were purchased and kept by religious authorities, so we cannot exclude pecuniary incentive from the motives of the author of our "blood. But we do believe that far more important was the significance of relics in terms of prestige for the city or the church that owned them; that is why the bones of Saint Januarius were stolen (after two attempts) by Sicone and brought to Benevento (with the excuse that he was a bishop from that city), and later rediscovered in Montevergine and brought to Naples after a war raid. In any event, we consider the following to be a very realistic scenario:

Saint Januarius was honored as a martyr and patron of Naples for almost a millennium. His skull had always been kept at the Bishop's church, and the burglary of his bones was felt to be a heinous offense to the city of Naples. When Charles of Anjou, an extremely religious individual, became king of Naples, he had the silver busto manufactured to contain the skull, began the building of the Duomo of Naples, and encouraged the presence in his city of many artists from every part of Italy and Europe. In 1389 the first miracle took place and in 1411 the bones were moved back to Naples. Thus, the conditions were perfect for one of those artists/chemists to have access to the Duomo and the relics of Saint Januarius, and to put some of his concoction into a couple of old small bottles

(or even better to add the larger one to the smaller, pre-existing and non-miraculous one). The natural reddish color of the gel made it a perfect candidate for this use. The gel when calcinated gives rise to ferric oxide (more or less hydrated form), a pictorial pigment already known as "caput mortuum", the residue of calcination of ferrous sulfate to make oil of vitriol or sulfuric acid. Everything "smells" like an artist's workshop. The two vials could well be (as it seems) much older, and it is an accepted fact that similar vials were found in catacombs near the graves of early Christians; sometimes they contained blood relics, but more often they contained wine or scented oils according to an older pagan tradition.

In any case such a miracle was sure to please the king, to make the city feel stronger and protected by the Saint, and to give more strength to the request of the Pope to have the bones brought back from Montevergine to Naples. If the author made some money from it, we cannot tell; but the satisfaction for such an accomplishment may have been sufficient.

- 2) *That the required stability of the hypothesized thixotropic gel has not been established:* We would have to wait 600 years to meet that requirement . . . by that time none of us would care. However, the behavior of colloids is well-enough established to extrapolate. Colloidal suspensions (sols or fluid dispersions of small particles in solution that remain suspended) over a century old are known to exist. Some of Faraday's original gold sols, prepared in 1856-1857, are still in existence at the Royal Institution in London (Everett, 1988). The gel state in which the relic spends most of its time is even more stable. While hydrous gels are known to initially age on standing, as we described in the instructions for preparing the simulated blood relic, they would be expected to stabilize when kept in a closed system such as the sealed reliquary. Nevertheless, preparation of these gels is tricky and while their stability is theoretically feasible, it is very difficult to experimentally foresee or control. For example, some of our gels last a few months, while others are more than one year old. It is necessary to experiment with many different starting concentrations and wait a long time to see the results, as well as to investigate the addition of materials (fish glue, etc.) used to stabilize inks and well-known to medieval artists. The aging process involves the growth and agglomeration of the particles which originally formed the gel. The reaction of calcium carbonate with ferric chloride produces a sol of the Graham type which is particularly amenable to thixotropy because of the very small particle size. As long as the reaction is the same, the "crudeness of ingredients and techniques" will be immaterial as long as the colloid is purified. More important than the stability of the gel is how the purification was done in the 14th century. That still needs to be established more than the stability of a thixotropic gel.
- 3) *That little or no evidence is presented that the blood is a thixotropic gel.* Our conclusion is based on the process of elimination. There are five major explanations that have been proposed:

- (a) The blood belongs to Saint Januarius who is liquefying it.
- (b) The blood is being liquefied by the psychokinetic influence of the crowd.
- (c) The blood is a deliberate fraud on the part of the clergy, who arrange to have the miracle "prepared" every time it occurs.
- (d) The blood is a low melting point substance that liquifies when the temperature changes.
- (e) The blood is a thixotropic gel.

Science has been unable to "prove" either "divine intervention" or "macro-PK". That does not exclude their existence. However, in a paper based on science and not faith, we must eliminate (a) and (b). The scenario (c) described by Broch (1992) in which an "American Pharmacist" prepares the concoction for the church is, while not impossible, highly unlikely (Rickard, 1992) for 600 years of liquefactions and is inconsistent with Broch's favorite recipe (ether and spermaceti) (Garlaschelli, 1993). The low-melting point hypothesis (d), which is traceable to Salverte (1826), is scientifically feasible but unlikely to be the only explanation. The blood-liquefaction ceremony is performed at very different room temperatures (May, September, and December) and the thermal transport mechanism needed to raise the temperature has not been adequately explained. This leaves (e), thixotropy. First suggested as an explanation for the miracle in the early 1940s by E. Newton Harvey (Alexander, 1946), it is both scientifically feasible and a best fit to the known behavior of the relic. The behavior of a thixotropic gel of iron oxide correlates well with the known features of the Naples relic such as the color, formation of bubbles, and shiny surface.

Even if we accept the feasibility of the two anomalistic hypotheses (a) and (b), excellent arguments can be made that they do not fit the known behavior of the relic as well as the conventional thixotropic explanation. The relic liquefies in the absence of a crowd, so (b), macro-PK, can be questioned. It is also noteworthy that several of the supposedly liquefying blood relics in Naples failed after some time; some were exchanged with other stranger fluids (St. Patrizia's) (Straniero, 1991), and others were forgotten or are no longer for public display and in any case do not visibly change their state from solid to liquid. And why would a saint liquefy his blood rather than produce manna to feed the poor? Hardly a first-rate saint! Hypothesis (a) shuns common-sense. We have not, as suggested, simply presented and defended a conventional hypothesis for the miracle. When all possible explanations are examined, thixotropy remains the most logical.

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Michael Epstein
National Capital Area Skeptics
Silver Spring, Maryland
U.S.A.

Luigi Garlaschelli
Department of Organic Chemistry
University of Pavia
Pavia, Italy

Super-Psi or Reincarnation?

It did require a bit more than average attention to get through the pages of the sophisticated Braude-Stevenson-Braude exchange of ideas. Neither one proposed a definitive solution. Stevenson does not claim that cases he described are a definitive proof of reincarnation, and Braude claims that the cases suggestive of reincarnation might be, but not necessarily are, explained by the super-psi hypothesis. Though the idea of super-psi is a non-falsifiable hypothesis, it should not be rejected out of hand. It then seems that, as such, it can explain away any and all reincarnation claims. But can it really?

My knowledge of the reincarnation literature is quite fragmentary. I am not sure if a case, similar to the one I am going to hypothesize, has been described. But my general familiarity with the subject justifies the belief that this hypothetical case is not an unrealistic assumption.

Assume that a child is born with two markings, one on the ventral, and one on the dorsal side of his body. At the age when children start to speak, he begins to talk about his previous life. The child remembers that he has been the head of a commercial company, and his life ended when he was shot by a disgruntled company employee. The child remembers many details from the life of his predecessor, so that it becomes possible to establish the identity of this person, who died from a gunshot through his chest. The markings on the child's body correspond to the points of entrance and exit of the bullet from the body of the murdered man.

If the super-psi hypothesis is true, then the question arises whose super-psi was active in this case? Braude writes: "... it (psi) might be driven by our deepest needs . . ." It is the most plausible supposition that psi, being goal oriented, is also need-dependent. While Braude succeeds in proving that super-psi could explain the knowledge of individuals who remember a previous life, he is silent on the need of these individuals to make such claim, the need that would activate their super-psi.

The need to be reincarnated can have only the entity that has once lived, and is no more alive. Consequently, it cannot be the embryo's need. It would be very

difficult to justify the embryo's mothers' need to make the fetus a reincarnate of an unknown somebody. The only one of this trio with a legitimate need to be reincarnated would be the murdered man.

Here then is what looks like a paradox: if the super-psi hypothesis is true, then the reincarnation hypothesis is also true. Thus, the reincarnation hypothesis alone, and reincarnation plus super-psi, both explain the cases of people who remember a previous life. Should we accept the simpler of these two alternative, since hypothetical realities should be kept at a minimum? Or, perhaps both hypotheses are not mutually exclusive, in which case two kinds of reincarnates would be in existence: reincarnates with and without the super-psi interaction?

Alexander Imich
305 West End Ave.
New York, NY 10023

BOOK REVIEWS

The Meaning of Evolution: The Morphological Construction and Ideological Reconstruction of Darwin's Theory, by Robert J. Richards. Chicago: University of Chicago Press, 1992. pp. xv + 205. \$19.95 Hardback.

This is a history of ideas about the relationship between embryological development and the origin of species. If it were no more than this, it would have little interest to readers of this journal who are not biologists. In a carefully unfolded argument, however, Richards shows that Darwin's own views on this relationship have been misread by some modern biologists, probably because they needed Darwin as an identified adherent of their own views on evolution.

Chapter I, "The Natural History of Ideas," is an overture in which Richards introduces the principal themes of his work. In Chapter 2, "Evolution vs Epigenesis in Embryogenesis," he reviews the earliest ideas, of the 17th and 18th centuries, on embryology. He shows that in the 18th century the word *evolution* served to describe both the development of an adult form from early embryonic stages and the emergence of new species from earlier, ancestral ones. This dual denotation persisted well into the middle of the 19th century. Today, the word *evolution* refers, nearly always, to the emergence of new forms of life from previous ones. Richards correctly reminds us that there was much discussion of the evolution of species in the 18th century and in the first half of the 19th century, long before Darwin published his *Origin of Species*. In Chapter 3, "The Theory of Evolutionary Recapitulation in the Context of Transcultural Morphology," Richards reviews the ideas current in the early 19th century that suggested embryological development to be a recapitulation of more primitive or "lower" forms of life. Chapter 4, "Emergence of Evolutionary Theories of Species Change" provides a short review of pre-Darwinian ideas on evolution.

Chapter 5, "Darwin's Embryological Theory of Progressive Evolution," comprises about one third of the entire book. Citing Darwin's publications and his correspondence, Richards shows that Darwin fully accepted the idea that embryological development is a recapitulation of the evolution of species. On page 172 he quotes a passage from Darwin's *Origin of Species* that should leave no doubt on this point. Most modern biologists and historians of science are unaware that Darwin held such views, and the idea of ontogenesis as recapitulation of phylogenesis is generally credited to Haeckel and considered "un-Darwinian." Certainly Haeckel's name became identified with the catchphrase "ontogeny recapitulates phylogeny," but Haeckel popularized an idea that earlier biologists, such as Lorenz Oken and (the younger) Johann Friedrich Meckel, conceived before him. Darwin discussed and endorsed the concept.

Does what Darwin thought about embryology now matter? Perhaps not, but a misreading of his views does matter, and importantly. Richards shows this in his last chapter, "The Meaning of Evolution and the Ideological Uses of History." The dominant view of evolution among modern biologists is that it proceeds by chance variations without direction. They have all but banned the word *teleology* from discourse in biology. Evolution, they affirm, occurs through random events and is going nowhere. Recapitulation, however, entails constraints and suggests directedness in evolution. This concept was congenial to Haeckel, but Haeckel's extreme views on racial superiority seemed to make it plausible for Gould to assign some blame to him for the rise of National Socialism in Germany under Hitler. In the politics of biology, however, one needs significant allies; and because Darwin is the most important figure in the development of the theory of evolution, the modern neo-Darwinians, Richards argues, wish that he thought as they think. They need him on their side in an ideological debate, and so they have read in the Darwinian scripture what they wished it to affirm. Their need for Darwin's endorsement blinded them to what Darwin really did think, which is that embryological development may be evolutionary recapitulation and evolution may be progressive.

Richards does not hesitate to call such modern historians of evolution as Mayr, Gould, and Bowler "ideologues." He is not, however, mean-spirited about this, and he provides an admirable summary of what he means by an ideologue. This deserves quotation at length:

A historical representation will be ideological . . . if the following conditions obtain: first, the historical account employs an interpretative framework or set of assumptions that are covert and neither justified nor argued for in the account; second, the framework or assumptions express the shared values and position of a particular community rather than the idiosyncratic view of the historian; third, the main function of the framework or assumptions is to justify the shared values and position rather than to realize the principal value of recovering the past; and finally, the historian's interpretations and arguments serve chiefly to justify the framework and thus the values. (p.175)

Richard's documentation of Darwin's views seems to me fully to warrant his conclusion. Reading his book led me to ask myself what other misreadings are now being used to suppress dissident views in science; and this is why I recommend the reading of this book to all members of the Society for Scientific Exploration as well as to all scientists who are not yet members. To what extent are ideologues suppressing us? And we must not fail to ask the corollary question: How many of us are ideologues without acknowledging our failing?

Darwin himself once wrote: "Great is the power of steady misrepresentation; but the history of science shows that this power does not long endure." Or does it? The misrepresentation of Darwin that Richards exposes has endured for a century or longer. If biologists are inclined to become ideologues, we need more historians like Richards to correct our biases.

This book is handsomely produced. It contains a useful bibliography and index. Some of the footnotes are long, but many of them are fully as interesting as the text itself. A large number of photographs add to the value and enjoyment of the book. They include portraits of all the important contributors to the development of pre-Darwinian ideas on embryology and evolution. A photograph of Darwin taken, I think, in 1881 (he died in 1882) is wrongly dated to 1884. The caption beneath a portrait of the elder Johann Friedrich Meckel (1724-1774) seems appropriate not for him but for the younger Meckel of the same name, his grandson, who lived from 1781 to 1833. These, however, are the only errors I found.

Ian Stevenson, M.D.

*Division of Personality Studies
Box 152 Health Sciences Center
University of Virginia
Charlottesville, VA 22908*

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Revelations: Alien Contact and Human Deception, by Jacques Vallee, New York: Ballantine Books, 1991, 273 pp., ISBN 0-345-37172-0, \$20.00, (available from The Sourcebook Project, P. O. Box 107, Glen Arm MD 21057).

Vallee, a French-born astronomer and computer scientist, has spent three decades investigating UFO events as well as the social nature of the UFO phenomenon. He was a long-time associate of the late astronomer J. Allen Hynek of Northwestern, serving with him as an investigator for the Condon Committee's examination of UFO sightings during the 1960s (USAF, 1969). The committee concluded that UFO sightings were largely misperceptions of natural events, but both Hynek and Vallee soon became convinced of the reality of the UFO phenomenon, although not necessarily of its extraterrestrial origin. This conviction appears throughout *Revelations* (e.g., pp. 7, 97, 165, 226 and 228). "[UFOs] are," Vallee tells us, "astounding physical anomalies that have the ability to affect the perception of time and space and the consciousness of those who come close to them" (p. 226-27) and are "associated with a form of nonhuman consciousness that manipulates space and time in ways we do not understand" (p. 236). However, as a scientist, he remains quite skeptical about reports of humans being abducted by spaceships, captive humanoids being held at secret locations and similar claims that fascinate and inspire some ufologists. Indeed, Vallee presented a paper at the 1989 SSE meeting cogently ar-

guing against the extraterrestrial origin hypothesis. He approves this *Journal* as "the only refereed publication in the field" (p. 238).

This book's "revelation" is that "some of the most remarkable sightings are actually complex hoaxes that have been carefully engineered for our benefit" (pp. 7-8). Who is the engineer? The U. S. government is his prime suspect; who else has the resources to carry out something like this? Government agencies, he theorizes, have been making "a massive effort" to "study the UFO phenomena," not "to solve it" but to "use it, to manipulate it as a cover for something else" (p. 229; emphasis in original). Vallee occasionally suggests that particular groups such as the Lyndon LaRouche movement (p. 110), "the American extreme right" (p. 234), and even "an Eastern bloc intelligence agency" (p. 115) are co-conspirators in this deception, but these suspicions are not elaborated.

While the author discusses a number of UFO sightings, he gives close attention to three: a reported landing and abduction in Pontoise, France, in 1979, an alleged UFO crash near Woodbridge (joint USAF and RAF) Air Base in England in 1980, and the photographs of glowing lights over Gulf Breeze, Florida, in 1987-88 along with the convergence of six AWOL U.S. soldiers stationed in Europe in that town in 1990. All these, he suggests, were staged events, designed to attract the attention of ufologists. The government acts even more insidiously, Vallee claims, by having its agents or hirelings act as provocateurs in spreading "proof stories" that permeate the ranks of ufologists. The most common stories are (1) that the bodies of dead aliens recovered from the Roswell, New Mexico, crash in 1947 are frozen at Wright-Patterson AFB in Ohio; (2) that a secret group of a dozen top military officers and scientists (known as MJ-12) has been briefing the president about UFOs since 1947; (3) that there is a large underground facility beneath the New Mexico desert where government scientists and extraterrestrials collaborate on experiments; and (4) that the government possesses documentary evidence showing that UFOs come from the planet UMMO which circles the star IUMMA about 15 light-years distant.

These and other stories tantalize ufologists into believing that irrefutable proof of their long-held beliefs is just around the corner and encourage them to further investigatory pursuit of the non-existent proof. Such pursuit, the author argues, distracts them from "the real nature of the UFO phenomenon" (p. 7) and causes them to set "aside research into genuine UFO cases" (p. 97). In fact, Vallee asserts (without elaboration) that when a USAF officer promised a ufologist secret film of disks and bodies of human-like aliens for a TV documentary she was producing and then withdrew it at the last minute, it precipitated a "disintegration of American UFO research" (p. 45). The reader is left to wonder whether such a single-minded and fragile group could conduct fruitful investigation in the absence of government deception.

Soon after reading *Revelations*, I described it a friend. "Why on earth would the government go to all that trouble?" she asked. This is the central and obvi-

ous question implicit in the book and Vallee's few short attempts to answer it are unconvincing. He offers three speculations. Perhaps the government wants to conduct its own investigation of UFOs without interference from often careless and sometimes fanatical amateurs. As noted above, the best way to accomplish this is to turn their attention to false sightings or to plant large chunks of disinformation in their ranks. Or, perhaps the CIA or the Air Force is manufacturing "flying disks as a platform for reconnaissance operations, for intelligence gathering, for counterterrorism" (p. 229). "Making people believe they are observing flying saucers might be a clever ploy" (p. 229) to keep their real uses secret. Last, perhaps the military has developed sophisticated saucer-like devices for psychological warfare. Their appearance over enemy bases or cities will create great confusion thus allowing our troops to capture them more easily. As these devices must be tested occasionally (this is why so many sightings appear near our own military bases), it is in the military's interest to have them passed off as extraterrestrial spacecraft.

Then my friend asked the obvious follow-up question: "Wouldn't you think that, if so many people in the government had been involved in deceiving the public on such a massive scale for nearly half a century, someone would have spilled the beans by now?" This question does not seem to have occurred to Vallee. He discusses only one situation that can even remotely be placed in the "spilled beans" category. In 1984 a ufologist received an anonymous mailing of microfilm of (almost certainly falsified) files in the National Archives showing that MJ-12 had personally briefed President Eisenhower at least twice. This "evidence" has confused and divided ufologists. According to Vallee, the individual he believes generated the false MJ-12 file "confessed that he had willingly allowed himself to be used" by alleged USAF intelligence agents and had "knowingly disseminated disinformation" (p. 47). Perhaps so, but this is a far cry from the Pentagon Papers or Watergate's "Deep Throat".

The author's other evidence of government deception hardly rises above speculative logic. Indeed, Vallee's own secondary investigations rely on unnamed and uncited sources too often to elicit much reader confidence in their reliability. He writes about his "contacts" (p. 229), "my correspondent" (p. 108), what "witnesses" said (p. 229), what his "friends" have told him (p. 150), or his conversation with the "inventor" of small flying disks (p. 161).

Except for UFO junkies, *Revelations* is a tough read. Vallee immediately plunges the reader into the byzantine world of ufology, providing neither an initial overview nor a map of the territory. One mystery quickly follows another with little attempt at linkage. Each has a new (or nearly so) cast of characters. In some cases, knowledge of the personalities of the major players is more crucial to understanding what is going on than is any hard evidence. The narrative frequently alludes to long-running controversies among ufologists, with which the casual reader will be unfamiliar. Also disconcerting is the frequent, apparently pointless discussion of UFO events seemingly unrelated to his the-

sis of government deception (e.g., sightings in Texas in 1897, the disappearance of a U. S. destroyer in 1944, a shadowy English will purporting to make an inheritance available to organizations that attempt to contact extraterrestrials).

Occasionally Vallee gets quite careless. For instance, after giving the declination and right ascension of IUMMA (star of the planet UMMO, noted above) about 15 light years away, he writes, "this would place it squarely in a region of great transparency, near the Galactic North Pole, which is free from hydrogen clouds. *Therefore*, IUMMA should be visible to us as a fifth magnitude star, easily seen with the naked eye" (p. 99; emphasis added). Vallee gives no optical luminosity for the star, however, and it is impossible to conclude that it would be of the fifth magnitude without that information.

This is called the third book (see Vallee, 1988, 1990) of his Alien Contact trilogy (an odd title for books written by one so doubtful of UFO's extraterrestrial origin), but there is little continuity between the three books. In fact, they did not all have the same publisher and it was not until after the second book's appearance that it was even labelled a trilogy. By training and familiarity, Vallee has much to offer in the formidable task of making some sense of the UFO phenomenon, but *Revelations* is in that respect a disappointment.

Bradley C. Canon
University of Kentucky
Lexington, KY 40506-0027

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RESPONSE

Vallee's Response to Canon

The need to carefully separate spurious data from valuable observation is crucial in any science. This is especially true in the development of fragile frontier areas where no consensus yet exists on the corpus of evidence to be analyzed. In writing *Revelations*, my purpose was to call the attention of my

fellow researchers to the fact that several highly visible UFO cases I had studied seemed to be tainted by human manipulation and in some cases by government deception.

When Bradley Canon's friend asked, "why on earth would the government go to all that trouble?" (Canon, 1993) she expressed a puzzlement which is in the mind of every student of the field. It is true that I only have speculations to offer in response. These speculations, however, have received further validation since the book's publication. Revelations made clear not only the security motivations but also the actual chain of command in the case of the "low observable" objects glimpsed in Nevada and elsewhere. Continuing sightings of bizarre objects in Belgium and in Germany and in the vicinity of Edwards Air Force Base seem to fall under the same rubric. In a related development, a sensational pattern in abduction reports involving the Linda Napolitano case in Manhattan and the "Wendy" case of a self-proclaimed hybrid woman in San Francisco has now given us further examples of human manipulation.

As for the contention that people involved in deception "would have spilled the beans a long time ago," it betrays superficial acquaintance with the nature and structure of such operations. To take only one example among a vast and complex field, how much has Mr. Canon's friend found out about the MK-Ultra mind control projects conducted since the fifties? Compensation of the victims of one particular series of CIA-sponsored experiments conducted secretly at Montreal's Allan Memorial Institute by prominent psychiatrist Ewen Cameron between 1950 and 1965 has only been announced last November (Reuters, 1992). In all that time none of the perpetrators came forward voluntarily to talk about it.

It is noteworthy that the settlement in this case arose from a lawsuit in which 55-year old Linda Macdonald, who had been referred to the institute suffering from depression, claimed she was drugged, kept asleep for eighty-six days and given more than one hundred electroshocks. Released after five months, she did not remember her husband, children or the first twenty-six years of her life. Is it necessarily absurd to see a parallel between such outlandish, highly classified projects and some of the more bizarre abduction reports that are now surfacing? Will the reviewer's friend ask "why on earth" two governments conspired to subject this poor woman to psychic driving to wipe her brain clear?

It is indeed regrettable that in a few cases I was not in a position to publish all my sources by name. However I have gone much farther than the general literature of the field in this regard, so much so that two separate groups threatened legal action in an attempt to silence me and to censor some of the information in the book. I remain confident that the text contains enough specific information to encourage further investigation into those incidents that I have exposed as hoaxes or examples of manipulation.

My reference to JSE as "the only refereed publication in the field" is justifiably challenged by the reviewer. The politics of ufology being what they are, I

should have qualified JSE as "the only refereed publication in the field *which is not linked to a particular UFO group.*" I never implied that articles published in other journals (to which I have occasionally collaborated) were necessarily worse or better than those in JSE, and my remarks were not intended as a slight to their editors.

Aside from these misunderstandings, Bradley Canon's review also contains a number of plainly inaccurate statements that demand to be corrected. In particular, neither I nor Dr. Hynek ever served as investigators for the Condon Committee and I am at a loss to imagine where such a notion could have originated. Both Hynek and I were convinced of the reality of the UFO phenomenon long before the Condon committee came into existence, as the reader of my recently-published diaries will plainly realize (Vallee, 1992).

Revelations is indeed the third book in a trilogy. The continuity is obvious since the first volume, *Dimensions* deals with the historical and mythological perspective of the UFO problem, while the second one, *Confrontations* is devoted to a review of new empirical evidence based on my recent field investigations. It is true (but hardly important) that for reasons that had to do with my relationship to various companies the hard cover version of *Dimensions* was brought out by one firm and the subsequent volumes by another. All three works have now been reprinted in mass paperback format by Ballantine, including a revised version of *Revelations* which corrects some inaccuracies and clarifies some of the points that were obscure in the hard cover edition.

I fail to see why the trilogy should not be called *Alien Contact*. As Bradley Canon rightly observes, I believe that UFOs are "associated with a form of nonhuman consciousness." Contrary to a notion that has clouded the ufological debate far too long, such a form of consciousness doesn't have to be extraterrestrial in order to be alien.

Jacques Vallee
1550 California St.
Suite 6L
San Francisco, CA 94109

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SOCIETY FOR SCIENTIFIC EXPLORATION

POSITION PAPER

Scientists in their normal professional activities make many contributions to the understanding and betterment of the world. They often take up problems raised by the public and communicate via the news media whatever new knowledge they may acquire concerning those problems.

There are however a number of topics concerning which the public turns to scientists for enlightenment only to find that the scientific community has slight interest and slender information. Some such topics, referred to as anomalous phenomena, tend to fall outside the mainstream of scientific activity because their purported properties appear to be incompatible with established scientific knowledge. It is our position that scientists should improve their understanding of claims of anomalous phenomena and attempt to find evidence for or against their validity. If it is found that evidence supports the reality of a particular phenomenon, scientists should then work to understand the phenomenon and to disseminate both the evidence and its possible interpretation.

Progress towards an agreed understanding of such topics (beginning with the basic question of their reality) can best be achieved through the normal processes of open publication, debate, and criticism which constitute the lifeblood of science. It is with this conviction that in 1982 the Founding Committee established the Society for Scientific Exploration, formed for the Study of Anomalous Phenomena, that now conducts most of its business simply as the Society for Scientific Exploration. The Society provides a forum for the presentation and discussion of research on these and related topics through regular meetings and through the publication of the *Journal of Scientific Exploration*.

The Society does not intend to endorse the reality or significance of any particular topic. On the other hand, the Society does not regard current scientific knowledge as sacred and immutable, and no subject will be prohibited from discussion or publication simply because it is not now an accepted part of scientific knowledge. We anticipate that members will approach each subject with the conviction that honest evidence deserves honest investigation that is constructively critical and free from prejudice.

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East Albuquerque, NM 87115

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Medical Consultant
Dallas Diagnostic Association
Santa Fe, NM 87501

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Engineering Anomalies Research
School of Engineering & Applied Science
Princeton University
Princeton, NJ 08544

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SIAC
5150 El Camino Real
Suite B-31
Los Altos, CA 94022

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Institute for Advanced Studies
4030 Braker Lane West
Suite 300
Austin, TX 78759

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Waltham, MA 02254

Prof. Ian Stevenson
Dept of Behavioral Medicine
and Psychiatry
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Charlottesville, VA 22908

Dr. Jacques F. Vallee
1550 California St., #7L
San Francisco, CA 94109

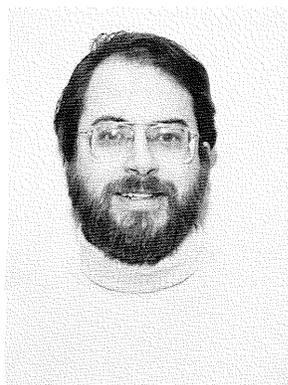
Prof. Jessica Utts
Division of Statistics
University of California
Davis, CA 95616

ANOMALOUS PROPAGATION

by Topher Cooper

There are two ways to slide easily through life: to believe everything or to doubt everything; both ways save us from thinking.

—Alfred Korzybski



The Object - Identified:'

The Object plunged from space and hit the outermost layers of Earth's atmosphere. At first, there was little effect from the tenuous air, but a few seconds later, when The Object reached thicker air, a huge shockwave developed which resisted The Object's further progress. Finally, a few seconds after it first encountered the atmosphere, when it was about 10 kilometers (6 miles) above the surface of the Earth, virtually all The Object's momentum was lost to the air.

Its titanic kinetic energy (10 to 20 megatons' worth) and momentum could not simply disappear, however – it was transferred to the shockwave which continued along the original path of The Object. The shockwave swatted the Earth like a giant hand, knocking about 2,200 square kilometers (900 square miles) of forest flat. The year was 1908 and the place was Tunguska, Siberia.

Although a number of more exotic scenarios – involving mini black holes, anti-matter or exploding UFO engines – have been proposed for the Tunguska event, most scientists now agree that the one above accounts for all the undisputed features of the incident. But what kind of thing was The Object? The traditional answer has been that The Object must have been massive enough to produce the devastation but with a small enough density to have transferred all of its momentum to the air at the necessary height. The only thing which seemed to fit was a very unusual and rare kind of object – a very fluffy cometary nucleus, at least 100 times less dense than the Halley's Comet nucleus.

New calculations by Chyba, Thomas and Zahnle, however, take into account aerodynamic forces which were previously neglected. These calculations show that large meteors are subjected to much stronger stresses than were previously believed. A normal cometary nucleus (much less the giant snowflake that the Tunguska object was hypothesized to be) would be ripped apart in the upper atmosphere. A stony meteor, the most common variety, would be torn apart at just about the correct height, however. The meteor,

though now dispersed into fragments, could still be considered as a single object – but one of much lower density than the parent body. In fact, a stony meteor of about 30 meters in diameter and traveling at 24 kilometers per second would, according to these calculations, explain the Tunguska event just about perfectly. These calculations also solve another puzzle – why are most craters formed by the relatively rare iron meteors rather than the much more common stony ones? The answer is now apparent: because iron meteors are strong enough to remain intact until impact, but the rocky ones produce Tunguska type events, which leave no obvious, long-term geological traces, rather than craters. (Isn't it odd how people outside a specialty rarely hear about these puzzles until a solution is in hand?)

SOURCES: Chyba, C.F., Thomas, P.J., and Zahnle, K.J.; "The 1908 Tunguska explosion: atmospheric disruption of a stony asteroid"; *Nature*, V361, #6407 (7 Jan., 1993), pp 40-44. Melosh, H.J. "Tunguska comes down to Earth"; *Nature*, V361, #6407 (7 Jan., 1993), pp 14-15

Transpacific Reincarnation

When Simon Heh, a Tibetan-American boy who is now five, started making accurate predictions last March, his family started to wonder. Was he perhaps a 'rinpoches' or 'tulkus' – a reincarnation of a holy teacher? When Simon met Geshe Tsepel, a lama who runs a Tibetan religious center in San Diego, he confidently asserted that he knew the 62 year old monk, "You have been my best friend." After Tsepel dreamed about a teacher he had had when he was young, Lobsang Phakpa who had died in the early 1950s, he wondered if the family might not be right. He wrote to his old monastery in India for an opinion, mentioning five different monks that he thought Simon might be (not wishing to influence their decision too much). They responded that they believed that Simon Heh was the true reincarnation of Lobsang Phakpa. This is probably the first time that such a reincarnation has been recognized in the US – traditionally they are only found close to the previous incarnation's monastery. Simon Heh became the lama Sanggyal Dorjee (Buddha Strong) in a religious ceremony on the third of January, and will go to India for training.

SOURCES: AP; January 16, 1993; Contributed by Michael Epstein

Swooning

It's a rather mysterious malady, which sweeps communities without warning and without explanation. The symptoms are dizziness, nausea, diarrhea and fainting. It strikes whole groups at a time – everyone is fine and then, within moments a large percent of the group will be showing symptoms. The victims are mostly teenagers and mostly female. It is unmistakably contagious – but the contagion is frequently via mere mention of another case, so it is clearly at least largely psychological. Health workers usually label such outbreaks as "mass hysteria" but that is a label for their ignorance rather than for their

knowledge. No one knows why these outbreaks occur, why some are affected and others are not, and what causes them to come to an end.

What could be the largest outbreak in history is going on as I write. Hundreds of teenage girls in the delta region of Egypt have been affected. In one incident, 150 girls in a train station were stricken when they heard a rumor that one of the previous victims had died in the hospital. The latest estimate puts the number of victims at around 1500 and it is now spreading south and to adult women. The Egyptian government and media have probably been making matters worse by showing similar levels of "hysteria."

Sources: UPI; April 6, 1993, "Egypt grapples with reported mysterious fainting spells"; Reuters; April 11, 1993, "Fainting epidemic spreads in Egypt"

The Olympia Declaration

The Third International Symposium on Science and Consciousness took place in Ancient Olympia, 4-7 January, 1993. One hundred and four scientists and specialists with multidisciplinary backgrounds from 20 countries participated in a conference whose purpose was to explore the relationship between science and consciousness and to build a bridge between them by means of cooperative dialogue.

One of the outcomes of this Symposium was the 'The Olympia Declaration':

- As every informed and concerned person knows, our planet Earth is undergoing ecological and social crises which may destroy much of humanity and other life within the next few decades.

- We believe it is vital to recognize that the basic problem behind these crises is a lack of understanding of more profound aspects of human consciousness and a dangerous denial of spiritual values. We speak especially of the realization of our fundamental interconnectedness and interdependence and the deep values of cooperation which arise from this realization.

- Therefore we, the participants in the Third International Symposium on Science and Consciousness, call for intense, world-wide efforts by all people, especially scientists, educators and decision makers, to increase our understanding of consciousness and to foster the development of spiritual values in our lives.

- If we do not do so, we are unlikely to survive.

Contributed by: Dr. Charles Tart

Whichever It Was - Don't Look Up

Since Charles Fort's day strange things falling from the sky have been among the staples of those who collect examples of unusual occurrences. In recent years, however, when bad-smelling, blue ice falls from the skies, those in the vicinity are, quite reasonably, more likely to blame faulty airplane toilets than an unknown phenomenon. That was exactly what the residents of

various Chicago suburbs blamed – over 230 of them called the Federal Aviation Administration after their property was bombarded (for the first time) last December third. A series of further incidents followed.

After an apparently careful investigation the FAA announced that the droppings came from sick birds rather than from airplanes. Avian ecologists suggest that geese, who frequently stop at the warm lagoons of local sewage treatment plants, may have been responsible. The geese drink the water and develop diarrhea. Some residents are skeptical – they believe that the quantity of material is much too large to be explained by any reasonable number of geese.

SOURCES: UPI; January 16, 1993; "Geese, not planes, could be cause of 'sky droppings';" UPI; January 23, 1993; "Scientist: sick geese behind sky droppings"

The Conventionality of Unconventional Medicine

We hear a lot about alternative or unconventional medicine. But how many people in the U.S. look to it for help with their medical problems? And who are those people? An ill-educated, and therefore easily duped minority, as some have implied? Are alternate treatments sought in addition to conventional treatments or instead of them? Surprisingly, given the amount of effort expended in "fighting quacks," until recently there have not been any rigorously obtained answers to these and related questions – only assumptions and speculations occasionally backed up by experience of unclear generality.

Now, however, a systematic survey has been done. Over 1500 adults were called at random and were asked about their health care practices – including their use of unconventional therapies. Non-English speakers, people under 18 years old, those with physical or cognitive impairments which might make answering the survey burdensome, and, obviously, those without phones were not included in the survey.

Many people found the results of the survey surprising. Approximately one third of those surveyed had used an unconventional form of medical therapy (defined to be "medical interventions not taught widely at U.S. medical schools or generally available at U.S. hospitals") in the previous year (1990). Of those who had used some form of unconventional treatment, approximately one third had seen an alternative medical practitioner during the period. Among those who had seen an alternative medical practitioner, the average number of visits during the year was 19. The amount of out-of-pocket money (i.e., not reimbursed by insurance) spent nationwide for alternative practitioner fees, commercial diet supplements and over-the-counter megavitamins (the three expenses evaluated in the survey) can be estimated on the basis of this survey to be roughly equal to the amount spent out-of-pocket in the same period for hospital care.

The sixteen alternative therapies specifically asked about in the survey, in order of decreasing popularity, were: relaxation techniques, chiropractic, massage therapy, imagery, spiritual healing, commercial weight-loss programs,

lifestyle diets such as macrobiotics, herbal medicine, megavitamin therapy, self-help groups, energy healing, biofeedback, hypnosis, homeopathy, acupuncture and folk remedies. Only one percent of those surveyed reported using unconventional therapies not on this list.

Unconventional therapies were more likely to be used by people 25 to 49 than either younger or older people; were more likely to be used by the college educated; and were more likely to be used by the affluent. African Americans were less likely to have used unconventional therapies than other racial groups. Neither their sex nor their insurance status made a detectable difference in the rate at which people used unconventional therapies.

Although many in the survey used unconventional therapies for conditions which they did not consult a conventional practitioner for, this was principally restricted to chronic, non-life-threatening conditions. No one in the survey, for example, saw only a non-conventional practitioner for cancer, diabetes, lung problems, skin problems, high blood pressure, urinary tract problems or dental problems.

SOURCE: Eisenberg, D.M.; Kessler, R.C.; Foster, C.; Norlock, F.E.; Calkins, D.R.; and Delbanco, T.L.; "Unconventional Medicine in the United States"; *New England Journal of Medicine*; (Vol. 328, no. 4) January 28, 1993; pp246-252

Suits Him

The outcomes of Uri Geller's libel suits in courts around the world have started to come in. Geller was ordered in March by a Federal Court to pay CSICOP's expenses (\$106,433.97 at the time of the decision) in their defense against a suit Geller had brought against the organization and James Randi. The court felt that CSICOP's inclusion in the case was not proper – that CSICOP was not in any part responsible for Randi's actions.

On the other hand, a Japanese court, though dismissing Geller's \$1.28 million dollar libel suit against Randi, did find that Randi had "exceeded the socially permissible level" and had infringed on Geller's rights. The award was 500,000 yen, a bit over US \$4000. Randi is claiming a moral victory because of the relatively trivial amount, but this does not take into account the rareness of libel suits in Japan and that the court probably considered the finding as a humiliating public rebuke to Randi.

Randi has a better case for a moral victory against Geller in the settlement in two libel cases against newspapers in Budapest. They were ordered to pay Geller about US \$25 each.

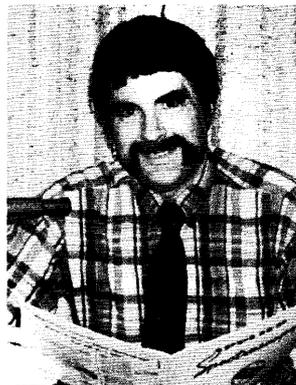
SOURCES: UPI; March 22, 1993; "Tokyo Court orders U.S. magician to compensate for libel"; various electronic mail postings by James Randi to his "Geller Hotline" mailing list. To subscribe, send your electronic mail address to <geller-hotline-request@ssr.com>.

Opinions expressed in this column are those of the author and do not neces-

sarily represent those of JSE. Comments may be directed to the author by electronic mail at "cooper@cadsys.enet.dec.com" or U.S. post at Topher Cooper, Digital Equipment Corporation, 77 Reed Road (HL02-3/G13), Hudson, MA 01749.

THE SKEPTICAL PERSPECTIVE

by Michael Epstein, National Capital Area Skeptics and Jim Lippard, Phoenix Skeptics



I'm pleased to have Jim Lippard, editor of the *Arizona Skeptic*, as a contributor to this issue. Jim is currently completing the requirements for a doctorate in philosophy at the University of Arizona and is active in several skeptical computer forums, which he discusses in this issue.

Local Skeptic Group Activities

Several local skeptic groups are presenting programs allowing for differing views of paranormal phenomena than that of the "average" skeptic. The New York Area Skeptics (NYASk) featured a talk by Ike Cohen entitled "Unsolved Mystery of the Crop Circles." According to the article (Tytell, 1993), Mr. Cohen maintained a scientific perspective and expressed the belief that many crop circles are of non-human origin, although he freely admitted that it is possible that all crop circles were made by people. Three NYASk members spent an evening in the audience of faith healer W.V. Grant with the intent of observing how Grant gets his "healing" information and to interview the "healed." The most exciting part of the evening was an encounter with Grant's security force (Okulewicz, 1993). The January meeting of the National Capital Area Skeptics (NCAS) featured Dr. Stephen E. Braude, professor of philosophy at the University of Maryland Baltimore County. Professor Braude, a past-president of the Parapsychological Association and author of several books on paranormal phenomena, spoke about "Taking Physical Mediums Seriously," in which he presented the case that the studies of Spiritualist mediums at the turn of the century represent some of the best evidence in support of the existence of psychokinesis and psychic phenomena. NCAS also sponsored its first weekend workshop titled "Within Reason: Tools and Applications of Critical Thinking." NCAS members continued to follow up the unsuccessful search for Sir Francis Bacon's vault in Williamsburg, VA, and went on an equally rewarding ghost-hunting trip to Occoquan, VA (Watson, 1992); paid a visit to the Princeton Engineering Anomalies Laboratory (Epstein, 1992); and followed up on

the story of a Stone Age family in the Weekly World News (Tucker, 1992). The Rocky Mountain Skeptics hosted several Chinese visitors to the 1992 Dallas CSICOP conference, and published an issue of their newsletter focusing on the paranormal in China, including Qigong (Zhang, 1992), divination, and Yi Jing. A new skeptic organization, the Rational Examination Association of Lincoln Land (REALL) published its first newsletter surveying activities in Illinois (Bloomberg, 1993) and operates a BBS (217) (787-9101) which carries the Fi-doNet SKEPTIC conference. *The Arizona Skeptic* featured articles on the Travis Walton UFO abduction, "Fire in the Sky" (Rutkowski, 1993) and the Linda Napolitano UFO Abduction Case (Lippard, 1993).

Maybe Yes, Maybe No

Maybe Yes, Maybe No: A Guide for Young Skeptics, a book by Dan Barker is a Prometheus Books publication (1990) intended to arm young children with the sword of critical thinking. Desiring to bring my son up correctly, I recently purchased the book, figuring that it would make good bedtime reading. Well, maybe yes and maybe no! The first part of the book features young skeptic Andrea, who successfully debunks a haunting at a friend's house. When faced with the friend's dogmatic opinion that her investigations still didn't prove "there is no such thing as ghosts," Andrea replies intelligently: "Maybe yes, maybe no." Fine so far. But then the book lists all the things skeptics don't believe in such as ghosts, UFOs, ESP, telepathy, telekinesis, prophecy, out-of-body experience, dowsing, levitation, astrology and horoscopes, and faith healing. Not satisfied with just the secular realm, Barker goes on: "Skeptics do not believe in miracles." Why? "A miracle is something that breaks the rules of nature. Since the laws of nature never change, a miracle is something impossible. Most religions tell miracle stories . . . Some religions teach that you can pray to a god . . ." Let's just hold on a minute here. Is this a guide for young skeptics or young secular humanists? The laws of nature may never change, but our ability to understand them sure does. That is a fine point that the author ignores. Identified as a lecturer and composer, he advises us that "to be a good scientist you have to be very careful to follow certain rules," such as "if something is true, then it must make sense." That certainly makes sense to me. And that the world is flat and the sun revolves around the earth made good biblical and observational sense to people a few centuries ago as well! While this book has plenty of good advice, it has some bad advice too. The verdict is in. Did I like the book? Maybe yes, maybe no!

GEnie PSI-NET

The PSI-NET Round Table consists of a bulletin board area for discussions and a library of downloadable text files on paranormal topics. You can get to this area on GEnie by issuing the command M 1160. Although the bulletin board has not been very active lately, topics of recent discussion have included the Linda Napolitano UFO abduction case (which has some corresponding re-

ports available for download in the file library), ghost sightings and the "Amityville horror," episodes of the TV show "Sightings," and an astrology challenge. (There are a number of PSI-NET participants who are also active in the Astrology bulletin boards, where a discussion of Michel and Franioise Gauquelin's "Mars effect" has been going on. The Astrology Round Table may be reached with the command M 1180.) The PSI-NET area is moderated by Michael Stackpole of the Phoenix Skeptics, co-moderated by Jim Lippard of the Phoenix Skeptics, and has been known to be frequented on occasion by James Randi.

Usenet Sci.Skeptic and Talk.Origins

The Usenet is a worldwide network of computers which communicates with software originally developed for Unix machines, but is now well-connected to the Internet. Well over a thousand news groups exist on a wide variety of subjects and work something like computer bulletin board networks such as Fidonet. The volume of traffic and the number of participants is immense. The sci.skeptic news group regularly logs over a hundred messages a day, and recent topics have included Charles Honorton's ganzfeld experiments, "new physics," remarkable powers of melanin, the proper role of skeptical organizations, Afrocentric theories of the origin of science and philosophy, "biblical astronomy," and many others. The talk.origins news group focuses on the creation/evolution controversies from a variety of angles—scientific, philosophical, and theological—but also occasionally addresses other subjects such as the claims of Velikovsky and his followers. A recent brief participant was University of California, Berkeley law professor Phillip Johnson, author of the book *Darwin on Trial*, but he apparently lacked the time to respond to criticisms of his book and articles. (JL)

Having finally figured out how to access sci.skeptic, I was very pleased to see the quality of discussion as well as the occasional "flame." Highly recommended is a file of "most frequently questioned answers" prepared by Paul Johnson, York Dobyns, Chris Rutkowski, Ken Shirriff, Robert Sheaffer and John Baskette, and concerning skepticism and paranormal claims in general. The hottest topic on sci.skeptic in recent months was the visit of Riley G Matthews (AKA Riley G, Psychic Detective). An alleged "randigram" (i.e., a hostile email message from The Amazing Randi) and subsequently the alleged complete electronic correspondence between Randi and Riley G. resulting from a Randi lecture in New York were posted on sci.skeptic. Riley G., a retired police officer, was recently featured in FATE magazine (1993). (ME)

Skeptical Chemists

Chem Matters magazine is published by the Education Division of the American Chemical Society (ACS) for high school chemistry students, and has a readership conservatively estimated at 150,000. The magazine has re-

cently published several skeptical articles of interest to anomalists, covering topics such as homeopathy (Goldfarb, 1991) (if all the molecules are gone, can the drug take away your headache?), and the miracle blood of Saint Januarius (Meadows, 1993). The unique feature of this magazine is that it includes a detailed classroom laboratory guide, allowing students to reproduce and critically study the phenomena discussed in the articles. For more information, contact the ACS, 1155 Sixteenth Street, NW, Washington, DC 20036.

Last Words

My last words in the previous Skeptical Perspective stirred up a bit of skepticism on the part of a few JSE readers. It seems that there is some disagreement regarding when Galileo said "Eppur si muove!" The reference I had quoted (Brandreth, 1989) suggested it to be a deathbed statement. But to satisfy one "irritated reader and anti-skeptic," who agreed that it was supposedly said by Galileo, but not on his deathbed, I looked it up in another reference (Gamow, 1961). According to this reference: there is a story that immediately after the "confession," Galileo exclaimed "Nevertheless, it moves!", but this is not true, and it only gave ground to an old anecdote according to which Galileo was watching the wagging tail of a friendly dog which entered by mistake into the Holy Office of the Church. Well, maybe Galileo thought it!

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Opinions expressed in this column are those of the authors and do not necessarily represent those of the National Capital Area Skeptics, the Phoenix Skeptics, JSE, or any other organization. Copies of some articles are available on electronic media and others may be available as hard copy. Comments or requests may be directed to the authors at:

Mike Epstein, electronic mail at CompuServe: 76640,1540 or Internet: mse@enh.nist.gov or by U.S. post at Michael Epstein, B-222 Chemistry, NIST, Gaithersburg, MD 20899.

Jim Lippard, electronic mail at Lippard@ccit.arizona.edu or by U.S. post at Jim Lippard, Dept. of Philosophy, University of Arizona, Tucson, AZ 85721.

GUEST COLUMN: NIH CREATES THE OFFICE OF ALTERNATIVE MEDICINE

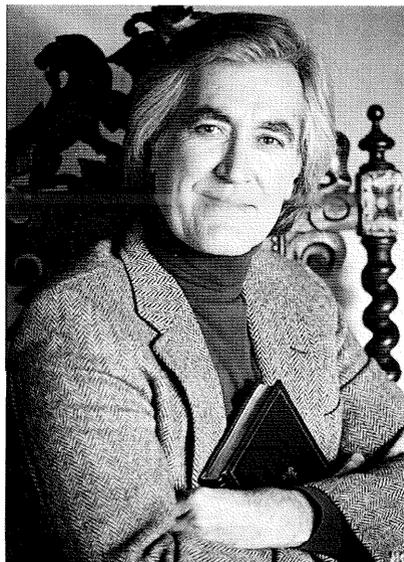
by Larry Dossey, M.D.

Medical Consultant, Dallas Diagnostic Association, Santa Fe., NM 87501

The National Institutes of Health (NIH) has recently created the Office of Alternative Medicine, whose mission is to explore medical therapies that only recently have been considered "fringe" and unorthodox. Several panels have been created within the new office to assess mind/body interventions (psychotherapy, hypnosis, meditation, biofeedback, art, music, dance, therapeutic touch, prayer, and psychic healing), traditional and ethnomedicine (acupuncture, ayur-veda, herbal medicine, homeopathy, Native American approaches, natural products, traditional oriental medicine), structural and energetic therapies (acupressure, chiropractic, massage, reflexology, rolfing), pharmacologic and biological treatments (anti-oxidizing agents, cell treatments, chelation), and electromagnetic applications.

The Office was created not by developments within NIH but by events occurring without. Berkley Bedell, a former Democratic representative from Iowa, now in his seventies, had surgery and radiation for prostate cancer about

Larry Dossey, M.D. is co-chair of the Panel on Mind/Body Intervention, NIH Office of Alternative Medicine, and a member of the Society for Scientific Exploration. Also, he is a physician of internal medicine with the Dallas Diagnostic Association and formerly was chief of staff of Medical City Dallas Hospital. He lectures widely in the U.S. and abroad. He has published numerous articles, and several award-winning books: *Space, Time and Medicine* (1982), *Beyond Illness* (1984) and, most recently, *Meaning and Medicine: A Doctor's Tales of Breakthrough and Healing* (1991). His latest book, *Healing Words: The Power of Prayer and the Practice of Medicine* published by Harper, San Francisco, is due out in October 1993.



four years ago. Two years later the cancer appeared to recur. Bedell did research on his own, tapping into the underground cancer network. He eventually underwent an unorthodox treatment in Canada and believes the therapy controlled his cancer. Bedell, along with Frank Wiewel, executive director of People Against Cancer based in Ortho, Iowa, told their Senator and fellow Iowan Tom Harkin about Bedell's experience and the growing number of Americans who are outraged with the cost, side-effects, inhumaneness, and frequent ineffectiveness of mainstream modern medicine. Harkin, who heads the Senate Appropriations Committee which controls the purse strings of NIH, was impressed. His committee issued this mandate:

The committee is not satisfied that the conventional medical community as symbolized by NIH has fully explored the potential that exists in unconventional medical practices In order to more adequately explore these unconventional medical practices the committee requests that NIH establish an office to fully investigate and validate these practices. The committee further directs that the NIH convene and establish an advisory panel to screen and select the procedures for investigation and to recommend a research program to fully test the most promising unconventional medical practices.

Following a series of 1992 meetings involving experts in unorthodox therapies from all over the United States, the NIH Office of Alternative Medicine is firmly established. A formal report is being drafted describing the aforementioned alternative therapies – the evidence for or against their effectiveness, the promise they may hold, and what further research needs to be done. Since initial funding will be limited, recommendations are also being made about which therapies should be researched first.

The director of the new office is Joe Jacobs, M.D., a Yale-trained pediatrician who first became familiar with American Indian medicine through his mother, a Mohawk, when he was a child, and later in his work as a physician on the Navajo Reservation in the Southwest. "I describe my role as the captain of the Star Ship Enterprise," Dr. Jacobs has said. "We're looking for new things, in a sort of entrepreneurial activity. The purpose of our office is to look at those things on the fringe and give them a rigorous review."

Many persons within orthodox medicine believe the time is long overdue to expand traditional therapies beyond using drugs and surgery. Consider coronary artery disease, the most common cause of death in the United States. Currently there are 6,160,000 living Americans who have either had a heart attack, have angina pectoris (the pain associated with coronary heart disease), or both. This year an additional 1,500,000 will experience a heart attack and approximately 500,000 will die, 60 percent before reaching the hospital for treatment. Even though some 300,000 bypass operations will be performed at a cost of \$30,000 to \$50,000 each, totalling over \$9 billion for this procedure alone, this

hardly scratches the surface of the problem. This operation, as well as the expensive, technologically advanced treatments such as coronary artery angioplasty, thrombolytic therapy, antiarrhythmic drugs, and pacemakers, are all palliative, not curative; none do anything about the underlying atherosclerotic disease that causes the problem. Arguing for an alternative, preventive approach to heart disease, Alexander Leaf, M.D., of Harvard Medical School and Massachusetts General Hospital, states, "Most compelling to me is that the nearly total preoccupation of physicians today with these palliative interventions will do nothing for the next generation of 30-, 40-, or 50-year-olds, dooming them to the same heart disease" (Leaf, 1993).

Several alternative methods for heart disease already exist but simply are neglected. For example, consider excessive blood cholesterol, one of the major risk factors for heart disease. Meditation has been shown capable of lowering blood cholesterol by one-third and is cost-free and virtually free of side effects, a claim no drug can make (Cooper & Aygen, 1978). Yet this approach is almost never recommended by authorities on hypercholesterolemia. Studies have also shown that alternative methods employing diet, exercise, and group therapy can actually reverse coronary artery disease once it is present and dramatically reduce angina pectoris (Ornish, et al., 1990).

Alternative approaches abound in the field of cancer. One promising approach, which has been studied scientifically by David Spiegel, M.D., of Stanford Medical School, and his colleagues involves using group therapy in addition to surgery, chemotherapy, and irradiation. Spiegel, originally skeptical of the role of the mind in cancer, found that women with metastatic breast cancer who engaged in group therapy for one year lived on average twice as long following diagnosis as women treated only with conventional measures (Spiegel, et al., 1989).

Some researchers within NIH fear the new office will siphon research monies from orthodox projects. Currently, because of budgetary tightening, all but the top 14 percent of grant applications from promising young scientists are rejected. It is not likely, however, that the fledgling office will hurt orthodox research. The \$2 million allocated to the new office in 1992 represents only a tiny fraction – a mere 0.019 percent – of the NIH's total budget of \$10.3 billion for the fiscal year 1993.

The creation of the Office of Alternative Medicine has ignited a firestorm of controversy within orthodox medicine. Reaction has ranged from praise to damnation. Dr. Avram Goldstein, a professor emeritus of pharmacology at Stanford University, said of the office, "What's it going to be under? The Department of Astrology? This could help, or it could be a tremendous waste of taxpayer's money."

The National Council Against Health Fraud (NCAHF) has been particularly vitriolic. NCAHF is a private, nonprofit, tax-exempt organization that includes about 2,500 subscribers to their bimonthly publication. It is a self-declared watchdog for medical science and a stern promoter of what it considers

reputable therapies, and it tries to expose any therapy it considers fraudulent. (Within medicine, NCAHF is analogous to CSICOP – the Committee for the Scientific Investigation of Claims of the Paranormal.) Victor Herbert, J.D., of the Mount Sinai School of Medicine and a member of the Board of Directors of the NCAHF, stated that the new office is a "rip-off of the public of \$2 million . . . They are screening garbage looking for diamonds. There are no diamonds in garbage." William Jarvis, Ph.D., of Loma Linda University School of Medicine and president of NCAHF, objects that the new Office was created by pressures from outside the NIH, not by developments from within it. "The real story," he says, "is not that alternative medicine has turned the corner and shown some promise but that there was political meddling by insiders in Washington that created this because of their own naivete." Herbert agreed. Congress funded the Office, he stated, "at the urging of a misguided former Congressman who had been 'treated' by an 'alternative' practitioner" (Schuster, 1992).

The new office also has many supporters from within orthodox medicine who are highly respected. Halstead R. Holman, a professor medicine at the Stanford University School of Medicine, says of the new office, "I think it's a great idea that the NIH is going to do this, and I support them 110 percent. It's unfortunate that a lot of these therapies are viewed as alternative, because it puts a stamp of craziness on things that are potentially valuable." Michael A. Friedman, M.D., of the Cancer Therapy Evaluation Program of the National Cancer Institute, also favors the new office. "One should not approach this with any arrogance and say that some things a priori are worth investigating and other things not," he states. Barric R. Cassileth, Ph.D., consulting professor of community and family medicine at Duke University Medical Center, Durham, N.C., agrees, stating, "I am not an ally of someone who says, 'Let's close the door before we look behind it.'" Cassileth also is unsympathetic with critics' charges that the Office was created by political manipulation. "I doubt there is one institute in Bethesda [at the National Institutes of Health]," she states, "that did not come about at least in part because of political pressure."

The American public seems generally unconcerned with this controversy and continues to pursue alternative methods of health care with unprecedented enthusiasm. David M. Eisenberg, M.D., of Harvard Medical School and Beth Israel Hospital, and his colleagues recently described this current demand in an article, "Unconventional Medicine in the United States," published in the prestigious *The New England Journal of Medicine*. They found that in 1990 about a third of all American adults sought out alternative medicine of some variety. This involved 425 million visits, which exceeded the total number of visits to primary care physicians in the United States. Expenditures for alternative care were a whopping \$14 billion, most of which was out-of-pocket, unreimbursed by insurance plans.

One of the most interesting findings of Eisenberg's study was that over 70 percent of persons seeking alternative care chose not to inform their personal physicians they did so. This may help explain why many allopathic physicians

believe the fuss over alternative measures is a tempest in a teapot; they simply are unaware of what their patients are doing.

Skeptics frequently charge that persons interested in alternative medicine are economically deprived and poorly educated, thus easily misled. The Eisenberg study found the opposite: alternative health care consumers tend to be educated, upper-income whites in the 25-49 age group.

The response of orthodox physicians to these trends seems generally not to be cordial. In an editorial accompanying the Eisenberg article, Edward W. Campion, M.D., stated, "The public's expensive romance with unconventional medicine is cause for our profession to worry." The possibility that alternative methods might be effective seems not to be entertained.

Some advocates of these developments see benefits other than the emergence of specific alternative therapies. They believe these events signal a new appreciation of the art of medicine and a more humane style of medical practice. In many alternative therapies, factors are honored that have become de-emphasized in modern, technological medicine – empathy, compassion, touch, kindness, and the participation of the patient in his or her own healing process.

Major hurdles confront the new Office. Even if validated, how can unorthodox therapies be integrated into mainstream medicine? How are they actually to be researched? Are controlled studies – the kind used to investigate a new drug – suited to evaluate therapies like meditation, diet, and acupuncture? Who should perform the research? Should it be done at NIH by researchers who may be relatively uninterested or actually hostile to alternative approaches, or by sympathetic investigators? How can communication be fostered between alternative practitioners and orthodox physicians and surgeons? Should legal and punitive actions against practitioners of some alternative therapies be relaxed until these therapies have been investigated?

Advocates of certain alternative therapies may not be pleased with the eventual outcome of these developments. Many alternative therapies are relatively poorly researched, and it is likely that some of them will not be proved effective, safe, or cost-effective. Some will fall by the wayside when objectively evaluated. But others will almost certainly be validated and will help change the face of the practice of medicine in the United States. But these changes will not occur overnight. Budgets are tight and funding will be limited. Researching these methods will be time-consuming: skilled investigators must be identified; protocols developed; studies performed, analyzed, and published; and the results made known and circulated within the scientific community.

Certain developments may be of particular interest to readers of the *Journal of Scientific Exploration*. The Panel on Mind/Body Interventions has formally recommended that the Office of Alternative Medicine fund research on the fundamental nature of consciousness – not just its local manifestations in affecting the health of individuals but its nonlocal, distant manifestations as well. This could result in fruitful points of contact between the fields of medicine and parapsychology.

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Dinsdale Prize

As announced in Volume 6 No. 2, Dr. Helmut Schmidt was selected as the first recipient of the Dinsdale Prize of the Society for Scientific Exploration. Following is a short biography of Tim Dinsdale by Henry Bauer and a description of the Dinsdale Prize. Immediately following this is an edited version of the Dinsdale Prize lecture presented by Dr. Helmut Schmidt at the eleventh annual meeting of the Society for Scientific Exploration held at Princeton University, June 11–13, 1992.

Tim Dinsdale

In 1960, Tim Dinsdale filmed, at Loch Ness, an animate thing sticking 3 feet out of the water, 6 feet wide, moving at 10 mph: bigger and faster than eel, fish, or seal. The sensation caused by his film led some prominent and respectable people to found the Loch Ness Investigation Bureau which for 10 years organized the search for the Loch Ness creatures.

Tim, dismayed that the scientific community would take no overt part, felt called to obtain such convincing data that the subject would become fit for scientific study. So he abandoned his career in aeronautical engineering and made a living by selling insurance and by lecturing and writing, so that he could spend the maximum amount of time at Loch Ness. He made more than 50 separate trips, staying for as long as several months at a time, often drifting alone on the water in a small boat.

Through his film and through his dedication, Tim brought many others into the hunt; I've been continually surprised, when I meet other Nessie enthusiasts, to find how many besides myself owe their interest to Tim's involvement. I recall picking up by chance his first book, sneering mentally as I did so, and then taking pause at the reproduced stills from his film. And I read the book, and wondered at his naivety, at apparently expecting people to take him at his extraordinary words. I couldn't decide whether he was massively deluded or incredibly right, and so I had to find out more.

In the early 1970s the underwater flipper photos were obtained, and I became a believer – at least a 90%-of-the-time, 90%-certain believer. This coincided with an intellectuals' recession in the United States, there were no jobs for Ph.D.s and we were getting no graduate students in chemistry. So I decided to branch out, and proposed to the National Science Foundation a study of scientists' attitudes toward the existence of the Loch Ness creatures; a longitudinal study, to capture changes of opinion as the evidence mounted, as I was sure it would. In rejecting the proposal, several of the referees said, to make a study of scientists' attitudes toward anomalous claims, choose a more substantive case, like the Velikovsky Affair.

I'd never heard of that, but decided I'd better find out. And I did, I found out that Velikovsky and his supporters and his critics and just about every commentator on the controversy had ludicrously mistaken ideas about how science really works. So I left chemistry and ventured into anomalistics and science studies.

Of course, I couldn't do that comfortably while holding a position as professor of chemistry, and so I looked for an administrative job to do while I learned my new research trade.

When I interviewed for the deanship of Arts and Sciences at VPI, I was quite worried as the provost accompanied me from one meeting to the next and always introduced me in terms of my other intellectual concerns with Velikovsky and Loch Ness. But he gauged his audiences right, and I went to VPI, and pretty soon thereafter we had established the Center for the Study of Science in Society which now has one of the best-regarded graduate programs in that field here or abroad.

A dozen years ago, telling this tale, I introduced Tim Dinsdale as not only a monster-hunter but also a dean-maker; and a few years later I could have added, progenitor of the Center for the Study of Science in Society. Add to that the influence Tim had on many other people besides myself. Surely there aren't many people who have stimulated so much good in so many ways.

So Tim's memory is worth honoring; but why by this Society?

The Prize

In the mainstream of science, within the scientific community, what is studied and those who carry out the studies are granted a *priori* credibility. That's not so with the sort of anomalous claims that this Society is concerned with. One could argue that the chief problem for us is to acquire enough credibility that the mainstream will even look seriously at the claim.

But credibility can be won only by the people who do the work, because the topics themselves, in the abstract, are by definition incredible. I've discovered for myself that my respect for people I've met through this Society has made me willing to contemplate the possible validity of some anomalous claims which, hitherto and in the abstract, I had judged as not worth entertaining.

Tim Dinsdale's quest for the Loch Ness monsters was and is widely regarded as misguided if not absurd. Yet Dinsdale acted with such straightforward honesty, with such non-combative dignity, that he was held in high regard even by those who thought his work a wild-goose chase. In his public appearances, Tim was always careful to proclaim himself an amateur and not a scientist; yet his behavior met the highest standards of how we expect, or hope, that scientists might behave: disinterestedly, seeking only the truth. When Tim died, he received the uncommon distinction of a respectful obituary in the London *Times*.

So Tim Dinsdale seems to me a worthy model for serious anomalists; as does the first recipient of the Dinsdale Prize.

Henry Bauer

Non-Causality as the Earmark of Psi"

HELMUT SCHMIDT

P. O. Box 296, Mora, NM 87732-0296

The Physicist's Approach to Psi

As a student of physics, one grows up in a climate of optimism. One experiences how Nature can be described in terms of mathematically simple laws which the human mind is able to comprehend. In quantum theory one finds an elegant and powerful tool which—one feels—may well be the final theory for all there is to know on this earth, ranging from atoms to living organisms.

Quantum theory, apart from its practical success, has raised profound and puzzling questions on the nature of physical reality and the role of the human observer, but these questions have remained on the periphery because they seemed of no practical relevance.

Through the results of psi research it became apparent that quantum theory has flaws in a very practical sense. Applied to a systems that include human subjects the predictions of quantum theory were seen to be sometimes incorrect. This finding may well touch upon the open problems of the role of the observer in quantum theory, the nature of reality, and perhaps even on the problem of consciousness.

For our present discussion we will put these profound problems aside and look at psi from the view of an optimistic and practical minded physicist. We will consider the psi effects as a challenge to either modify the quantum formalism or to look for other novel mechanisms.

The Causality Principle

A most striking feature of psi effects is the violation of the causality principle. Causality, in the meaning in which I am using this term, concerns the time order between cause and effect, with the cause always preceding the effect. From early childhood we get used to the principle of causality. If the milk is spilled, somebody must have tipped the cup, and there is no way to undo the accident by changing the past. Causality has become so much an integral part of our common sense feelings that a non-causal world appears almost unthinkable. And even though theorists with little regard for naive common sense feel-

*PrizeLecture: Eleventh Annual Meeting of SSE, Princeton Univ., June 11-13, 1992

ings have occasionally toyed with non-causal models (Wheeler and Feynman, 1945, Schmidt, 1966, and many others), all experimental evidence from the physics laboratory has so far supported the causality principle.

Does Precognition Violate the Causality Principle?

A first hint of non-causality came from reported cases of precognition. If a person can predict some future chance event, then this future event seems to affect the person's present mental state, with the cause producing an earlier effect, acting backward in time. J. B. Rhine brought precognition into the laboratory. He reported that subjects could predict the order in which cards would appear after shuffling.

From the pre-quantum viewpoint of a deterministic world, with the atoms moving like planets in deterministic orbits, precognition might have appeared as a strange ability to mentally calculate the future. Quantum theory, however, told us that some processes are in principle indeterministic and unpredictable. This raised the questions whether some persons could still predict those "in principle unpredictable" events.

In order to generate such events (Schmidt, 1969), I used a combination of a fast computer clock and a Geiger counter to build a random number generator that could produce the numbers 1, 2, 3, and 4 in truly random sequence, so that—according to very basic axioms of quantum theory—nobody should be able to guess the next number with more than the 25% chance accuracy.

As it turned out, selected subjects were still able to predict the numbers to be generated at success rates between 26% and 27%, significantly above the chance success level. This showed that quantum theory—when applied to system that include human subjects—is not always correct. This in itself is certainly a most exciting finding. But does it indicate a causality violation?

Psychokinesis

There seemed to be one way out to still save causality, based on Rhine's reports (Rhine, 1943) on psychokinesis (PK). These reports suggested that some people could mentally affect the outcome of chance events such as dice falls. Subjects in precognition experiments therefore might have succeeded not by looking into the future, but by offering a wild guess and then using PK to make the final outcome match the prediction.

It wasn't easy for me to take the possibility of PK seriously. And when I finally had built some PK test equipment I gave up after a few half-hearted attempts. Only much later could J. B. Rhine persuade me make a systematic effort to find good performers. In the end, success in PK tests seemed not more difficult than success in precognition tests, and the scoring rates in both cases were comparable.

For practical convenience most PK tests use a binary random generator, an "electronic coin-flipper" which the subjects tries mentally to unbalance such as to, say, produce an excess of "heads". In my first experiments (Schmidt,

1971) the binary random generator was automatically activated once per second and the generated heads and tails were displayed by clockwise or counterclockwise motion of a light in a circle of 12 lamps. The subject aimed at having the light move primarily in the clockwise direction.

With the help of digital electronics it was easy to arrange very reliable PK tests so that, I think, even the early tests left no room for error. Later when small computers became available many researchers were able to start reliable experiments and confirm the existence of PK effects. Some researchers, including myself, have tried to work with particularly successful individuals, and some workers, such as Dean Radin and Charles Honorton, have used themselves as the most available and most motivated subjects. Many others, such as Robert Jahn and his group, have worked with largely unselected subjects.

All these approaches have produced significant PK effects but nevertheless we have not yet reached the stage where we can reproduce the effects reliably on demand. This may be partly due to the subtle psychological factors involved in PK success, but there may be also more basic obstacles to reproducibility introduced by the non-causal features of the underlying mechanism.

The Goal Orientation of PK

Imagine a typical PK experiment in which a binary random number generator (RNG) is connected to a display device. The random decisions which the subject tries to affect are made in the RNG, and are displayed shortly afterwards to the subject. Contrary to what one might expect, the subject need not know anything about the random generator, but succeeds with a goal oriented attitude, focusing only on the final outcome shown by the display device rather than on the necessary preliminary steps leading up to this outcome.

The first suggestion that this goal orientation is more than a matter of psychology comes from experiments with different kinds of random generators which indicate that the nature of the random generator, such as its degree of internal complexity, do not affect the PK success rate.

More explicitly emphasized is the goal-oriented feature and its associated element of non-causality by experiments which introduce a time delay between the generation of the random events and the subsequent mental effort of the subjects (Schmidt, 1987).

In such a typical experiment we first activate a random generator many times and store the produced binary random sequence on computer disk or in permanent computer memory. At this time nobody looks at the generated events, but we usually make copies of the data and even computer printouts which are immediately sealed to preclude human observation.

At a later time, usually weeks or months later, we use the stored binary events, the random sequence of "heads" and "tails", to activate some display device while the subject is instructed to aim for an increased number of heads or tails.

The results of these PK experiments have produced highly significant effects, and there is no indication that the time delay makes PK success more or less difficult. If copies of the pre-recorded data were made at the generation time, these copies still agree at the end. In all practical respects it seems that the subject's mental effort during the test session had an effect on the decisions made by the random generator weeks or months earlier: we have a non-causal action of the present into the past in a quite pragmatic sense in that we can experiment with the non-causal effects. This action into the past should not be understood in the sense that my present action would *change* the past. Rather, the past chance events happen as if they could sense what I will do later, and adjust their outcome accordingly.

Psi and Quantum Theory

Since the inception of quantum theory there has been an ongoing controversy on whether or not the theory could adequately describe the human observer himself. The discussion, however, was based mainly on philosophical arguments and on matters of personal taste on what should be considered a satisfactory theory. The mentioned psi experiments provide, I think, the first clear experimental evidence that quantum theory is not quite correct when applied to a system that includes a human subject.

The relevance of psi effects for quantum theory is emphasized by the finding that the effects we observe in the laboratory act only on chance processes. Formulated more specifically, this led to idea that PK violates conventional physics only in a weak sense: It leaves the conservation laws for energy, momentum, symmetry etc. untouched and only affects the outcome of not pre-determined chance processes. Thus PK can affect only outcomes about which the physicist is not certain, and it does nothing that is impossible according to conventional physics; it merely changes the probabilities for possible outcomes.

This weak violation hypothesis links psi in a quite specific manner to quantum theory and implies already some non-causal element. To see this remember that quantum theory can lead to quantum correlations between happenings at distant locations. For example, one can generate a correlated pair of photons flying in opposite directions, so that both photons are polarized in the same direction while nature has no yet decided which this direction is. When one photon at A meets a polarizer it is a matter of chance whether the photon passes or gets reflected. Due to the quantum correlation, the other photon at B meeting a similar polarizer must make exactly the same decision as if it knew which decision the photon at A had made. While this quantum correlation is puzzling and a topic of much discussion we cannot talk about a "telepathic" link between A and B because the coupling cannot be used to transmit information from A to B. We can only passively watch and see how the two distant photons make identical decisions.

Things change if we assume a PK subject at A able to affect the photon's quantum decision. Imagine a continuous stream of photon pairs. If then the

subject succeeds to have an excess of photons transmitted at A, the same must happen at B. Thus an experimenter at B notices instantly the PK effort made at A: we have an instant telegraph. And if relativity theory is right this should enable us to send signals into the past. I will not go into practical details because the PK experiments with pre-recorded random events provide an easier way to study non-causality experimentally.

While the present formalism of quantum theory is causal and inconsistent with psi, one might try to change the formalism slightly to make room for the observed effects. Physicists had already tried to change quantum theory for other reasons. In an attempt to make quantum theory more intuitively appealing they tried to introduce "hidden variables" which might give a deterministic underpinning to the formalism. As it turns out, such models lead necessarily to non-causal or non-local effects in the sense that changes of a hidden variable at one location may imply an instantaneous change of the corresponding variable at a distant location. In the conventional hidden variable models these non-local effects are not observable because the hidden variable cannot be observed or changed at will. If the brain had some access to the hidden variables, however, one might see this as the source of some basic psi mechanism. Several writers have proposed modifications of quantum theory to accommodate psi (Walker, 1979, Mattuck and Walker, 1997, Schmidt, 1982), but unfortunately all modifications of quantum theory attempted so far appear to me rather clumsy and arbitrary when compared to the beautiful and simple original quantum formalism. As soon as a larger number of clever theorists become aware of the existence of psi and its quite real challenge to current physics, the outlook may brighten.

Non-Causal Lawfulness

One might be inclined to argue that a retroactive effect of a subject's present mental effort on a previous random event must lead to logical inconsistencies. The situation may remind us of a science fiction scenario where a person can travel back in time and interact with his parents while they were still children. This would lead to problems if the person had a nasty streak and would cause a fatal accident to his father walking to elementary school. Science fiction writers have to avoid or to talk their way out of such interventions which would lead to "impossible" scenarios.

One can counter this argument against retroactive PK by presenting detailed models which are logically self consistent and nevertheless show retroactive or non-causal effects. Indeed, we can see such effects in motion pictures. Suppose that the director has decided that the hero must throw two sixes in a game of dice or accomplish some other unlikely feat. Then the director simply takes many recordings of the scene, repeated over and over, until the desired two sixes have come up. While all recordings made represent possible histories, the director discards all but the lucky one with the two sixes.

This selection procedure leaves us with a film that shows happenings that

are physically possible, but rather unlikely and goal oriented: the appearance of the two sixes sets a goal or a future boundary condition to which the preceding events must lead up to.

One class of non-causal models whose internal consistency becomes easily evident, is obtained by replacing the movie director by some mathematical selection principle. According to quantum theory the future depends on chance, i.e. we have many possible future world histories from which the real one is selected by chance. We could imagine now a law that—like the movie director—introduces a bias towards certain histories that satisfy some teleological criterion so that what happens now is not only determined by the past but also by the future. The resulting history is then clearly self consistent because it was selected from among many physically possible histories.

Speculations in this direction have not been restricted to science fiction writers. Considering the evolution of biological species, for example, biologists have wondered whether the few billion years available were enough to get such marvelous results by mere chance mutations or whether there must have been some additional "master plan" or "life force" to direct the chance mutations. Perhaps further progress in molecular biology may some day lead to an answer.

I would imagine such a possible master plan not in terms of "divine intervention" but in the form of some mathematically simple law. Imagine for example that one could formulate some simple mathematical measure for the "complexity" of a biological system. Then the master plan may specify that such mutations are more likely to occur which, a million years in the future, lead to more complex systems. Such a master plan would change the statistics of mutations, favoring the development of complex biological systems such as human brains. To ask for the "plausible mechanism" for such changes in the mutation rates might miss the point because the master plan could be one of those basic physical laws that cannot be reduced to other more familiar laws of nature.

One kind of goal oriented world history which does not require any novel laws of nature has been discussed under the label of the "anthropic principle". Assume that the universe contains an virtually unlimited number of earth-like planets. Then some of these should by mere chance—no matter how unlikely this occurrence is—have led to the present world with its highly developed brains including myself with my own consciousness and my ability to observe the world around me. That would explain why all observers in the universe see very complex, statistically unlikely life forms, because planets without such life forms do not breed conscious observers. In such systems, however, the unlikely apparently non-random processes that led to the present state and to my existence are a matter of the past; from now on I see nothing unusual happen and the future mutations are ruled exclusively by chance. Such an anthropic principle therefore could not account for the observed psi effects.

From the view-point of the practicing psi researcher, one might see psi as some goal oriented principle that favors histories which will in the future lead

to happiness or some form of satisfaction on the part of the subject or the experimenter. The pre-recorded event turns out as a head rather than a tail because the appearance of a head will—later in the test session—give satisfaction to the subject, or even because it will contribute to a successful experiment which will, in the end, make the experimenter happy. Some researchers think that the result of an experiment can be affected even by the mental attitude of the readers who study the final published report. Such a situation would make it very difficult to reliably control the test conditions, but this is typical for non-causal systems in general.

The Loss of Isolated Systems

Nature has been generally friendly to physicist in allowing them to study fundamental laws on hand of practically "isolated systems". Newton could find the laws of planetary motion because he could study one planet at a time in its orbit around the sun. If the sky had been teeming with planets, all strongly interacting with each other, it might have taken a long time to recognize the simple laws at the bottom of the apparent confusion. Similarly physicist could study elementary particles, one or two at a time, while disregarding the rest of the universe. By preparing a systems in the laboratory one can often provide a desired initial condition and then study what happens next without any unwanted interference from the outside. In a causal world the behavior of the system depends only on the past, on the manner in which we prepared the system.

In the presence of non-causal interactions, however, the present behavior of a system may depend on its future history which is beyond our control. If we prepare two systems identically, they may show different behaviors if they will be exposed to different conditions in the future. Thus the results of seemingly well controlled experiments may appear erratic and irreproducible, unless we fully understand the non-causal mechanism and can control the future of the systems accordingly.

Conclusion

From the view of the optimistic physicist psi should be explainable in terms of some yet unrecognized law of nature applicable to animate and inanimate nature alike. The basic structure of this law should be mathematically simple so that the observed complexities result from a combination of basically simple laws and a practically very complex brain structure. The psi experiments indicate that the new law must be non-causal, which makes the psi effects so intuitively implausible and inconsistent with current physical theory. One might hope to find the origin of this non-causality in some modified form of quantum theory, and the reports on psi effects should provide a mayor stimulus for physicists to review the experimental and conceptual basis of the quantum formalism. Even if the microscopic origin of the non-causality were known, however, there is still the practical questions of the global implications. These

global features may even be studied independent of the microscopic mechanism with the help of phenomenological models in the same way one can study water waves phenomenologically without having to go into the underlying molecular structure of water. Indeed, the development of new phenomenological models or improvement of old ones (Schmidt, 1975) may be the most efficient thing to do at this time, in giving us a better feeling for non-causal systems in general and helping us to design meaningful experiments.

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Towards an Adequate Epistemology for the Scientific Exploration of Consciousness

WILLIS W. HARMAN

Institute of Noetic Sciences, 475 Gate Five Road, Suite 300, Sausalito, CA

Abstract— The scientific exploration of phenomena and experience relating to consciousness (a category which includes many "anomalous" phenomena) has long been hampered by two obstacles. One is that subjective experience does not meet the commonly accepted criteria for data in a scientific analysis, in that it is not public, objective, and replicable. The other is that many consciousness-related phenomena do not appear to fit comfortably into the accepted scientific worldview. Scientists have improvised ways of dealing with these two obstacles, so that for much of practical science (e.g., research on pain) they don't get in the way. Nevertheless, the situation can hardly be considered satisfactory. Two concepts have recently come to light which may help liberate us from this predicament—one new, the other revived from the respected writings of American philosopher William James. The first, based on recent work by Max Velmans, involves a different model of perception; the second, referring back to James' concept of "radical empiricism," proposes a different criterion for admission of scientific data.

Introduction

Many of the most perplexing "anomalous" phenomena involve consciousness* in one way or another. Among the most obvious examples are the class of phenomena known as¹ "meaningful coincidences," referring to two or more events where there appears to be a meaningful connection although there is no physical connection. The term includes C. G. Jung's concept of "synchronicity" and most of the range of the paranormal phenomena such as supposedly "telepathic" communication, seemingly clairvoyant "remote viewing," and a host of historical and anecdotal examples falling into the categories of "miracles" and "psi phenomena."

The scientific exploration of phenomena and experience relating to consciousness has long been hampered by two obstacles. One is that subjective experience does not meet the commonly accepted criteria for data in a scientific analysis, in that it is not public, objective, and replicable. The other is that many consciousness-related phenomena do not appear to fit comfortably into the accepted scientific worldview. (For instance, the common-sense assump-

(This article is based on work-in-progress involving the thinking of a number of scientists and philosophers who are participating in a project on this epistemological question. I wish both to acknowledge their contribution and to accept full responsibility for my interpretation of where our explorations together have brought us. In particular, I would like to acknowledge the contributions to my thinking of Max Velmans, Ron Brady, and Eugene Taylor.)

tion that conscious volition is causal—that my choosing can cause things to happen—conflicts with the scientific assumption that the universe operates according to causal laws and that these laws can be objectively known.)

Scientists have improvised ways of dealing with these two obstacles, so that for much of practical science they don't get in the way. For example, research on the effectiveness of analgesics, such as aspirin, goes on in spite of the fact that pain is a subjective experience; similarly, effective research has been accomplished on topics like imagery, emotions, dreams, etc. which depend for data on subjective self-reports. It is assumed that the conscious will of the experimenter, which may at one level seem causal in that he/she devised the experiment, can in principle be explained in terms of scientific laws, implying determinism (at least in a statistical or quantum-mechanical sense), if the experiment and experimenter are viewed in a more comprehensive framework. "Paranormal" phenomena, in which consciousness-related events appear to contradict both scientific and conventional pictures of reality, are typically explained away on bases on non-replicability, assumed faulty observation, or probable collusion and fraud.

Nevertheless, the situation can hardly be considered satisfactory. "Downward causation," causation-from-consciousness, is mainly unacceptable as a scientific concept in spite of the fact that it is one of the most impressive facts in our practical experience. Psychic phenomena, near-death experiences, insight of a spiritual or mystical nature, have the power to change persons' lives; yet they tend to be explained away or otherwise disposed of when serious scientific investigation is proposed. The attention medical researchers give to the role of consciousness in spontaneous remission of life-threatening illness is curiously meager considering how importantly medical practice might be affected by its thorough understanding.

Two concepts have recently come to light which may help liberate us from this predicament—one new, the other revived from the respected writings of American philosopher William James. The first involves a different model of perception; the second a different criterion for admission of scientific data.

Re-Perceiving Perceiving

Consider the conventional model of perception of an object *O* by a subject *S*, as viewed from the standpoint of an external observer *E*. From *E*'s perspective, the object is public, objective, and observable; *S*'s experience is not. Thus scientific investigation of the object being observed, the physical phenomena involved in perception, or *S*'s neurophysiology, is straightforward. However, scientific research on *S*'s experience in perceiving the object is held to be difficult and questionable.

In a recent paper, Max Velmans² claims this apparent problem is simply the result of a confusion. In fact, *E*'s perception (despite his scientific training) is no less subjective than *S*'s. Research on spatial localization in various sense modalities, perceptual illusions and "virtual reality" has demonstrated that the

world as experienced (the phenomenal world) is a projection based on clues from "out there." Representations of external events are actually formed within the subject's mind, but the mind models the world by projecting experiences to the judged location of the events they represent. With this "reflexive" model of perception, the phenomenal world is a representation in the mind, which only seems to be "out there." Being *part of* consciousness, the phenomenal world cannot be thought of as *separate* from consciousness. The phenomenal world is just a representation; it cannot be the "thing itself."

Standard science assumes that the things it studies are perceptually public. The reflexive model reminds us that the individual's phenomenal world is *private* to each human being. In this model each observation results from the interaction of the observer with the observed; each observation is observer dependent and unique. If the observation is sufficiently repeatable, intersubjectivity can be established by agreement. (For example, scientists experience no difficulty in agreeing upon the characteristics of a rainbow, despite the fact that every observer sees a different rainbow, formed by a different set of raindrops. Similarly, it is easily established that persons who have gazed for a period at a red circle tend to see a green circle after-image.) Intersubjective agreement requires merely that their experiences are sufficiently similar to be taken for "tokens" of the same "type."

Within this model, the phenomena we call "physical" are just a subset of the things we experience. The traditional gulf between first-person and third-person perspectives is narrowed—it is merely the shift of focus of interest from being interested in one's own experience to being interested in the nature of what is observed. The third-person perspective has dominated reductionist science; this in effect *denies* the legitimacy of S's experience while *asserting* the legitimacy of E's experience of S. In the reflexive model the experiences of S and E are equally legitimate. If S's experiences are to form a data base for science, they merely need to be potentially sharable, intersubjectively validated, and in some sense repeatable.

Thus we see that "objective" data and "subjective" experience are not as different as ordinarily assumed. They can be handled in very similar ways. Observed phenomena in natural science are:

- (1) public in the sense that they are "private experiences shared";
- (2) intersubjectively shared rather than "objective"; and
- (3) repeatable in the sense that successive instances are sufficiently similar to be taken for "tokens of the same type."

These same criteria can be applied equally well to observations of an external object or to phenomena usually thought of as "subjective," such as images, dreams, emotions, seeing a UFO, or seeing the ghost of someone recently dead.

These last two examples remind us that we still have a problem with reports of phenomena that don't fit with the usual picture of reality. This second obstacle, which relates to the intractability of the "mind-body problem" and para-

normal phenomena, is the subject of a long-neglected contribution by the American philosopher William James.

The "Radical Empiricism" of William James³

James' work has been honored by intellectual historians, but little noted by empirical scientists. In James' view, every explanation about reality is undergirded by a metaphysical system, whether overtly stated or simply implied. In his concept of "radical empiricism," he proposes to substitute for the prevailing metaphysic of physicalism, "a metaphysics of experience." In so doing, he aims at defining an appropriate epistemology for research on human consciousness.

James' radical empiricism admits data from the senses, and thus includes within its purview the experience of the physical world. But it also encompasses the broad spectrum of inner realities found within the subjective life of the person. In his *Essays in Radical Empiricism* James defines his term thus: "To be radical, an empiricism must neither admit into its constructions any element that is not directly experienced, nor exclude from them any element that *is* directly experienced." Nothing within the totality of human experience is excluded from being a potential topic of scientific investigation; although of course, not all claims may be ultimately verified. How widely James meant this to be applied is indicated by the fact that he was involved for 25 years with exploration of psychic phenomena, spiritism, and religious experience. He did not buy into the doctrines of enthusiasts in these areas, but he insisted that the experiences are appropriate data for a complete science.

In James' view, experiences, ideas, and consequences all contribute to the emerging understanding which comprises science. Persons' phenomenological descriptions may differ from one another, so consensus is always partial. Science's accounting of nature is always incomplete and tentative; scientific models and theories only tell a partial story.

James' arguments were not persuasive enough to stand up against the enormously successful results of the positivistic scientific mind-set of the first half of this century. He died in 1910 before he could articulate the details of his metaphysical system, and few other scientists took the same concern. However, his arguments may fall on more accepting minds these days, when the shortcomings of a strictly positivistic worldview are more widely apparent.

The epistemology of radical empiricism has important implications for the phenomenology of the science-making process, especially the problem of consciousness. As James argued, science and the systems of mathematical laws that allegedly govern causality can hardly have an existence independent of the human mind. Thus *it is a fundamental illusion to think that we can know about the world of matter with a system of science which essentially omits, and seems even to deny, consciousness.*

At the core of his radical empiricism, James resolves the dualism of the mental and the physical by asserting that one should not be subsumed under the

other. Rather, he believed that no external world of objects can exist *except as a function of some consciousness*. This means that there can be no objective science without human consciousness to create it; no world of causal mathematical laws except in so far as they are a product of human thought. When the conscious awareness of the scientist is conditioned by training to look outward only, the present form of science may seem to offer a reasonable worldview. But when consciousness turns back upon itself and attention turns inward, not only is another realm of experience added to the picture, but *a new order to external reality may be seen*. The observer is changed in the process; never again can certainty be placed in the articulation of absolute laws that leave this factor of consciousness disregarded.

New Promise for the Exploration of Consciousness

With these two concepts—(a) recognizing the uniform way of dealing with all experience/data, whether it be "objective" or "subjective," and (b) the criterion of "radical empiricism" to govern the data admitted—the main obstacles to a comprehensive exploration of consciousness would appear to have been removed. The fundamental reason conventional science has been so minimally effective in increasing our understanding of consciousness is that the epistemology of physics, molecular biology, and neuroscience *is not suited to the area of consciousness; a new scientific epistemology is needed*. Cognitive science has thus far been limited in its achievements because it has failed to come to grips with this need.

Traditional scientists embrace an epistemology which argues that reality can be *confirmed* by matching our inner and outer worlds. That is to say, if the theoretical mental construction accurately describes and predicts the perceived outer world, and no instances can be found which falsify that construction, it is assumed to confirm that the constructed picture of reality is correct. However, to explore the experience and phenomena of consciousness, it is necessary to have an epistemology which takes the personal characteristics of the observer into account.

Modern science has, in the main, attempted to control observer bias by excluding the observer. "Scientific findings" were claimed to be independent of the person who does the scientific work. If findings prove to be replicable, the scientific community concludes that they represent a *discovery* about reality rather than a construction by the observer. However, this term "finding" conceals a metaphysical assumption that things can be found which exist independently of observers. As discussed above, shared agreements among similarly trained observer-scientists may lead to pragmatically useful knowledge, but such agreements do not prove that their sense data are objective, i.e., independent of observers.

In considering what kinds of observations could or should be admitted as scientific data, considerable misunderstanding has resulted from the confusing of two meanings of the terms "objective" and "subjective." One meaning of

"objective" is in the sense of unbiased. The other meaning is in the sense of external to the person, as contrasted with "subjective" meaning **inner**. The epistemology we seek is "objective" in the first sense, but includes the subjective in the second sense.

Velmans' concept of experimenter's and subject's observations being of identical kind implies an issue of the degree to which research must be participative. If both "experimenter" and "subject" as described above are in the same position with regard to data—namely that they can only offer up their own private experiencing of their phenomenal worlds, then in any research effort they are collaborators at the same level. In an exploration of consciousness, the persons contributing reports of their experience are partners in every way with the scientific investigator. If they are so considered, this directly challenges the power of the "experimenter" to manipulate or deliberately deceive the "subject," and it also challenges the validity of the conclusions drawn under conditions of manipulation or deception—including a good share of the findings in the social sciences. This issue of the relative powers of the scientific investigator and persons whose experiences may be under scrutiny, is much discussed in the literature on "qualitative methodology" in the social sciences.

This point should not be misunderstood to equate the observation of a naive bystander with that of a highly trained scientist—which Michael Polanyi⁴ has written about using terms like "art" and "connoisseurship." But the difference is a subtle one. We are only recently recognizing the special understanding of nature found with what we once called "primitive" peoples or "savages." Scientists have been slow to recognize the possibility that mystics and trained observers in other cultures such as the Tibetan, which focus more on inner realms of experience, might be capable of observations which are as foreign to Western scientists as the latter's trained observations are to the man in the street.

The Unconscious Mind in Science Construction

The above implies an intrinsic dilemma with regard to the epistemology of science which is too little noted. In order to have confidence in the scientific view of reality, we have to answer the epistemological question: How do we know what we believe we know? To answer that, the mind needs to step outside itself, so to speak, and observe itself at work. Our view of reality is inevitably determined partly by that reality, and partly by the mental processes through which we arrived at the view we have. However, to know about those processes we need already a scientific study of the mind, for which we need a scientific epistemology—which with a frustrating circularity, leads us back to the initial question.

This circularity implies that even if the results of generations of scientific inquiry appear to be convergent toward a particular picture of reality, a profound caution is advisable regarding how much faith is put in that picture. (This point has been made in another way by Thomas Kuhn.⁵) In any case, this dilemma suggests that as we search for the appropriate epistemology for a

comprehensive science, we need to pay particular attention to what is known about unconscious mental processes.

Research on perception, hypnosis, repression, selective attention, mental imagery, sleep and dreams, memory and memory retrieval, acculturation, etc. all suggests that the influence of the unconscious on how we experience ourselves and our environment may be far greater than is typically taken into account. Science itself has never been thoroughly re-assessed in the light of this recently discovered pervasive influence of the unconscious.

The unconscious enters importantly into the construction of science in at least two ways: (a) the creative/intuitive mind (an aspect of the unconscious) is intimately involved with all the important conceptual advances in science; and (b) the contents and processes of the unconscious influence (individually and collectively) perceptions, "rational thinking," openness to challenging evidence, ability to contemplate alternative conceptual frameworks and metaphors, scientific interests and disinterests, scientific judgment—all to an indeterminate extent. What is implied is that we must accept the presence of unconscious processes and contents, not as a minor perturbation, but as *a potentially major factor in the construction of any society's construction of its particular form of science*. (This consideration even puts into question whether or not the logical construction of a science from a rational epistemology may already be a culturally biased approach.)

There is some precedent for taking into account unconscious processes and contents in the training of the researcher. In training to be a psychotherapist, the individual has to go through inner explorations similar to those anticipated in his/her future clients; with these experiences comes learning, and personal change. Similarly, in training to be a cultural anthropologist, the person must learn to experience being of another culture; this too brings personal change. In training for the bench, the future judge—at least ideally—goes through self-examination to uncover personality characteristics which could cause one to be unconsciously biased. In general, learning to be a faithful observer implies inner change. The scientist who would explore the topic of consciousness (in the broad sense we are considering here) must be *willing to risk being transformed* in the process of exploration.

Aiming at a Broad Scope of Inquiry

A word must be said about the tactic of limiting preliminary inquiry to common everyday consciousness, avoiding areas where the reports and the data are problematical, as for example, profound meditative insight or investigation of the paranormal. Would this not be in accord with much of the history of science, where preliminary inquiry has often and profitably been restricted to simple and limited cases?

We are advising the opposite, namely to seek an epistemology suitable for exploring even questionable areas of human experience. One justification for this comes from an analogy with the origin of the evolutionary hypothesis.

There was much to be learned from studying separately the great variety of microorganisms, plants, and animals with which the planet is populated. But Charles Darwin boldly turned his attention to the synthesizing question: How can we understand all of *these together*? The result was the concept of evolution, around which practically all of biology is now organized.

That same question comprises the chief importance of studying the lesser recognized and seemingly anomalous, but still persistently reappearing reports of experiences and phenomena—experiences and phenomena many of which have a face validity stemming from the fact that they have been reported across cultures, and down through the centuries. What sorts of conceptual frameworks and organizing metaphors can be used to help us understand all of *these together*?

A More Careful Definition of the Present Endeavor

The philosophically sophisticated reader will have detected a contradiction in what has already been said. We claim to be seeking an epistemology for the study of consciousness. Since all of our experience is in consciousness, that leaves nothing out. It would appear that we cannot distance ourselves from the most fundamental question of all—the basic epistemological question of how we know anything. Since many generations of philosophers have been unable to agree on the answer to that question, it would seem unlikely that we would succeed where they have failed.

However, our goal is a more modest one. Science has an accepted epistemology, which has proven inadequate to dealing with various aspects of "ordinary" experience including conscious awareness, intuition, creativity, attention, and volition, as well as a wide range of "anomalous" experiences. We seek, not the ultimate epistemology, but an intermediate one, a provisional one—one which will extend the present form of science to include that which it has heretofore excluded. As we learn more, and as scientists become more adept at exploring the vast realms of inner experience, it may well be that this epistemology will itself have to be set aside in favor of a still more adequate one.

The intrinsic dilemma of science was identified above: In order to have confidence in the scientific view of reality, we need an adequate epistemology. But that implies a prior understanding of the mental processes underlying observation, and for that we already need the scientific epistemology. In our present endeavor we are attempting to break into this circle with a provisional answer to the epistemological question. If we can identify a provisional epistemology which avoids the known limitations of the epistemology presently guiding physics, molecular biology, and neuroscience, and which has the capability of furthering the exploration of consciousness, we may be able to give impetus to that research area. What is learned (including the further development of our "organs of perception" in the sense of Goethe and

Rudolf Steiner⁶) may result in the discovery that our provisional epistemology is also limited, so that it eventually must be displaced as well.

Present science constitutes a base camp, so to speak. It competently deals with a certain kind of knowledge—that which aims at prediction and control. However, modern society's mistake was to believe that, with that base camp, the summit was in sight. We now see that a further advance camp—perhaps many—will be needed. Scouting parties have gone ahead, and we know something of what will be encountered. Two very different non-Western epistemologies—that of the inward-looking East and that of the indigenous peoples with their intimate relationship with nature—have much to teach us. The enigmas of what the British called "psychical research" remain to be dealt with. Even though our knowledge from these advance scouts may be fragmentary, we get some clues regarding characteristics of the provisional epistemology.

There is a danger in this approach, to be sure. Just as today's scientists allowed themselves to be deceived by the apparent successes of the objectivist-positivist-reductionist epistemology, so we could be deceived through our attempts to promote a provisional epistemology of consciousness research. What will save us from deception is continual reminding that (a) any science we can share through words, formulas, and images is at best models and metaphors representing certain aspects of experienced reality, and (b) that the best we can do now will undoubtedly seem inadequate as our "organs of perception" are enhanced through personal transformation.

Attributes of an Epistemology of Consciousness

With all of these considerations in mind, we can now propose a number of characteristics of the sought-for epistemology for conducting a comprehensive exploration of consciousness in the broadest sense:

1. The epistemology will aim at being objective in the sense of being open and free from hidden bias, while dealing with both "external" and "internal" (subjective) experience as origins of data.
2. It will be phenomenological or experientialist in a broad sense; that is, it will include subjective experience as primary data, rather than being essentially limited to objective, physical-sense data.
3. It will insist on open inquiry and public (intersubjective) validation of knowledge; at the same time, it will recognize that these goals can only be met incompletely when seeking knowledge that includes deeper understanding of inner experience.
4. It will be holistic in the sense that it attempts to complement reductionistic science (seeking to understand the whole through understanding the parts) with an approach that honors the irreducible characteristics of the whole. In other words, it will not think to explain (or explain away) experiences that are rich in meaning by reducing them to combinations of simpler experiences.

5. It will recognize the partial nature of all scientific concepts of causality, and the complementarity of the usual "third-person view" of ordinary science with a "first-person view" in which causality may appear quite differently. (For example, the apparent causality, in "third-person" science, of a brain state resulting in physiomotor action does not invalidate the subjective feeling of volition in "first-person" science.)
6. It will recognize that science deals with models and metaphors representing certain aspects of experienced reality, and that any model or metaphor is permissible if it appears to be useful in helping to order knowledge, even though it may seem to contradict another model which is also useful. (The classic example is the history of wave and particle models in physics.)
7. It will be participatory in recognizing that understanding comes, not alone from being detached, objective, analytical, coldly clinical, but also from cooperating with or identifying with the observed and experiencing it subjectively. This implies a real partnership between the researcher and the individual or culture being researched; an attitude of "exploring together" and sharing understandings.
8. It will comprise "radical empiricism" (in William James' sense) in the sense that it will address the totality of human experience; no reported phenomena will be written off because they "violate known scientific laws." Furthermore, consciousness is not a "thing" to be studied by an observer who is somehow apart from it; consciousness is at once the observer and the observed, or if you like, the experience of observing.
9. It will involve recognition of the inescapable role of the personal characteristics of the observer, including the processes and contents of the unconscious mind. The corollary follows, that to be a competent investigator, the researcher must be willing to be transformed in the process of exploration.
10. It may well in time have to be replaced by another, still more satisfactory epistemology, for which it has laid the intellectual and experiential foundations.

Reconciling Science and Our Sense of the Spiritual

A comprehensive study of consciousness must inevitably deal in some way with experience of the sort typically termed spiritual or religious. Religion is, generally speaking, the institutionalized concern with these areas of human experience. In terms of their functions in society, religion and science are different enterprises. However, their interests overlap, particularly in three areas:

- In explorations of non-ordinary states of consciousness (e.g. trance and meditative states; near-death and out-of-body experience; mystical awareness), and their implications with regard to the nature of human beings and their relationship to the universe.

- In the study of "meaningful coincidences," (events that seem to be meaningfully connected, although there is no apparent physical connection), particularly as these relate to events reputedly having a spiritual component (as, for example, a person sensing a strong warning, as by a "guardian angel," and discovering later existence of a real danger; or one person intending the healing of another and the latter feeling restorative effects).
- In the study of the origin or evolution of life and consciousness, and the source of our sense of ultimate values and transcendent meanings.

Insofar as they claim to be representing aspects of reality, science and religion should eventually say compatible things about these three areas; if they seem to say contradictory things, the dependability of one or both remains open to question. These three areas comprise one of the most exciting frontiers in all of science, because in their exploration we may at long last resolve the tension between the two kinds of competing worldviews in modern society—the scientific worldview which prevails in our powerful institutions, and the humanistic-spiritual outlook by which we guide our daily lives.

Footnotes

- * We are here using the "consciousness" to connote the totality of conscious and potentially conscious states of mind, not in the limited sense of "conscious awareness" only.
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Puzzling Eminence Effects Might Make Good Sense

SUITBERT ERTEL

Institute of Psychology, Georg-August-University, Gosslerstrasse 14, 3400 Göttingen, Germany.

Abstract—According to Gauquelin's eminence hypothesis, planetary effects increase with increasing professional renown. The author's former findings, however, did not always support this hypothesis. In some cases planetary effects went down, or first up and then down, with increasing eminence. Müller's recent unexpected results with very eminent professionals, which showed a considerable weakening of planetary effects instead of an amplification, gave rise to the hypothesis that the relationship of planetary effects to eminence might be curvilinear, instead of linear, across all planets and professions. Thus previous results suggesting linear relationships might have been due to restricted eminence sampling. By extending the analyses to athletes (olympic medallists), scientists, and actors covering a wider range of eminence, marked curvilinear patterns did in fact emerge.

Among world-wide research on astrological claims, Michel Gauquelin's work, has been called a golden grain in a heap of dust—Hans Eysenck's metaphor (Eysenck & Nias, 1982), and an erratic block rolled on the road of science—Arno Müller's metaphor (Müller, 1990). Both ways of putting it refer to an apparent anomaly calling for scientific scrutiny. Will Müller's most recent negative results on eminent people of his own collection (Müller, 1992a, partly published 1992b) eventually wash off the golden color from the grain and reduce the block to nothing? Gauquelin's eminence hypothesis is at issue here. I will give an account of its status.

In his first book published in 1955, Gauquelin referred to the incidental observation "that a certain degree of [professional] success was necessary for the planetary effects to be visible." (Gauquelin, 1983, p. 28).

In his second book published in 1960, he repeatedly tested this connection by comparing famous with less famous samples (sports champions, p.89; actors, p.118; politicians, p.109; painters, p.122; scientists, p.76, Gauquelin, 1960). He reported consistent differences of planetary effects in each case.

He then generalized the findings by introducing the eminence doctrine: "The greater the heights reached by an individual in his chosen profession, the more likely he is to have been born in 'planetary conformity' with his peers" (Gauquelin, 1973, quoted in Gauquelin, 1988, p. 39).

The evidence for Gauquelin's generalization was based, however, on a crude dichotomous classification into famous and not famous, which of course does not account for the entire range of eminence levels that might actually be

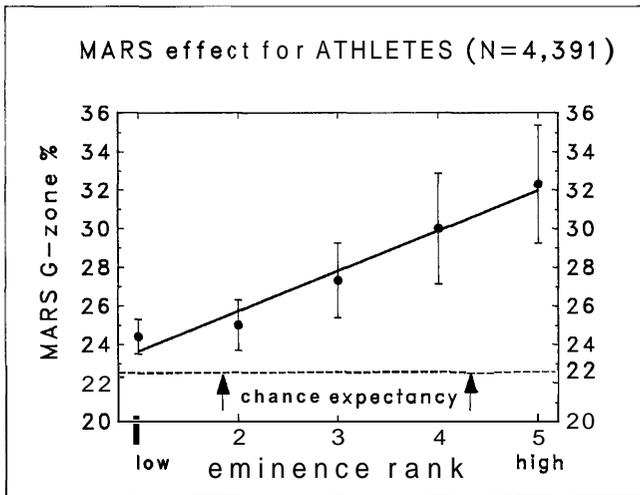


Fig. 1. For athletes ($N = 4,391$) positive deviations of Mars G-zone proportions increase with eminence.

distinguished. Moreover, Gauquelin's breaking down of his professional samples into high versus low eminence tended to be based on flimsy criteria. So I came to a challenging conclusion: If differences in planetary proportions between samples of high versus low eminence emerged by applying flimsy criteria, they should become more conspicuous by testing them with more refined criteria. If Gauquelin's lemon was genuine, an improved way of squeezing it should yield more juice. If further juice would not come out, Gauquelin's lemon would be dubious.

Eminence scaling requires appropriate reference books, biographical dictionaries, Who's Who's and the like, a great number of them, and the counting of citations. I started out with athletes. The names of 4,391 Gauquelin athletes were looked up, and each citation increased his or her eminence score by 1. With 18 reference sources the highest citation count obtainable was therefore 18.

The wide range of eminence *raw scores* was then replaced by a smaller number of eminence *ranks*. As higher raw scores are rare compared to lower raw scores, higher ranks should cover a wider range of raw scores than lower ranks to avoid an imbalanced distribution. Further technicalities have been minutely described in an earlier article (Ertel, 1988). For the sources of data in the present study, see appendix.

Did Gauquelin's lemon yield more juice when squeezed with an improved device? Yes, it did (Ertel, 1988, p. 68ff); see Figure 1. Here are five eminence ranks, and for each rank is given the percentage of athletes born with Mars in a G-zone (G-zone or Gauquelin zone refers to sensitive key sectors at the horizon or meridian). The results show that the more eminent the athletes the greater the proportion of Mars-born individuals among them.

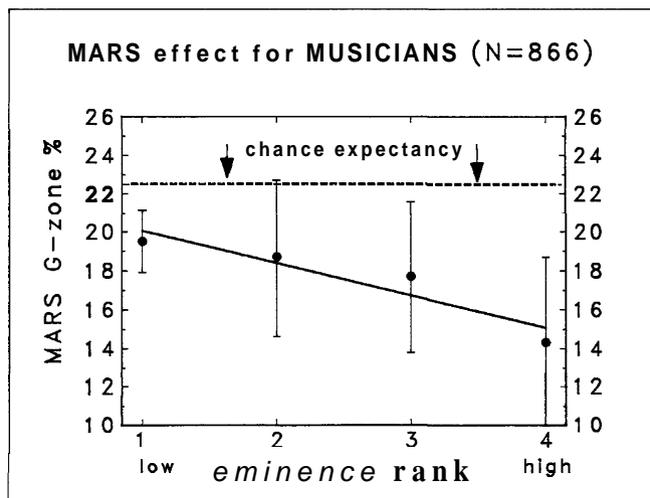


Fig. 2. For musicians (French only, N = 866) negative deviations of Mars G-zone proportions increase with eminence.

Figure 2 shows Mars G-zone proportions for musicians (from Ertel, 1987). Its slope here is reversed for the following reason. Planetary effects may be positive, e.g., *more* future *athletes* are born with Mars in G-zones than expected by chance, and they may also be negative, e.g., *fewer* future *musicians* are born with Mars in G-zones than expected by chance. In short: Mars in G-zones at birth is "preferred" by athletes, but "avoided" by musicians. If planetary effects increase with eminence, the slope should rise in the first case and drop in the second, departing from the chance line progressively in both cases. The slope in Figure 2 is thus totally predictable from Gauquelin's eminence hypothesis.

The slope in Figure 3, however, representing proportions of *scientists* born with *Saturn* in G-zones (see Ertel, 1989) could never have been predicted by Gauquelin's hypothesis. The *overall* Saturn effect for scientists is clearly positive, but the eminence curve slants downwards instead of upwards. Thus, Gauquelin's hypothesis, in its original form at least, does not hold.

In what follows I will revise Gauquelin's eminence hypothesis, and try to restore, eventually, consistency among eminence observations which presently might appear rather contradictory.

A closer look at Figure 3 shows that it does not represent all possible eminence levels. With scientists at eminence rank one, the proportion of *Saturn*-born individuals is far above the chance level shown by the general population. The high level of rank one scientists must therefore decline to the low level of the general population. It is not unreasonable, therefore, to postulate the existence of scientists of still less eminence ranging between rank one and the general population. As a matter of fact, rank one on our eminence scale is quite high—such scientists must have had obtained an entry in at least one ref-

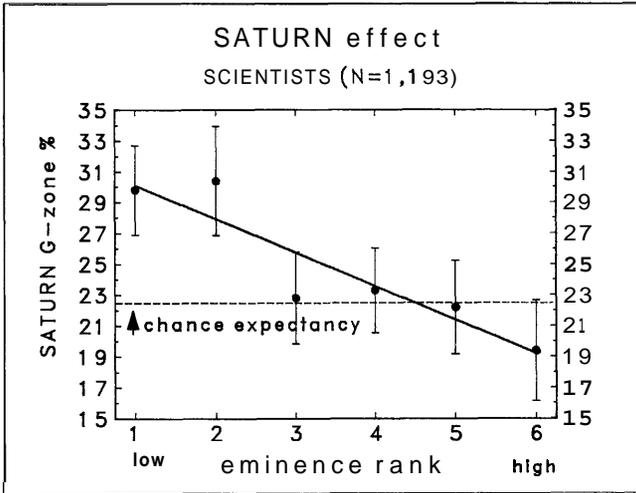


Fig. 3. For scientists ($N = 1,193$) positive deviations of Saturn G-zone proportions decrease with eminence.

erence book, and perhaps the majority of scientists would not qualify. So it is reasonable to assume that if lower-than-rank-one scientists had been included in this study they would most probably have provided the missing left portion of this curve. The curve would then take on a curvilinear shape, that is, an inverted U as shown in Figure 4 in which has been added, on the extreme left of the scale, Saturn G-proportion for ordinary people.

To summarize the various eminence observations so far obtained for *more-than-expected* planetary proportions: There are, first, upward slopes consistent

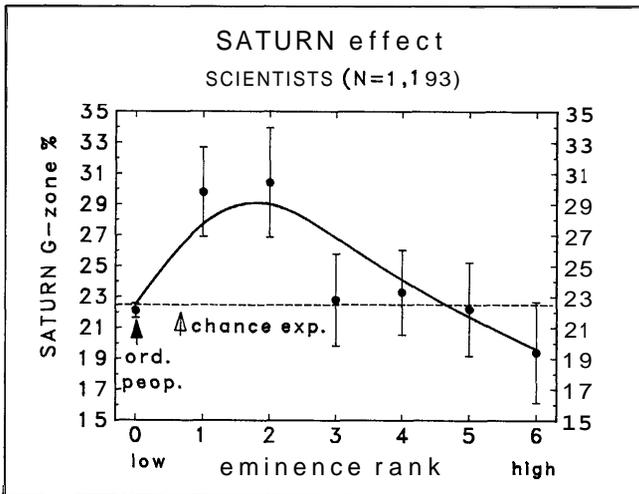


Fig. 4. With a sample of ordinary people added ($N = 7,749$) the curve takes on a curvilinear shape.

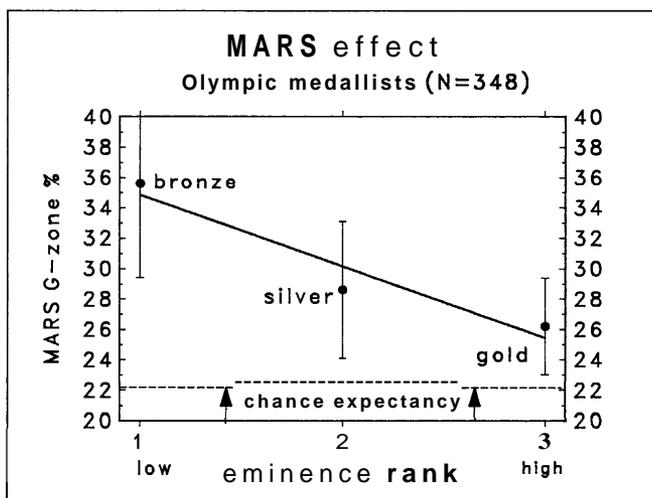


Fig. 5. For olympic winners ($N = 348$) positive deviations of Mars G-zone proportions decrease with medal rank.

with Gauquelin's expectation (Mars — athletes). There are, second, downward slopes inconsistent with Gauquelin's expectation (Saturn — scientists). Now we have been invoking a curvilinear relation with upward and downward slopes together as parts out of a broader pattern. Thus, the number of shapes does not actually increase.

Now, a hopeful inference is pending: Might the linear slant upwards (Mars — athletes) be regarded as just a section of an entire curvilinear relationship, that is, as its lower or left-hand section? Could it be that a downward slanted complement of the sports eminence curve on the right has not come to the fore because subsamples of the most eminent athletes were too small?

My first observation supporting this idea (unpublished) is several years old. Among Gauquelin's top athletes there are $N = 353$ olympic winners, most of them ranking high with citations. Among Olympic winners, however, we may still distinguish bronze, silver, and gold-medallists. Following Gauquelin's rule, we would expect an increase of Mars-born proportions from bronze to gold medallists. When I checked this at that time (the highest medal won was used for assigning an athlete to one of the three groups) I obtained the results shown in Figure 5. All three Mars G-zone proportions are high, but the curve goes down from bronze to gold instead of up. This result struck me, and I made a note starting with "Strange finding!"

But of course this graph is not complete. We should now add, at the left-hand section of the olympic eminence scale, athletes who never won medals in olympic games, and who in fact are in the great majority ($N = 4,038$) among Gauquelin athletes. We should also add, on the extreme left of the scale, the general population, people whose physical ability merely allows for watching olympic games on TV ($N = 7,749$). And when we do that, we obtain an eminence relationship that has in fact a curvilinear shape (Figure 6).

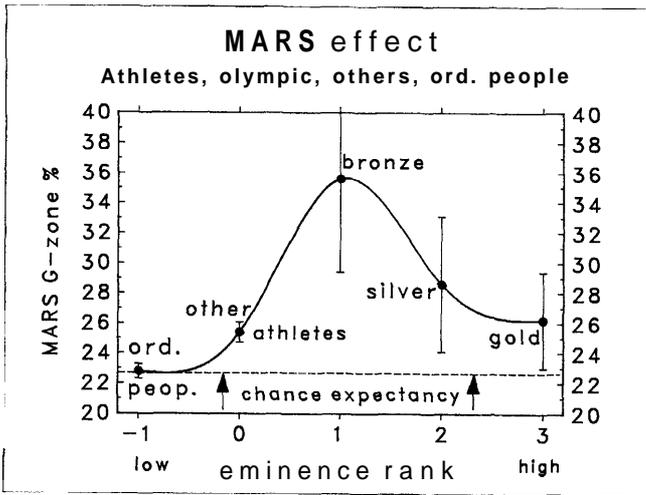


Fig. 6. With olympic non-winners ($N = 4,043$) and ordinary people added ($N = 7,749$), the curve takes on a curvilinear shape.

Another indication of curvilinearity that disturbed me some time later was obtained with actors, for whom Gauquelin had reported a Jupiter effect. Applying citation counts, I expected to replicate, for Jupiter and actors, the upwards slope that I had originally found with Mars and athletes (cf. Figure 1). I did actually find this for eminence in the lower range (Figure 7). But I also found a marked halt and even a drop in the upper range. My notebook says, reluctantly: "There is an unexpected drop with highest ranks." I hoped it would disappear by adding counts of citations from a newly-discovered reference book, a comprehensive one on theater and actors. But the turn downwards did not disappear.

Indeed, at that time, I did not welcome such observations. Anomalies within existing anomalies are not comforting. Recently, however, I was heavily reminded of these discomforts when reading Arno Muller's aforementioned report of negative results with very eminent people. Miiller's results gravely contradicted Gauquelin's eminence hypothesis, but they were consistent with my seemingly anomalous observations just described, and thus appeared to call for an eminence hypothesis suggesting a general curvilinear connection. If Gauquelin's claim were true, Miiller's unexcelled samples of celebrities should have displayed unexcelled levels of G-zone proportions. But in fact they were even much lower than those of former Gauquelin samples, which apparently makes sense only if curvilinear relations are real.

Let me do a final empirical check. Muller's two samples of celebrities, male and female, contain 143 actors and actresses. Almost all of them are internationally famous: Gina Lollobrigida, Orson Wells, Silvana Mangano, Grace Kelly, Ingrid Bergman, Marlene Dietrich, Gert Froebe, Bette Davis, Sophia

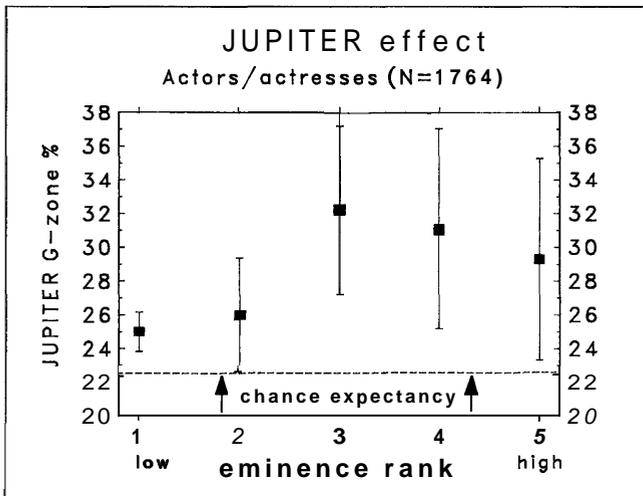


Fig. 7. For athletes (N = 1,764) positive deviations of Jupiter G-zone proportions suggest a curvilinear shape.

Loren etc., whereas Gauquelin's big sample of N=1,740 actors/actresses is generally far less eminent, see Figure 8.

What should happen? In the actors' sample collected by Gauquelin we had noticed a slight drop of a Jupiter effect with upper ranks (Figure 7). If we add to this sample Miiller's super stars, the curvilinear hypothesis predicts that the drop should become more prominent. As can be seen in Figure 9, this is exactly what occurred. In fact the curvilinear shape has greatly improved. Had Gauquelin himself collected a greater proportion of superstars he might have become aware that his linear eminence hypothesis does not apply. It is to Arno Miiller's merit that he pushed the implications of Gauquelin's eminence hypothesis to the extreme, with seeming failure as the consequence. But actually Miiller's plain result forced us to accept the reality of such deviant observations, and to venture a more radical solution of the puzzle.

What have we achieved? On the one hand our result is modest. First, we do not know whether our revised eminence hypothesis will survive all future tests. Second, even if it does we would not really know what these eminence connections mean, whether curvilinear or not. Third, we would also not understand any better the meaning of planetary effects at all - aside from eminence implications.

On the other hand, however, I see three advances. First, if our hypothesis holds, we have to deal with an amazing degree of precision regarding astro-psychological connections. Our library efforts at finding subtle differences among outstanding people at the end of their lives is but a reconstruction of how planets somehow discriminate as these lives began. Up to now we have been underrating the precision of planetary eminence discrimination and its importance had almost escaped us. Second, we have replaced the discouraging

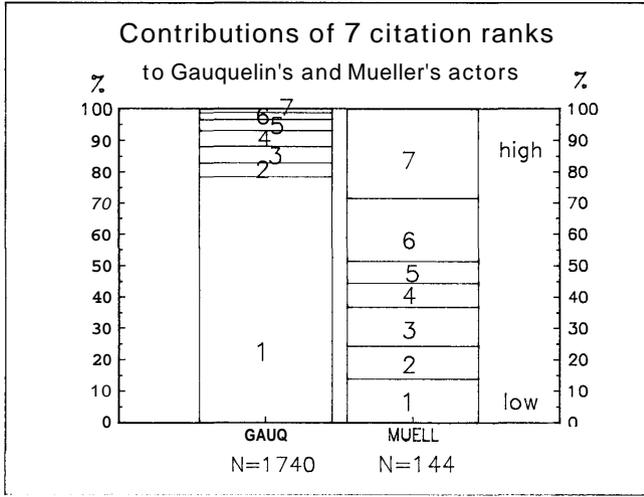


Fig. 8. Gauquelin's actors' total has a large subsample of lowest citation rank, Müller's total contains large subsamples of high citation rank.

confusion of up and down eminence trends with conceptual simplicity on a higher level. Third, we have prevented the triumph of true disbelievers who would see our laboring at self-created anomalies within anomalies as indicating a near end of the entire spook. Undisturbed by such sociological side-effects, we may now keep on playing with Gauquelin's golden grain in a heap of dust, and with that rigid block impeding smooth traffic on the road of science.

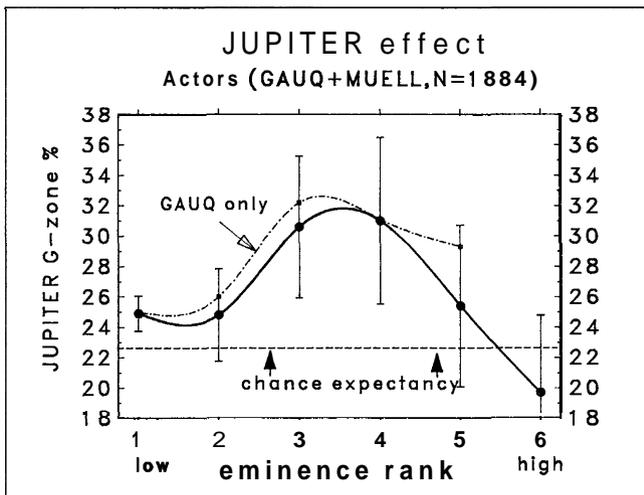


Fig. 9. With Müller's most eminent actors added (N = 143) the curvilinear shape is greatly enhanced.

Acknowledgements

I wish to thank Arno Miiller for providing birth and planetary data of 612 eminent men and Geoffrey Dean and Jim Lippard for linguistic improvement of this paper. Citation counts were patiently done by Sigrid Hubner and Claudia Kasten.

Appendix

I. Sources of birth and planetary data used for studies referred to in the present paper.

- | | |
|------------------------|--|
| Series A: | Gauquelin, M. & Gauquelin, F. (1970). Birth and planetary data. Series A. Paris: LERRCP. (5 vols.) |
| New Data: | Gauquelin, M. (1984). New birth data series. Paris: LERRCP. (3 vols.) |
| 1. Actors: | Series A, Vol. 5. New Data, Vol. 3. Muller, A. (1992a), see References. |
| 2. Athletes: | see Ertel, 1988, p. 58-61, 13 sources. |
| 3. Musicians (French): | Series A, Vol. 4. New Data, Vol. 3. |
| 4. Scientists: | Series A, Vol. 2. |

II. Reference works used for citations

1. Actors:

- Mc Neil, M.C.H. & Mc Neil, B. (1980).
Biography and Genealogy Master Index. Detroit, MI: Gale. 8 vols.
In one former study in addition:
Enciclopedia della spettacolo. (1954-62). Roma:
Casa Ed. le Maschere. 9 vols.

2. Athletes:

- (see Ertel, 1988, p. 82, 21 sources).

3. Musicians:

- Barlow, H. & Morgenstern, S. (1978). A dictionary of opera and song themes. New York: Crown.
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Boyer, J. (1953). *Kurzgefasste Geschichte der französischen Musik*. Wiesbaden: Breitkopf & Haertel.
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4. *Scientists:*

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Comments on Puzzling Eminence Effects

J. W. NIENHUYS

Department of Mathematics and Computing Science, Eindhoven University of Technology, P.O. Box 513, MB 5600 Eindhoven, The Netherlands

It is not easy to comment on a report in which no data are presented that permit any form of numerical reanalysis. Nonetheless, I do not agree with the conclusions of the author.

The author himself has demonstrated earlier (Ertel, 1988) that the Gauquelin data were biased, but he argued that these data showed an independent 'eminence effect'. Elsewhere (Nienhuys, 1993) I have argued that this eminence effect is not very convincing. In passing, I remarked that Ertel used three reference books that he had obtained from Michel Gauquelin. So some kind of bias might have come in that way.

I will now back up that argument with some numbers. The sources D, O, and B in the abbreviations of Appendix 3 of (Ertel, 1988) came from Gauquelin. These were the *Dictionnaire des Sports* of B. le Roy, four volumes of *Fahuleuses Histoires*, published by O.D.I.L., and the *World Almanac of Who*, edited by L.V. Umlauf.

If we ignore citations in these three sources, the eminence effect disappears almost completely. The following table is built up in the same manner as Table 1 in (Nienhuys, 1993), but for clarity percentages are added in the kS columns in cases where the full class has at least 30 members.

The numbers in the table do not show much of an increasing trend (i.e. an eminence effect). In addition we may remark that in the total of 4391 athletes investigated by Ertel (1988) there were 443 athletes that were not mentioned in sources D,O,B, but in at least one other source. Of these 443 there were 79 born in Mars sectors 1 or 4 (12-sector division) where theoretically 76 with a standard deviation of 3 may be expected. So these 443 didn't show (collectively) a Mars effect. As the 74 of these that were not published by the Gauquelins had only 7 (9.5%) born in a Mars key sector, something of a Gauquelin bias remains in this subsample.

If we present the data in the form of a graph, like Figure 3 of (Ertel, 1988), the graph of Figure 1 results, and again we see that the trend effect almost completely disappears. Only in the group of very famous athletes (4 citations or more) there is a surplus of 11 (namely 34 out of 91, where 23 would be expected from the key sector frequency in the total population of 4391 athletes). As these 91 athletes were almost all (with the exception of 9 Americans and 1

TABLE 1

Distributions of published French, unpublished French, published foreigners, unpublished foreigners, with corresponding numbers of those born in key sectors (6 sectors out of 36, citations in sources D,O,B ignored). Class 1: no citations, Class 2: 1 citation, etc.

| Class | PFR | kS | UFR | kS | PFO | kS | UFO | kS |
|-------|------|-------------|-----|------------|------|-------------|-----|-------------|
| 1 | 983 | 228 (23.2%) | 560 | 73 (13.0%) | 977 | 199 (20.4%) | 764 | 127 (16.6%) |
| 2 | 249 | 55 (22.1%) | 105 | 13 (12.4%) | 276 | 60 (21.7%) | 47 | 6 (12.8%) |
| 3 | 73 | 12 (16.4%) | 13 | 2 | 132 | 29 (22.0%) | 8 | 0 |
| 4 | 36 | 5 (13.9%) | 5 | 1 | 71 | 13 (18.3%) | 1 | 0 |
| 5 | 13 | 4 | - | - | 49 | 15 (30.6%) | - | - |
| 6 | 3 | 2 | - | - | 21 | 5 | - | - |
| 7 | - | - | - | - | 5 | 1 | - | - |
| Total | 1357 | 306 | 683 | 89 | 1531 | 322 | 820 | 133 |

Italian) mentioned in sources D, O, or B as well, this surplus of 11 is not impressive.

Ertel's Figure 3 showed also the results of the so called 12-sector data. In this connection we recall that the concept of key sector can be defined in several ways. Ertel's research dealt with sectors 36, 1, 2, 3, 9, 10, 11, 12 as key sectors (in a 36-sector division), whereas the original division in 12 sectors corresponds to sectors 1, 2, 3, 10, 11, 12 in the 36-sector system. I show the '12-sector' data in a separate graph, Figure 2, so Figure 2 summarizes the data in Table 1. The error bars in these graphs represent 90% confidence intervals around the population mean. Ertel's legend to his Figure 3 claims that they

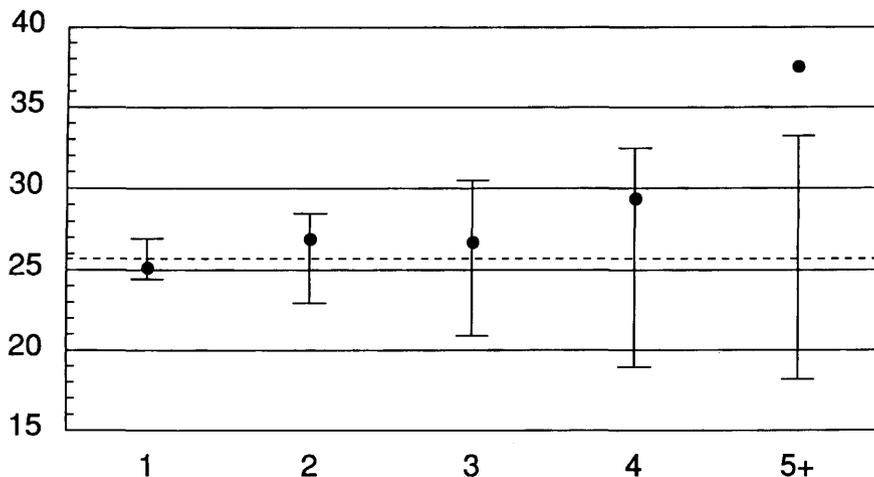


Fig. 1. The eminence effect vanishes when the three sources obtained from Michel Gauquelin are disregarded. Shown are: percentage (25.69%) of athletes born in key sectors 36,1,2,3, 9,10,11,12 (dashed line), error bars corresponding to 90% confidence intervals, centered on the dashed line, and actual key sector percentages of athletes with respectively 0, 1, 2, 3 or more citations. The last class contains 91 members, of which 34 were born in key sectors.

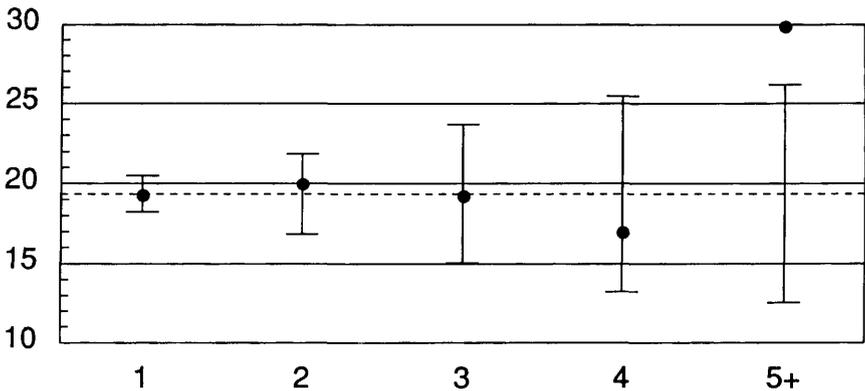


Fig. 2. As Figure 1, but now with sectors 1, 2, 3, 10, 11, 12 taken as key sectors. The average is now 19.36%, and the last class has 27 members born in key sectors.

represent 95% confidence limits, but their lengths do not match his Table 6. They seem closer to 90% confidence limits.

Instead of ignoring citations from D,O, and B we may ignore all other citations. In that case we get a graph (Figure 3) which shows a much clearer increase than the graphs of (Ertel, 1988). The conclusion seems inescapable: one cannot with any confidence claim that the eminence effect has nothing to do with a Gauquelin bias. Figure 3 represents graphically the fractions 66512714, 30511159, 1581518, and a chi-squared test yields 8.525 with 2 degrees of freedom, corresponding to a 'significance' level of 1.4%.

Ertel reports on several investigations of the eminence effect, to wit: wrong slopes (Mars Effect with olympic medallists, Saturn Effect with scientists), the unexpected drop in the highest ranks of actors with Jupiter and Müller's test of eminent professionals. After so many failings one might easily conclude that the whole idea doesn't work.

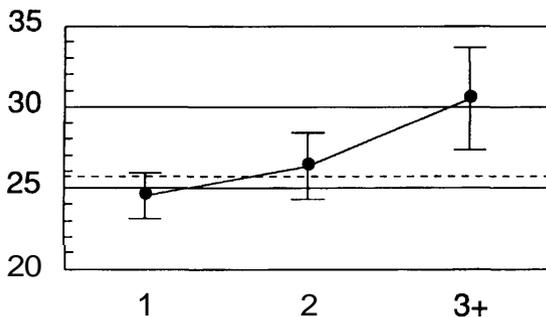


Fig. 3. As Figure 1, but now only citations in the three Gauquelin sources (and in no other sources) are counted. The slope is about 3% per citation, roughly double the slope of Ertel's eminence effect.

This is not Ertel's conclusion. He postulates ('a breathtaking inference') that the straight lines found or predicted previously were just part of a curve. Now there is no way to compare the value of a citation in *Dictionary of scientific biography* with one in *The international cyclopedia of music and musicians*. One might argue that the general public is very familiar with contemporary film actors, musicians and sports champions, but that in matters of science public knowledge is restricted to Pythagoras, Newton and Einstein plus a few national heroes (like Descartes, Pascal, Pasteur and Curie in France), and hence that 'scientific excellence' rates far below 'sports or music fame'. Because there is no way to objectively compare excellency in different professions, Ertel can at will hypothesize that the scientists are so excellent that the scientists are collectively in the 'truly excellent' area (where the trend in the planetary effect is reversed) and the musicians not. With this type of analysis almost any outcome can be fitted to existing data.

Ertel's curvilinear model in fact has four parameters: the onset of the first and second slope, and the values of both slopes. Maybe the location of a horizontal piece in between can be counted as a fifth parameter. No wonder he can fit just about anything with at most seven points and large error margins. Usually (in physics) the addition of extra parameters to a model is considered the opposite of simplification. But Ertel confuses unification with simplification.

I'll leave it to the reader to ponder whether this exercise in curve fitting mirrors 'the precision of planetary eminence discrimination' or 'the near end of the entire spook' in a quagmire of complexity. But to consider 'prevented the triumph of true disbelievers' as an advance in science seems strange.

The strong influence of the sources D, 0, and B may serve as an illustration of the limits of citation analysis. In this case the conclusions are only as good as the choice of sources permits. If for some reason the sources are biased then this bias is in the conclusions too. For example, if the sources are chosen in such a way that the USA (with a population of over 200 million) is represented much less than a few European countries with a comparable total population, then these sources will of course indicate a lack of excellent sportsmen among Americans.

The conjectured *modus operandi* of the Gauquelin bias may partly explain the puzzling eminence effects. Suppose this bias is the result of choosing for separate groups of champions separate proficiency levels. In each separate group the choice of a single level that maximizes the 'Mars Effect' would nearly always be done in such a way that the very famous (Gold Medal winners) would be included. So the very famous would not be the subject of any kind of bias, and therefore their Mars percentage would be close to that of the general population. In other words the bias would be strongest in the low range, and decrease towards higher ranges. Also, one would expect that in the unpublished material the Mars percentage would also decrease with increasing eminency. In fact, with some imagination one can discern such trends in the data of Table 1, but they are not very strong. Therefore I think that selection by

separate setting of proficiency standards cannot be the only source of the Gauquelin bias, other methodological errors may also be responsible.

Acknowledgements

I wish to thank my son H.-W. Nienhuys for help with programming, and C. Koppeschaar for helpful discussions.

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A Systematic Survey of Near-Death Experiences in South India

SATWANT PASRICHA

Department of Clinical Psychology, National Institute of Mental Health and Neuro-Sciences (NIMHANS) Bangalore, 560 029 India

Abstract—In order to determine the prevalence rate of Near-Death Experiences (NDEs) in south India, the author systematically surveyed four villages with a total population of 6430 persons. Eighteen persons were reported to have died (or nearly died) and revived. Thirteen (72%) of them reported having had NDEs which is about 2 cases per thousand of the population surveyed.

Eleven features of the south Indian cases are compared with the features of north Indian cases. Most of the principal features of the south Indian cases resemble those reported from north India. However, they also differed in the frequencies of four features; two of the features were reported chiefly by the north Indian subjects while another two only by the south Indian subjects. All these four features have been reported among American cases. The author suggests the possibility of a genuine phenomenon underlying the similarities of features among cases in two different cultures.

Introduction

Persons who come close to death from grave illness or injury and survive, sometimes afterward report unusual affective and cognitive experiences which occurred at a time when they were ostensibly unconscious and near death or even considered dead. Such experiences are often referred to as "near-death experiences (NDEs)." Although scattered reports of such experiences have come from many parts of the world, the majority of studies derive their data from Western patients (Greyson and Stevenson, 1980; Moody, 1975; Ring, 1980; Sabom, 1982). To date, only two reports of such cases have come from India (Osis and Haraldsson, 1977; Pasricha and Stevenson, 1986).

In an earlier paper we (Pasricha and Stevenson, 1986) reported some 16 cases of near-death experiences that came to our attention while investigating reincarnation type cases in India. The paper included summaries of four cases of the Indian NDEs. It also compared the frequency of 12 features in the 16 Indian NDEs with their frequencies in 78 American NDEs. The data for the American cases derived from an earlier study reported by Greyson and Stevenson (1980). Six of the features were reported mainly by the Indian subjects, namely: taken to other realm by messengers; passed to a 'man with a book;' sent back because of a mistake; subject not 'scheduled' to die yet, another person said to be due to die instead of subject; brought back from other realm by messengers; and (subjects had) residual marks on the body after NDE. Three features were found

only among the American subjects which were: saw own physical body; sent back by a loved one or an unknown figure but not because of a mistake; and reviewed own life, "panoramic memory." And the following three features were reported by the subjects of both the cultures: saw deceased acquaintances (in other realm); saw 'beings of light' or religious figures; and apparently revived through thought of loved living persons or for other reasons and own volition. We had argued in that paper that the difference in features between the two cultures may be due to the effects of one's own beliefs regarding life after death and that some of the experiences may seem different in details of description but on closer examination, found to be similar in nature.

There have been few studies of the NDEs in non-Western cultures and far fewer systematic surveys anywhere in the world. The only attempt at a systematic survey of the general population was that of Gallup (1982), which used a somewhat ambiguous question¹. The present paper reports the first investigation of cases concerning the prevalence and features of NDEs in a particular geographical area.

The survey was conducted between 1985 and 1986 by using appropriate sampling techniques. The present article reports my findings of a systematic survey completed in four villages. It also includes features of the NDE cases identified during the survey and investigated up to 1989.

Method

The Survey Villages and Population

In order to learn the prevalence rate and characteristics of NDE cases, I undertook a systematic survey in an area (Channapatna, District Bangalore) of Karnataka State, India. Bangalore is the capital of Karnataka State (south India). The Bangalore district is divided into 11 subdivisions, called Taluks (roughly corresponding to Tehsils in northern India and to counties in the United States and the United Kingdom). One of these subdivisions, the Channapatna Taluk, is situated on the Bangalore-Mysore road, about 60 kilometers southwest of Bangalore. This area was chosen for two main reasons: First, it included both urban and rural populations and second, it was easily accessible from the institution where I work (the National Institute of Mental Health and Neurosciences [NIMHANS]) in Bangalore.

Criteria for Inclusion

In determining the prevalence rate the following inclusion criteria were applied: (i) the subject must have been a resident of the survey village at the time of the survey; and (ii) the subject must have been alive at the time of the survey.

Using the latest available voters' registration lists, 663 target respondents (persons designated for interviews) were identified in the four survey villages. In order not to miss any cases (as far as possible), every household was included in the present study² and one member from each household was interviewed with regard to his or her knowledge of the cases. The person interviewed was

usually the head of the household, but a younger member was interviewed when the head of the household was not available. Eighteen respondents had either moved out of the village or were not available at the time of our visits (we made two call backs for the respondents who were not available during the first scheduled visit). Finally, a total of 645 respondents were available for the interviews.

The ages of the respondents ranged between 16 and 90 years. Three hundred and ninety-nine (62%) were males and 246 (38%) were females. Four hundred and sixty (71%) were illiterate or functionally illiterate; 76 (12%) had attended a primary school; 62 (10%) had gone to a middle school (or intermediate school); and 47(7%) had had education up to high-school or beyond. Four respondents were college graduates.

Information regarding their occupations was available for 640 respondents. Half the respondents were cultivators, and of the remaining half 158 (25%) were housewives; 123 (19%) were laborers; 19 (3%) had their own business; 13 (2%) were caste laborers³; five were in government service; and two were students.

Information on religion and socioeconomic status was available for 642 respondents. All the respondents were Hindus, and a majority of them (65%) belonged to the lower middle socioeconomic group. Only one respondent belonged to the upper class; 10 respondents belonged to the upper middle, 85 (13%) to the middle; and 132 (21%) to the lower socioeconomic class. The socioeconomic status was appraised by using a standard technique developed for the rural population of India (Pareek & Trivedi, 1964). The technique consists of a scale which includes such parameters as the level of education of the head of the household, the type of house he lives in, his furniture, and agricultural equipment etc.

An interview schedule was administered to each respondent, which, in addition to eliciting the usual demographic data, asked questions about the respondent's belief, familiarity, and knowledge regarding cases in which a person had apparently died and revived.

The respondents who said that they were familiar with cases of the kind being studied, were asked to give specific information about a subject whom they knew or of whom they had heard, either in their own village or any other place. Later, 12 of the 13 subjects identified within the survey village were interviewed in detail about their experience—one subject had died after the first round of interviews. In addition, 4 informants who were present when the subject revived or narrated his experiences to them, were interviewed. If the respondents did not have any knowledge of or familiarity with the cases, only their demographic details were noted, which did not take more than five minutes. If on the other hand, an informant was the subject of a case or had first-hand knowledge of a case, for example of one occurring among his (or her) family or close friends, the interview was much longer and lasted for 40 minutes or more.

When the subjects were interviewed, they were first allowed to narrate their experiences spontaneously and then specific questions were asked from a checklist (see Appendix) in order to elicit specific information not mentioned

spontaneously. In all, 12 subjects were interviewed. One subject had died by the time the first round of identifying the cases was completed. The essential information for this case was obtained from the subject's relatives. The relatives or friends of subjects were interviewed in 5 more cases; hence 17 informants (including 12 subjects) were interviewed for 12 cases. In addition to the questions about the subject's physical condition, they were asked about what the subject had told them (the informants) about his or her experience. In general, the versions stated by informants who were not subjects agreed with what the subjects themselves stated about their experiences.

Results

Belief, Familiarity, and Knowledge of Revival⁴ and NDE Cases

Of the 645 respondents interviewed, 148 (23%) respondents believed that it was possible for a person to die (or almost die), recover, and have memories of the period during which he or she was dead (or apparently dead). Sixty-two (42%) of these respondents had heard of one or more such cases in their own village and 47 (32%) respondents had heard of such cases in other villages. The belief in revival on the part of these informants might have derived from their knowledge of cases. However, the remaining 39 (26%) informants, although they believed in revival, had not heard of or known about a case.

Cases in the Survey Villages

Sixty-two (42%) of the respondents who were familiar with revival cases also referred to one or more specific cases in their own village. However, not all respondents who were familiar with such cases, knew of every case in his or her village. In all, informants made references to 26 cases.

Eight cases were excluded since they did not fulfill the inclusion criteria; seven of the subjects of these cases had died long before the survey, and one had moved to another village. The remaining 18 cases were investigated for further details. Table 1 shows the distribution of cases in the four villages surveyed. It

TABLE 1
Prevalence of Revival and NDE Cases in the Survey Villages

| Population Village | 1985 (estimate)" | No. of Revival Cases | No. of NDE Cases |
|-----------------------|---------------------|-------------------------|---------------------|
| Patalu | 1979 | 5 (2.5) | 3 (1.5) |
| Kannamangala | 1312 | 1 (0.8) | 0 (-) |
| Sunnaghata | 1253 | 3 (2.4) | 3 (2.4) |
| Sigrajipura | 1886 | 9 (4.8) | 7 (3.7) |
| Total | 6430 | 18 | 13 |

* Estimated annual growth (by geometric progression) from 1981 census figures (Census of India, 1981).

Note. Figures within parentheses show prevalence per thousand.

includes the cases of persons who had seemingly died (or nearly died) and revived and the number of cases in which NDEs occurred.

Eighteen cases of the revival type were reported in a total population of 6430. In five cases the subjects had been seriously ill or were thought to have died, but they had remembered no experiences during that period. The remaining 13 cases were reported to have had NDEs. One of the subjects died after my first contact, and her family members did not remember much about the experience except that after reviving, the subject said that she was taken to "Yamapatana" (the place in Hinduism where Yama, the god of death lives). Since sufficient information, except on demographic characteristics, was not available for this last case it was not included in the analysis of the main features.

The Prevalence of NDE Cases

In all, 13 cases were reported among an estimated population of 6430 persons (based on annual geometrical growth projection of the 1981 census). Hence the prevalence rate of NDE cases was 2 in one thousand population. In three villages the occurrence of cases ranged between 3 and 7 cases; in the fourth village (Kannamangla) no case of a NDE was reported.

Demographic Characteristics of the Subjects Who Had NDEs

Thirteen of the subjects who reported a near-death experience, 12 (92%) were females; the median age of the subjects at the time of the NDE was 29 years (range 20–70 years), and it was 50 years (range 31–80 years) at the time of our first interview with them; the median time lapse between the NDE and the interview was 20 years (range 1–49 years). Information on the socioeconomic status of the subjects was available for 11 cases. We could not obtain information in two cases as the subjects were reluctant to give information during our first interview and they were not available on a subsequent visit. Six of them belonged to the lower middle class, four to the lower, and one to the upper middle class. Information regarding their occupations was available for 10 subjects. Six of them were housewives, three were laborers and one was a farmer.

Physical Condition of Subjects Prior to the Time of NDE

Although reasonably good medical facilities are available in Channapatna, and the survey villages were situated at a distance of 5 to 14 kilometers from it, people generally did not seek medical consultations unless they recognized an emergency. (Such underutilization of medical facilities is quite common in most Indian villages.)

Six subjects were suffering from a mild to severe physical illness prior to the experience. The subjects reported that they had suffered from a wide variety of ailments including: fever for one day; fever off and on for six months, dysentery and difficulty in breathing for one day; 20 days following childbirth, involuntary trembling of the index finger followed by difficulty in lifting the forearm

and loss of consciousness; disorder with the abdomen, hand, and feet swollen; stoppage of passing urine and stools; some brief illness (no details available); and gall bladder operation. I was able to confirm the physical condition of three subjects from the informants. Informants were not available for the remaining cases.

Except for three cases, all the subjects had their experiences while at home. Two of the subjects had a NDE on their way to or from the hospital while one had it at the pond where he had nearly drowned and was rescued. It follows from what has been said above that except for two patients who were taken to the hospital, all the subjects revived spontaneously and without the measures of resuscitation available in the hospital. The records of those two patients who were taken to the hospital were not traceable. In one case the patient had been admitted a long time earlier. In the other case sufficient details were not available for tracing the records. (Presuming that specific measures were taken to revive these two patients, data from them are too meager to warrant making any comparison with cases in which no formal techniques of intervention were used.)

Before I describe the main features of the cases, I shall next present summaries of four cases to provide a sample of the description of the NDEs as reported by the subjects themselves.

Case Reports

The Case of Muttaka

The subject, Muttaka, was about 65 years old at the time of my interview with her in December 1985. She gave the following account of her NDE, which had occurred about 20 years earlier, and of the circumstances at the time. She was suffering from an abdominal ailment. Her hands and feet were swollen, and she was unable to pass urine or stools; and she fell unconscious. "I saw three persons with curly hair coming. Then I found myself outside (on the threshold of) a door. Inside, a fat man was sitting on a bench and looking through some papers. He told those three persons: 'Why have you brought her. She still has [not completed her allotted] time' and he threw away my papers. After that I do not know how I came back."

Muttakka said that she seemed to go up in the same body, but there was no swelling on any part of her body and she was able to stand, which she was not able to do in her physical body because of her illness. She also felt that time moved very fast and that the whole experience lasted only one minute. However, no informant was available to tell us about the actual duration of her unconsciousness.

The Case of Javanamma

We interviewed Javanamma, a woman of approximately 60 years, first in December, 1985 and again in January 1986. She had fractured her right forearm

about six years earlier, and then had a fever three days later. Although she was given an ointment by a local doctor for application on the site of the fracture, she received no treatment for the fever. She gave the following description of her experience:

"I was dragged 'up' by four yamadoots (messengers of the god of death, Yamaraj). I saw one door, and went inside. I saw my mother and father there. I also saw the Yama who was fat and had books in front of him. The Yama started beating the yamadoots for having taken me there instead of another person. (The name of the 'other' person was not mentioned.) While the yamadoots were being beaten up, I was accidentally hit on the back. As a result, I felt a severe pain and developed a mark on my back. The pain was more severe there (in the other realm) than it was after I returned back. I was asked by my parents and the Yama to be sent back. I was scared to be there because there were so many people, and I was happy to be back so I could see my children."

She also had a mark on the forehead which had allegedly resulted from the branding done by the Yama. "That is done to every one who returns from there."

The Case of Kenchamma

We interviewed Kenchamma, in January 1986 when she was about 35 years old. At the age of about 28, when she was pregnant for 5 months, she had a spontaneous abortion and bled profusely. According to the mother of the subject, Kenchamma was bleeding profusely for about 4 hours and had lost consciousness. The people around her believed that she was dead because "there was no pulse, no breathing and her body had become cold." While allegedly dead, Kenchamma had the following experience: "Four women came and carried me along. They met a man while going (to the other realm) who asked those women, "Why are you carrying her? She has young children, return her back." While saying this he placed a hot rod on my knee and pushed me down. I felt a strong burning sensation from the heat; when I regained consciousness I had a wound on my knee which was treated with some indigenous ointment. The four women who had carried me along, brought me back." The subject claims to have had developed a residual mark as a result of the burning.

The Case of Gowramma

We interviewed the subject Gowramma, first in 1987, and again in November 1989 when she was about 22 years old. She had no illness till the time when she suddenly felt weak and fell unconscious for about ten minutes. She gave the following description of her NDE.

"I was taken up by some messengers in a jeep to Yamapatna (the place where Yama, the god of the dead lives). He had a listing of names in the books. Yama looked into the books and told the messengers, 'Send her back; she still has not

TABLE 2
Main Features of North and South Indian NDE Cases

| Feature | North Indian Cases | | South Indian Cases | |
|--|--------------------|------------------------------|--------------------|------------------------------|
| | Number Queried | Number Positive Response (%) | Number Queried | Number Positive Response (%) |
| Saw own physical body | 16 | — — | 7 | 1 (14) |
| Taken to "other realm" by some messengers | 16 | 12 (75) | 13 | 11 (85) |
| Saw deceased acquaintances | 16 | 4 (25) | 12 | 7 (58) |
| Saw religious figures | 16 | 12 (75) | 12 | — — |
| Went to "a man with a book" | 16 | 8 (50) | 12 | 10 (83) |
| Sent back because of a mistake; subject not scheduled to die yet | 16 | 10 (62) | 10 | 6 (60) |
| Another person said to be due to die instead of subject | 16 | 7 (44) | 10 | 3 (30) |
| Apparently revived through thought of loved living persons or other reasons and own volition | 16 | 1 (6) | 12 | — — |
| Sent back by a loved one or an unknown figure, but not because of a mistake | 16 | — — | 10 | 1 (10) |
| Brought back from other realms by messengers or by deceased relatives | 16 | 13 (81) | 8 | 7 (88) |
| Residual marks on physical body after NDE | 16 | 4 (25) | 12 | 4 (33) |

completed her time." During the experience, she claimed to have seen her body which was lying down, could see people, and also her house as from above. This was the only case in which the subject reported an out-of-the-body experience.

Main Features of NDE Cases

Table 2 presents the frequencies of the main features of the NDE cases from north and south India. The figures for the north Indian cases have been taken from an earlier article (Pasricha and Stevenson, 1986). A dash indicates lack of information about that feature; it was not spontaneously mentioned by the subjects (or their relatives) and we did not make systematic inquiries especially of the north Indian cases.

Discussion

In our earlier paper (Pasricha and Stevenson, 1986) we expressed the hope of conducting a systematic survey to learn the prevalence rate of NDE cases in India. The present paper is a report of such a study.

The survey showed that 18 of the estimated 6430 persons (2.8 per thousand) had died and revived with or without intervention. All of them, however, did not have or did not remember experiences during a near-death episode; 13 (72%) of them narrated some such experiences. The ratio of near-death condition and near-death experiences was therefore appreciably higher in these Indian cases than it was in an American series reported by Sabom (1982); he found that only 43 per cent of persons coming close to death could later remember any experience at the time.

In both the series of Indian cases the following features were markedly more frequent than in American ones (Greyson and Stevenson, 1980): the subjects were taken to the "other realms" by some messengers, they saw or met deceased acquaintances or relatives; they saw a man with a book; a mistake was discovered such that the subject's time had not yet come or another person was scheduled to die; the subject was brought back by the messengers; and on return from the other realm he or she had some mark or marks on his or her physical body.

The above picture is considerably different from the one presented by the American cases. The subjects of the American cases are not taken by any messengers, they just find themselves in the other realm; no mistake is discovered for their return back; they come back of their own volition or for the love of their surviving relatives or friends.

However, differences in features occur not only between American and Indian cases, they are also found between the cases reported from two regions of the same country namely, north and south India. For example, two of the south Indian subjects (one each) reported features that were not found in the north Indian cases but were commonly reported by the American cases. These were: the subject was sent back by an unknown figure but not because of a mistake; and the subject saw his physical body from outside. On the other hand, most of the north Indian subjects met religious figures, and one of them revived on his own volition rather than being sent back. Both these features namely, seeing religious figures or beings of light and revival through one's own volition, were missing from the south Indian cases but have been reported in the American cases.

The differences in reported features even within the same culture may be due either to insufficient data or to actual variations in the experiences. On the other hand, the resemblance of some features between the American and Indian cases points toward the possibility of some common experiences that remain unaffected by cultural influences. Further studies on larger samples across cultures will help in understanding factors responsible for the similarities (or differences) in features of the NDE cases.

Conclusions

The prevalence rate of NDEs in a region of south India was found to be 2 cases per thousand. The features of the cases resemble each other in the two series of cases from northern and southern India. However, they differ from each other on four features. All these four features have been reported among the subjects of American cases. It is possible that the subjects go through some common experiences irrespective of their cultural background.

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Notes

Department of Clinical Psychology, National Institute of Mental Health and Neuro-Sciences (NIMHANS), Bangalore, India.

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Reprint requests at the above address.

1. "Here is a question about unusual experiences people say they have had when they had been on the verge of death or have had a 'close call' such as experiences of continued life or an awareness of after death. Have you, yourself, ever been on the verge of death or had a 'close call' which involved any unusual experience at that time?" (Gallup, 1982, p. 183)
2. In an earlier survey of reincarnation type cases (Barker and Pasricha, 1979), we had chosen every tenth village and every tenth household in

each village for interviewing a respondent. We learned that choosing every tenth household was not an adequate method for identifying all the cases; we missed almost 30 percent of them.

3. For example, a person belonging to the Dhobi (washerman) caste did washerman's work, and a Kumhar (a potter) made pots for his living.
4. The term revival refers to persons who had come close to death, but the informants did not know whether they had had a near death experience or not.

Appendix

Information about NDEs (Checklist)

Age at occurrence of experience _____

Physical condition at the time of NDE _____

1. Healthy _____ 2. Illness _____ 3. Surgical operation _____

4. Childbirth _____ 4. Drugs _____ 5. Other _____

Duration of illness (if ill) _____

Taken to other realms by messengers _____

Saw own physical body _____

Review of life/Panoramic memory _____

Went to a man with a book _____

Who took you there _____

Description of other realms (brief) _____

Another person died instead of S _____

Sent back because of a mistake _____

(S was not scheduled to die)

Sent back for some other reason (Specify) _____

Met other deceased persons/religious figures _____

Who brought you back from other realms _____

Residual marks on physical body after NDE _____

Impact of the experience on S _____

Change in attitude toward life _____

Change in attitude toward death _____

Evidence of ESP after NDE _____

Any other information _____

Communication about NDE

Did you tell about your experience to anyone? _____

How long after the experience? _____

Relation with the person _____

Address of the person _____

Knowledge of NDE cases before own NDE _____

The Willamette Pass Oregon UFO Photo Revisited: An Explanation

IRWIN WIEDER

459 Panchita Way, Los Altos, CA. 94022

Abstract — In November of 1966, a Ph.D. scientist, returning to his California home from a business trip in Washington, was driving through Oregon and paused at a lookout point to photograph Diamond Peak. Of the three photographs taken at this location the last one included a strange object. This photo ultimately became the focus of a controversy among UFO investigators and has been the subject of numerous articles as well as a book. In this paper we present a chronicle of the author's investigation of this intriguing photo as well as an explanation of the true nature of the object in the photo.

Historical Background

The Photograph

The author first encountered the photograph in a book (Vance, 1977) where it was prominently displayed as evidence to confirm a theory on UFO propulsion. Fig. 1 shows the photo which exhibits a "classic" domed disk-shaped object with a series of alternating dark and light bands beneath and a faint vapor-like trail apparently emanating from below these bands.

The Witness

According to the investigator who first interviewed him', the witness-photographer was a Ph.D. Biochemist who had been a Naval officer during World War II. During that military service he had been trained in quick response identification of enemy aircraft under adverse conditions. Further, he was successful in his profession and preferred to remain anonymous. These factors constituted a nearly ideal combination for a reliable witness.

The Event

Evidently, the witness and his wife were returning from a business trip and decided to take a scenic route on the way back to their home in California. Between 9:00 and 10:00 A.M. on November 22, 1966 they were driving southeast on route 58 in Oregon, stopping at various spots along the way to take pictures of the scenery. One such stop was at the Diamond Peak vista point at Willamette pass. His wife was driving and pulled into the turnout, keeping the motor running while the witness got out of the car and climbed part way up a snow bank to get a few snapshots of Diamond Peak off in the distance. The peak was covered with clouds, but the witness took two photos and decided to

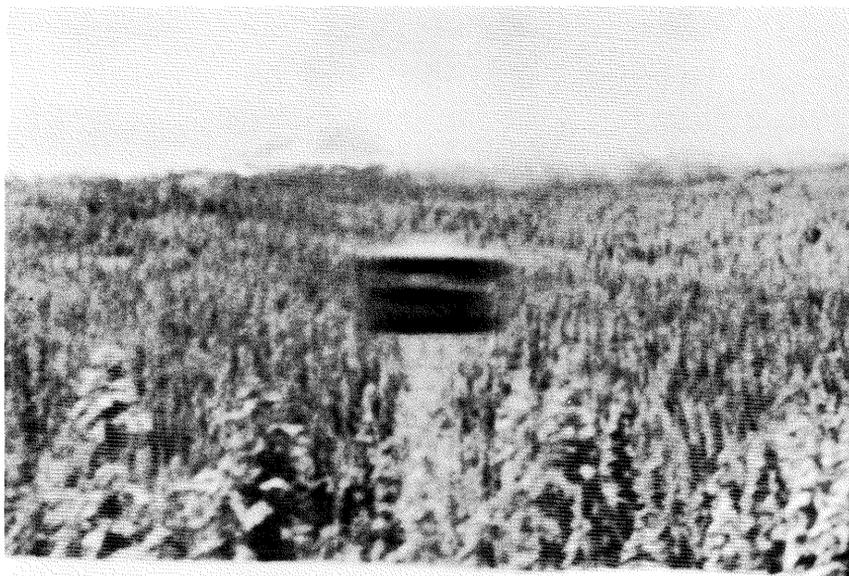


Fig. 1. A cropped enlargement of the object taken from the original negative.

wait a few moments to see if the clouds would part so that he could get a clearer picture of the peak. The witness stated that as the clouds started to open up he aimed his camera at the peak and was framing the picture when his eye caught something moving into the range of the viewfinder. Reflexively he snapped the shutter, looked up quickly and (utilizing his military experience) noted details of the object as it moved silently and rapidly to his right, disappearing in a matter of seconds into a cloud bank several miles away.

The First Acknowledgement Of The Sighting

When the film was developed, the witness' wife was the first to inspect the pictures and immediately noticed the strange object in one of the prints taken at Willarnette Pass. It was only then that the witness mentioned that he had seen something while taking the picture. The witness told his wife that what he had seen was like the top portion of the object in the photograph; he had not seen the banded structure beneath the object, nor had he seen the misty trail.

The Reactions To The Photo

Some months later the witness and his wife were showing the photo to a friend who happened to be in the Air Force reserves. At his urging, and through his contacts in the Air Force, a print of the photo was submitted for evaluation. Shortly thereafter the witness was contacted by a major in the Air Force who

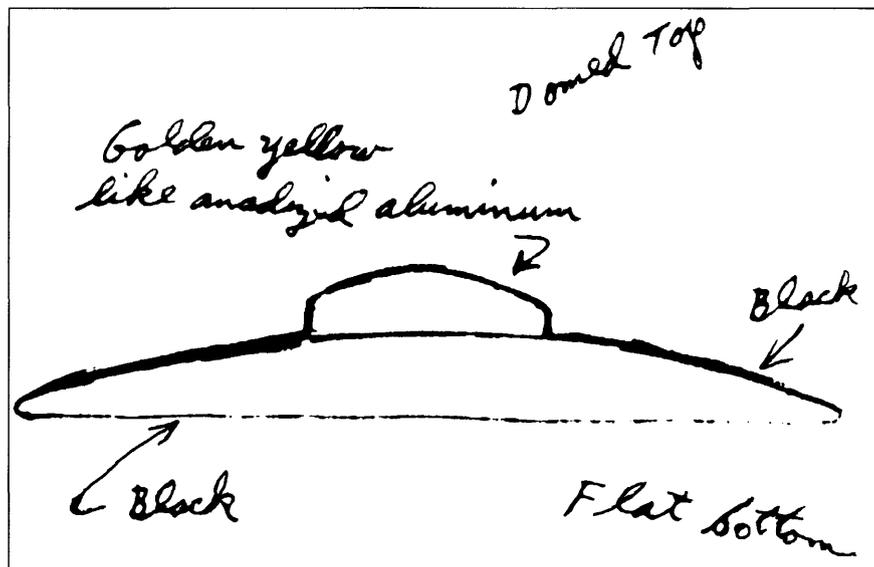


Fig. 2. The sketch made by the witness of what he saw at the time he took the photograph and immediately after.

suggested that the photo could have been made by tossing three frisbies into the air and snapping a picture, and expressed no interest in pursuing the matter further.

Nevertheless, in 1969, the witness contacted Dr. William K. Hartmann, who had been identified in the journal "Science" as the principal photographic analyst for the Colorado project under Dr. Edward Condon. However, Dr. Hartmann did not respond. This discouraged the witness so that he made no further attempts at eliciting interest until 1971 when he met veteran UFO investigator Paul Cerny. After interviewing the witness, Mr. Cerny asked him to fill out a standard report form.

The Witness Report

In the report the witness stated the following:

- a) The object rose, in apparent "pulses."
- b) He was aware of the object as he snapped the picture and immediately thereafter.
- c) The object disappeared up and to the right in 2-3 seconds.
- d) He saw only one object which did not show the tiered structure in the photograph.

His descriptive sketch of the object is reproduced in Fig. 2.

Publications Concerning the Photograph

There were at least 6 publications in the literature which dealt with or referred to this Oregon photograph during the period from 1971 through 1981. Initially the response to the photo was one of doubt², primarily because of the large discrepancy between what the witness claimed to have seen and what was recorded in the photograph, and in particular because of the dark bands which persisted even though an object of high brightness was supposed to have moved up and past these black areas during the exposure (Nixon, 1971).

A second argument was made that if the object had been only the uppermost part as shown in Fig. 2, and the tiered effect was a result of the object being in three locations during the photographic exposure, then background trees should have been visible through much of the image, and this was not the case (Nixon, 1972).

A further argument against the validity of the photo was that the witness did not mention the sighting to his wife when he returned to the car. (Nixon, 1973).

Nevertheless, UFO investigator Adrian Vance was apparently convinced that this photograph was valid and represented an opportunity to understand the unknown technology by which UFO's might be propelled. In order to explain the strange appearance of the photo, he proposed a mechanism for UFO propulsion which involves rapid disappearance and reappearance of solid bodies (Vance, 1973 and 1977).

In 1981 noted UFO proponent and investigator Dr. Allen Hynek believed sufficiently in the validity of the photo to highlight it in a publication of the M.I.T. alumni (Hynek, 1981). In it he refers to the photo as "one of the most puzzling on record."

Thus the reaction by the UFO community was mixed and ranged from enthusiasm to skepticism. Nevertheless, the photograph is still prominently displayed at UFO symposia and in television documentaries.

Phase I, The Belief Mode

The Author Gets Involved

As a physicist with an active curiosity, the author had been aware of the UFO puzzle but had avoided active participation in a UFO investigation because never before had a case presented an opportunity for him to apply the scientific method as part of the study. Despite doubts concerning the model proposed by Adrian Vance, his idea that this was an important photo (if we could understand it) was intriguing. Accordingly, in August of 1980, the relevant UFO investigators³ were approached to arrange a meeting with the anonymous witness. It was then learned that the witness lived nearby. This coincidence in combination with the witness' excellent credentials as outlined above made this event a very attractive one for study. Thus began a series of meetings with the witness which continued sporadically for several years.

Initially, we went over the sighting in exhaustive detail, and early on agreed that access to the original negative (which had been in the custody of Adrian Vance) would be essential for the research. After months of negotiation, Vance finally agreed to relinquish control of the negative, subject to the condition that it would be transferred in person. Subsequently, the author drove to the Los Angeles area and received the original negative.

The Negative

The negative was in its original strip form and the sequence of photos was consistent with the witness' description, starting up north and following the trail of his trip through Oregon, with the three shots taken at Willamette Pass in the middle of the strip. These three shots were as described by the witness, the first two showing the trees and distant cloud-covered mountain, and the third with a strange object between the camera and the trees. The original negative had not been tampered with and had been developed at the time and place identified by the witness. At this point, I had been infected by Vance's enthusiasm and began to believe that this photograph was unique. The enlarged prints and negatives made from the original negative⁴ revealed much more detail than the reproductions in the various journal articles and enhanced my enthusiasm for the project.

The Approach

With the authenticity of the negative established, inquiries were made to verify the education of the witness and confirm his credentials. As a result of these inquiries I found myself in a strong belief mode. It was then that the decision was made to assume that everything that the witness said was true, (especially the discrepancy between what the witness saw and what was recorded on the film) and to see where that would lead.

The First Investigations

Rather than viewing the discrepancy between the eye and the film as a negative factor as Stuart Nixon had, (Nixon,1971) I chose the Adrian Vance approach and treated it as a potential opportunity to learn something about UFO sightings. In order to confirm what the witness actually saw, an attempt was made with regressive hypnosis to bring the witness back to the time of the sighting, but he proved impossible to hypnotize. The witness remained adamant that what he saw was not three-tiered but was like the upper portion of the object in the photo.

Attempts at computer enhancement of the original negative had been initiated⁵ but had resulted in no improvement of image quality.

A Review of the Literature

In (Nixon, 1971) the argument was made that because the witness claimed to have seen one object and there appeared to be a multiple image on the film the validity of the photograph was therefore questionable. From a physics viewpoint this argument can be challenged because of differences between the human eye and photographic film. Parameters such as response time or spectral sensitivity may be quite different for these two detectors. Surely, there could be circumstances where different results could be obtained with two such different detectors.

The point that the witness said nothing to his wife when he returned to the car was used as evidence that the photo was invalid (Nixon, 1973). However, the witness' attitude on this matter is purely personal and should not necessarily cast doubt on the photo.

A further point was made that if the object was rising, the preservation of the extremely dark bands would not be possible since the bright upper portion of the object would have exposed the film in the dark areas as it swept by (Nixon, 1971). However, this argument did not take into account the possibility that the object could have swept up in a pulsed manner at a very high speed between resting places, thus leaving little or no trace of exposure in the dark areas.

The matter of the tree background not leaking through the image of the object (Nixon, 1972) was a potentially serious problem. If the object was really like what appeared only at the top of the image (see Fig. 2) and was really rising in pulses as the witness described in his original report, then there should have been at least one horizontal band where the object could have been for at most one third of the time—and the background should have leaked through with at least two thirds of its original intensity. This was clearly not the case, so that the approach calling for one object in three places had to be abandoned. Instead, it would be necessary to pursue models that allow for only one object that might appear differently to the naked eye from what would be recorded on film during a short exposure.

An Attempt At A Physical Explanation

Since the witness was absolutely certain that he had seen only the top portion of the object in the photo, it was necessary to concentrate on optical effects in the region underneath an object with this shape. From my viewpoint, the only way that the discrepancy could be explained was to invoke a physical process in the air underneath the object that was time-dependent in such a way as to give a different image in the short exposure time of the camera as compared with the longer response time of the human eye. This kind of an explanation would obviate the troublesome lack of transparency that was ever-present with a single object in pulsing motion during the exposure.

Possibilities that were considered included a time-dependent refractive or absorptive phenomenon in the air beneath the object with a duration which

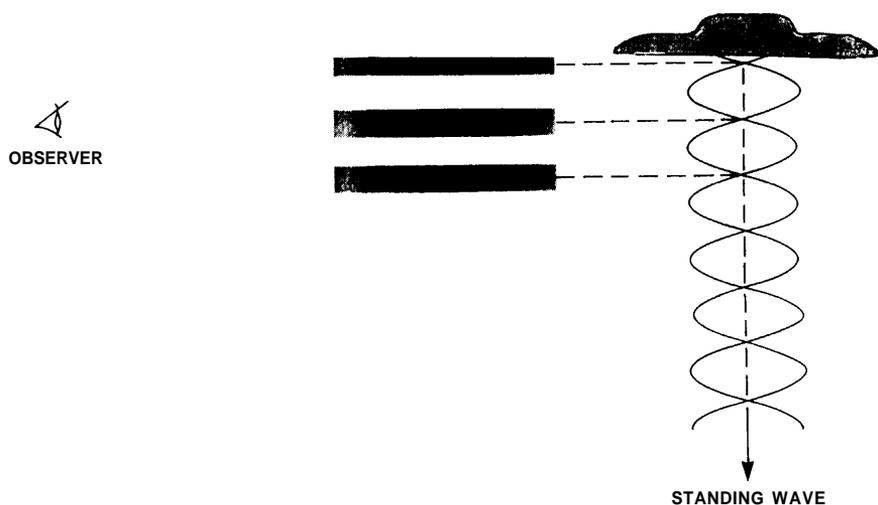


Fig. 3. Correlation between the dark bands of the object and a hypothetical standing wave.

was longer than the stated exposure time (0.01 sec) but shorter than the persistence time of an image on the eye (perhaps as much as 0.04 sec). While this did not leave much leeway it could possibly explain the discrepancy. Some experiments in my laboratory confirmed that a periodic absence of light for a time of 0.03 sec was discernible to the naked eye, while a periodic absence of light for only 0.01 sec was not. These experiments were performed with light from a continuous light source passed through a variable speed shutter and then projected onto a screen. The time dependence of the light on the screen was monitored with a fast detector and an oscilloscope. Thus if the dark bands under the object were dark for only 0.02 sec or less then the eye would not see them. On the other hand, if the camera were open for just 0.01 sec and in approximate synchronism with whatever was causing the time-dependent dark bands then the camera would record the dark bands.

Such an effect would need to be generated by some kind of a standing wave with a spatial periodicity matching the periodicity beneath the object. The effect would need to have a saturation property which could account for the sharp edges at the tops and bottoms of the black areas. Without specifying the nature of the standing wave, or the physical effect it might have on the column of air underneath the object or even whether the terrain below could reflect this wave coherently, the periodicity of the banded structure could be related to a standing wave. Fig. 3 is a schematic diagram of how such a wave could be associated with the appearance of the alternating bands in the photo. Notice that the dark areas under the main image roughly coincide with the areas of maximum change in the hypothetical standing wave. Any explanation using this model would have to include temporary density gradients in the air which could give a "mirage" effect at very small angles of observation, and which

could recover in a time short compared with 0.03 sec. The need for small angles in the mirage effect could explain the presence of only the first two nodes in the photograph.

Some time was spent considering this model but no known physical effects could even qualitatively explain the tiered effect that would be visible only with a short exposure on film and not to the eye. Refractive effects fell short because a dark region⁶ would have to be displaced far in the distance from the column of air under the object in order for the small angles needed for a "mirage" effect to be present. Since the underside of the object was the only uniformly black source in the vicinity of the sighting, and it couldn't be at the large distance from the column of air required for the small angles of the mirage approach, this model could not explain the phenomenon. Nor was there any better chance for time-dependent induced opacity since there were no known effects that could cause intense absorption in air at all the wavelengths covered by film sensitivity. Nevertheless, it was difficult to abandon the "optical effect" model because it appeared to have the potential of explaining the problem.

These ideas were presented orally at the 1981 CUFOS conference in Chicago Ill., but no written version was published in the Proceedings of that conference.

Further Detective Work On The Photos — Sideways Motion?

At this point it was unproductive to review the sighting any further with the witness as nothing new had turned up in repeated interviews. Instead, attention was focussed on the pictures themselves. The author had mentioned during the oral presentation in Chicago that the leading and trailing edges of the object were smeared and allowed some background to leak through and that this could be accounted for by sideways motion of the object. This effect was very clear when viewing the original negative with a bright light behind it and with a magnifying glass. However, the background leakage on the leading and trailing edges was inconsistent with the witness' statement that "the object rose in apparent pulses."

Another factor which made it plausible that the object was moving horizontally rather than vertically was the statement to me by the witness that he shot the picture reflexively when it appeared in his viewfinder. If the object had been rising in pulses to the extent shown by the tiered structure in the photograph during the 0.01 sec exposure, then in the normal human reaction time of 0.2 sec the object would have travelled twenty times as far as the vertical dimension of the tier, and it is extremely unlikely that the object would be on the frame at all let alone so near the center of the photograph. On the other hand, if the object were in horizontal motion in an amount shown by the edge smearing of the image during 0.01 sec, then in the normal reaction time, it could move only a few object lengths which is consistent with the location of the image on the negative. Thus, it was becoming clear that sideways motion of the object during the exposure was a better fit to the evidence than pulsing vertical motion.

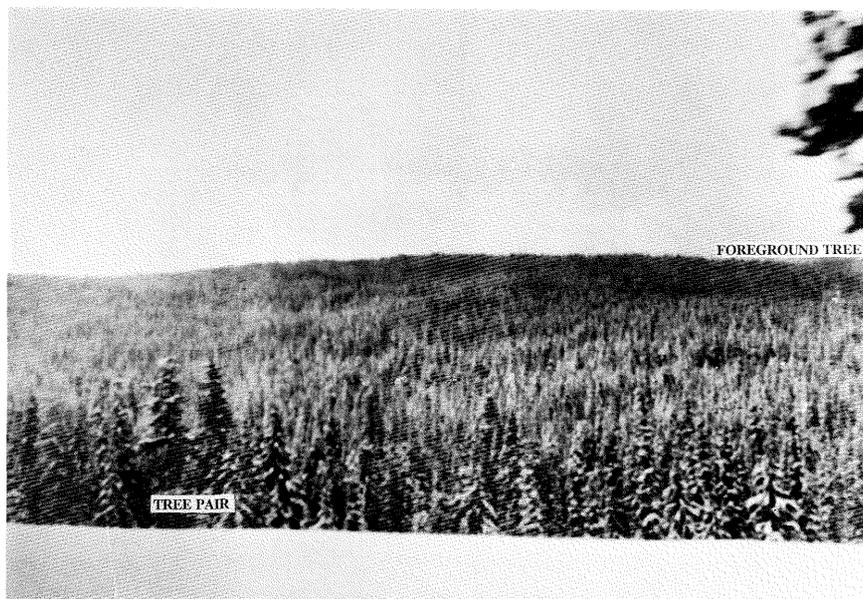


Fig. 4. A full frame enlargement of the first photo taken at the site

Further Interviews With The Witness

In an attempt at understanding the discrepancy between the evidence that the object appeared to be moving sideways and the witness' statement that the object was rising in pulses the witness was questioned once more on this point and he then revealed that he NEVER ACTUALLY SAW THE OBJECT RISING but had only surmised it after seeing the picture. He believed that such a pulsing, rising motion was consistent with the object that he saw. Questioning in other areas produced no further modifications to his original statement and he remained steadfast that he only saw a single cymbal shaped object which disappeared into a cloud bank in the distance. Despite this unwavering testimony of the witness skepticism was beginning to set in.

A Major Discrepancy

With this new attitude, a search for further discrepancies was initiated. Photos taken at the site were studied with a more skeptical eye. Figs. 4, 5 and 6 are full frame enlargements from the original negatives of the pictures snapped at the site. Note that in Fig. 4, which was taken first, there is a part of a large tree visible in the right foreground and a pair of distinctive large trees in the near background to the left of center. In Fig. 5, the second picture taken, these same two trees are identified as having been displaced to the right and there is now a large tree trunk in the left foreground. These pictures were clearly snapped at the same general location but at different angles. Now in Fig. 6 the third pic-

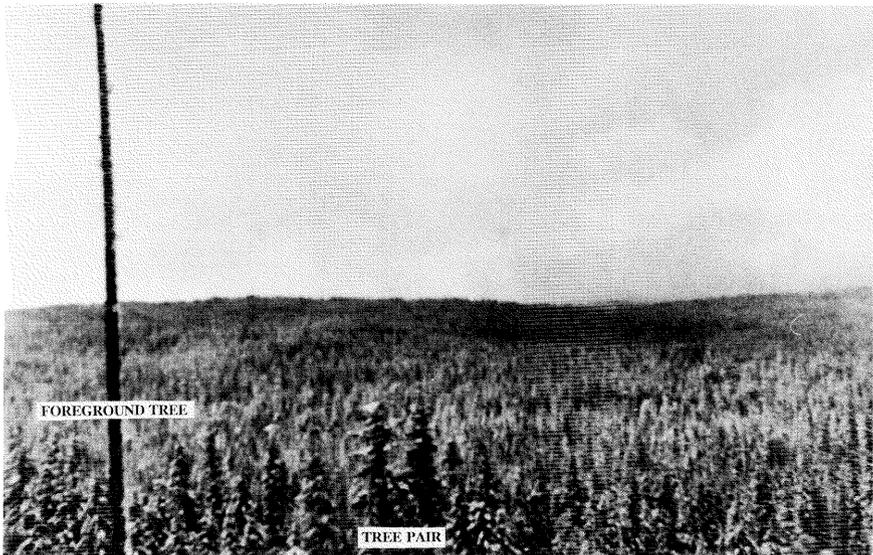


Fig. 5. A full frame enlargement of the second photo taken at the site.

ture which, according to the witness, was also taken at the same location, we cannot find the distinctive tree pair in the near background, and neither foreground tree is present. Furthermore, simple measurements of the distances on the prints reveal that it is geometrically impossible to squeeze a photograph between those foreground trees without one or the other showing.

The witness was asked about this apparent discrepancy, specifically if all the photos were aimed in the general direction of Diamond Peak and whether he took any of the three photos in some other direction. He stated that all photos were pointed at where he thought Diamond Peak was and that at most he changed the camera direction by 5 or 10 degrees. That amount of shift would explain the difference between the first two photos but not the third. Since the horizontal field of view of his camera was about 36 degrees, any shift of only 5 or 10 degrees would require that one or the other of the single large foreground trees be in the field of the third photo, as well as, the distinctive tree pair that was in the first two photos. Since they were not, it was evident that the third photograph was NOT taken at the same site as the first two. This realization completed the transition from the belief mode to the skeptical mode.

Phase II, the Skeptical Mode

A Review Of The Inconsistencies

In early 1982 the problem was put aside, and it was not until late 1989 that the author was again drawn to the investigation. The first task was to reconsider the inconsistencies which had led to skepticism; not only did they remain

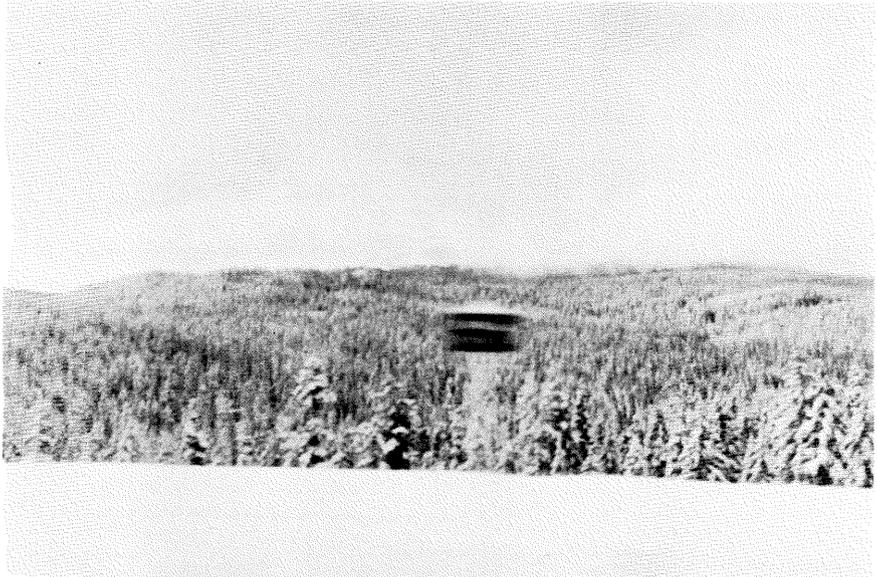


Fig. 6 A full frame enlargement of the third photo taken at the site

but another one surfaced. In addition to the problem of the sideways motion of the object rather than the vertical motion and the further problem of two sites rather than the one site there was a third problem. This was that, according to the date on the original negative, the witness waited for over a month to get the film developed after he returned to his home. Even though it had been believable (to me) that he failed to tell his wife when he got back in the car, it was highly doubtful that he would wait a full month (before he took the film in for processing) to see if the film had captured something as spectacular as he later described.

Oddly, none of these discrepancies had surfaced in the many publications on this photo.

An Object At The Side Of The Road

During the earlier investigation the witness had been asked several times if he had taken any photographs from his moving car during their trip. He was quite positive that he hadn't, so at the time, in the strong belief mode, I abandoned that line of inquiry. However, in view of my newly found skepticism and the strong evidence for a second site, the possibility had to be considered that the witness had been mistaken. Conceivably, he had taken a photo of something from the moving car at a different site along the side of the road. The problem was, where was the second site and what could look like the object that was photographed?

After discussing the possibilities for roadside objects with several acquaintances⁷ the most likely candidate was a road sign. A photo of a sign taken from

a moving vehicle could explain several of the features exhibited by the photo. It could explain the dark bands as spaces between the rows of letters, the "vapor trail" could be the smeared image of the pole supporting the sign, and the general appearance of sideways motion could be due to the motion of the vehicle from which the photo was taken.

The First Sign Fabrication

In order to test the sign theory with an actual experiment from a moving vehicle it was necessary to fabricate a sign. Measurements of the tiered portion of the image on enlargements from the negative yielded the relative dimensions of the tier to be in a ratio of height to width of about 0.7. To arrive at the actual size of the sign one would need to know the focal length of the camera lens which took the photo, the distance to the object, and the size of the image on the negative. For the camera used by the witness the focal length was known to be 50 mm and the width of the image on the negative was roughly 2 mm. Simple geometrical calculations predicted an object width of about 14" by 10" for a distance to the object of about 30 feet. This seemed like a good starting point, so a "sign" was constructed with these dimensions. Since any message which might be on the sign was unknown at the time five equally spaced alternating black and white stripes, of widths about 0.2 of the tier height were used to approximate two rows of lettering with space above, between, and below the rows of letters.

The Parallax Effect

From simple geometrical considerations it can be shown that, from a given observation point, the angular shift of an object due to relative motion of an observer and an object is inversely proportional to the distance between the observer and the object. It can also be shown that if the background is sufficiently far away the apparent displacement of the object on the background is independent of whether it is the object or the observer which moves.

The Speed of the Car

To apply the parallax effect to the Oregon Photo we noted that the background trees behind the object were at least 10 times as far away as a road sign. Thus for the purpose of calculating the required vehicle speed the parallax of the trees could be neglected and the assumption made that the angular shift of the sign would be the predominant effect of the vehicle motion. From the sideways smearing effect an indication of vehicle speed could be obtained if the distance to the object and the exposure time were known. The apparent velocity of the object could then be calculated by translating the amount of smear on the negative to a distance apparently traveled by the object during the exposure time. Assuming an object distance of 30 feet, a sign width of 14" and an exposure time of 0.01 sec, this reduced to an approximated object speed of about 11 mph. Then in accordance with the above discussion on parallax the

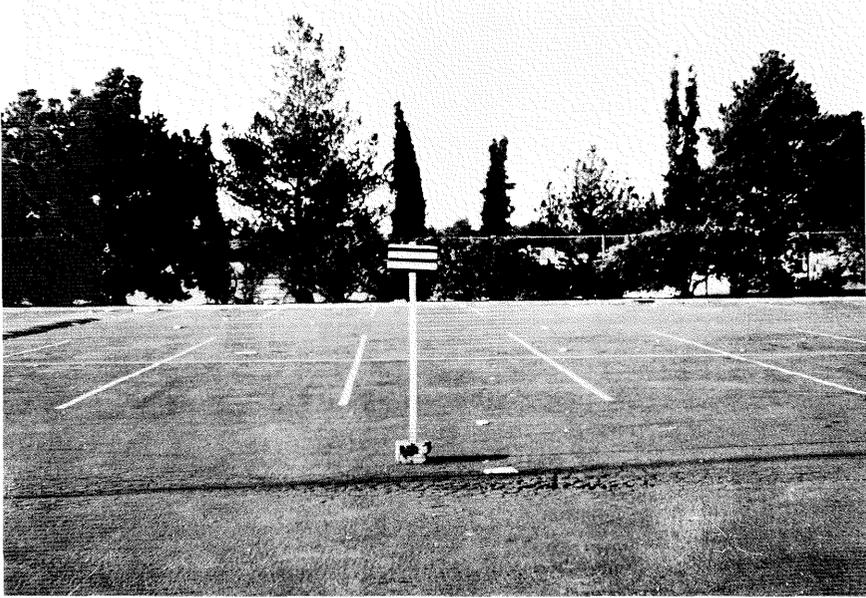


Fig. 7. The first experimental "sign."

same displacement would occur if the vehicle rather than the object were moving at 11 mph.

The width of the vertical supporting pole was arbitrarily chosen to be the same as the width of one of the tiers in order to have a substantial amount of smearing to give the vapor-trail appearance in the original photograph.

The Snow Effect

If the object were indeed a sign then it most probably would have an inch or two of snow on top, given the amount of snow showing on the trees and the size of the snowbank in the foreground. In addition a portion of the vertical pole would most probably be protruding above the sign. With this in mind, a strip of white was added to the top of the tier structure and the vertical support was allowed to protrude above the tier. Fig. 7 shows the resulting fabricated sign in a parking lot just prior to the experiment.

The First Trials

The experiment was performed using a polaroid camera set at an exposure of 0.01 sec. A vehicle was driven^R past the "sign" at various speeds while photographs were taken from the passenger side at each passby. The results were immediate and gratifying. Fig. 8 was taken during the very first series at about 15 mph and a distance of about 30 feet. Many of the features of the famous photo were already there.



Fig 8 The sign of Fig. 7 photographed from a moving car traveling at 15 mph and 30 feet away.

In order to generate pictures a little closer to the original photo, a few minor changes were then made. The white stripes were made shorter and greyer and the vertical pole was also greyed. Fig. 9 was taken at 11 mph and a distance of 30 feet at a different location after these changes were made. It was now abundantly clear that we were on the right track.

A Sign of the Times

The First Site Visit

The results of the first and second series of runs were so close to the original photo that a trip to the site was in order. It appeared that the sign was nearly right and, assuming the witness had given us the right location, a sign of the general size, shape, and distance to the road as the one in the experiment should be readily seen at the site. Of course some twenty odd years had passed since the sighting and things could have changed.

In June, 1989 we made a trip to the site and were disappointed since all that could be seen at the required distance from the road and at the required height were some old metal poles which looked as if they might have once supported signs. There were no signs even remotely similar to the small black and white sign of our experiment or any similar object, at about 30 feet from where a car would be driving by.

It seemed that too much time had elapsed and it was too late to determine the exact nature of the sign. One significant thing we did learn was that there were



Fig. 9. The sign of Figure 7, after modifications, photographed from a moving car traveling at 11 mph and 30 feet away.

two lookout points for Diamond Peak, about 2 to 3 miles apart, both on the right hand side of the road so that, traveling south on route 58, a passenger could take a photo of Diamond Peak from a moving vehicle through a window. The other thing we learned was that the first lookout area going south was not as deep as the second one. These points would prove to be significant in view of developments to come.

The Salem Sign Shop

Without much hope, the possibility was pursued that there might be some way of determining exactly which signs might have been at the two Diamond Peak lookout points at Willamette Pass in November of 1966. The starting point was a friend⁹ who knew some of the people associated with the Oregon Transportation Department. The trail led from district highway engineers, through various veteran supervisors, to people responsible for road sign maintenance, and finally to the sign shop in Salem, Oregon.

The foreman introduced me to various workers in the shop¹⁰ who collectively provided the following information:

- a) There was only one possibility for any sign at the lookout points from 1966 to the present time and that was a large 36" by 24" green sign with two rows of large white letters with upper lettered row showing DIAMOND, the lower lettered row showing PEAK and a vertical large white

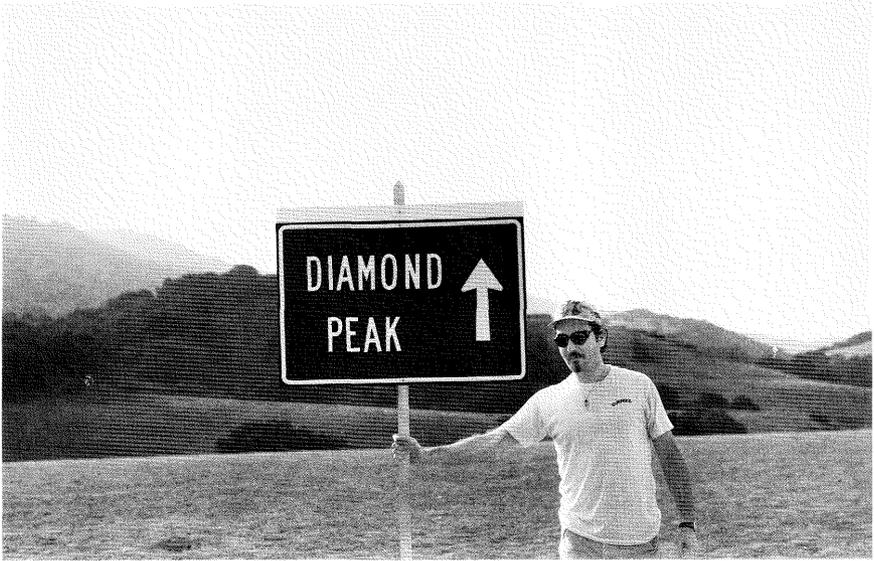


Fig. 10. The first version of a sign fabricated according to the instructions and specifications of the sign shop in Salem, Oregon.

arrow to the right of the lettering. There could be no other other sign because the state highway department had authority over the entire turnout, and that was the only sign that they had there.

- b) This type of sign would be at both lookout points and at the farthest point from the road since the turn off area was plowed during the snowy winter months. Indeed, a snow plow occasionally snapped off a sign pole and there could possibly be times when one or both of the turnouts had no signs at all.
- c) There had been only minor modifications in the sign since 1966, mostly in the reflective power of the letters and borders. The basic design of the sign had remained the same.

They also provided an exact dimensional layout of the sign including letter templates, an arrow template and a piece of an old sign painted the color that was used in 1966, in short, sufficient information and material to be able to recreate the original sign.

A New Problem

The information from the Salem sign shop presented a new set of problems. The original experimental sign was a much smaller and closer black and white sign with only "lettering" in comparison with the actual sign at the site which was green and white, much larger and further away and had an additional complication of the large vertical arrow on the right side. At first these differences

seemed overwhelming and it appeared unlikely that this sign was what the witness photographed. We had not even noticed such a sign at our first site visit or if we did we had dismissed it out of hand.

However, the color of the sign really didn't matter since the witness had used black and white film and the green sign background could easily appear to be black. The size and distance of the sign were of no special consequence either since there was probably sufficient room at one of the lookouts to double the original estimate of the distance. The arrow could be a problem, however, as it was difficult to see how relative motion could erase all that white vertical image and leave the dark bands intact.

With all this in mind a sign was fabricated using the specifications provided by the people in the sign shop. As before a white strip was added to the top of the sign and the pole allowed to protrude above the sign. A first version of the sign is shown in Fig. 10.

The New Parameters

Now that the precise dimensions of the sign were known the exact distance from the vehicle to the sign could be established by measuring the image on the negative or, preferably, a full frame enlargement. Working with the frame that contained the object, and using the relatively unsmearred vertical dimension, a reliable measurement could be made on an enlargement. The enlargement scale factor could then be determined by comparing the dimensions of the frame in the enlargement with that of the frame on the negative. Using this procedure, the calculated distance from the object to the camera was about 58.6 feet.

The next parameters that were needed were the vehicle speed and shutter open time. As discussed above these two parameters are linked as follows:

$$\text{apparent velocity} = \text{displacement/exposure time}$$

The frame containing the object was considerably darker than the other two frames taken at the site. Thus we can guess that even though the two frames taken at the first turnout were probably exposed for 0.01 sec as the witness stated, the darker frame was probably exposed for a longer time. For cameras such as the one used by the witness (a Kodak 35mm) the next longest available exposure times are 0.02, 0.033 and 0.0625 seconds. From measurement of the smear on the print enlargement, the displacement (at the object distance) was calculated to be roughly 8 to 12". The uncertainty arose because of the difficulty in deciding where the smear began and ended. Using 10" as a compromise we arrived at a velocity of 500" per sec for the 0.02 sec exposure, 300" per second for the 0.033 second exposure, and 160" per second for the 0.0625 sec exposure. These translate to 28.4 mph, 17.04 mph, and 9.09 mph respectively.



Fig. 11. The sign of Figure 10 photographed from a moving car traveling at a nominal speed of 15 mph and 60 feet away.

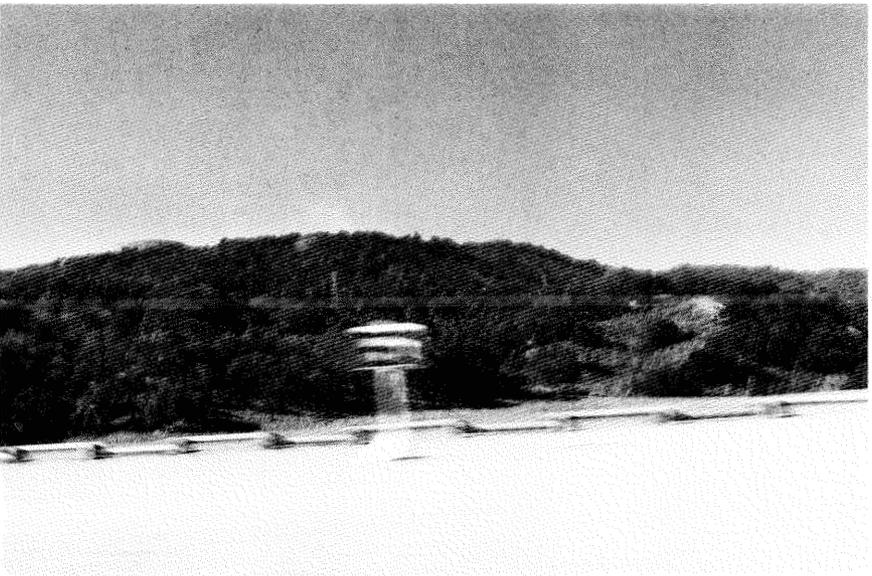


Fig. 12. The sign of Figure 10 photographed from a moving car traveling at a nominal speed of 22 mph and 60 feet away.



Fig. 13. The sign of Figure 10 photographed from a moving car at a nominal speed of 30 mph and 60 feet away.

Experiments With The New Sign

During the period from Nov. 1989 to Jan. 1990 several runs¹¹ were attempted at various locations in the S.F. Bay Area using a Nikkormat camera loaded with Kodak Panatomic-x black and white film (The same film as used by the witness). Several new problems were encountered and solved during that period. For example, with the increased area the sign would topple over in the prevailing Bay Area breezes before we could do the driveby. This required a redesign of the mount with a much heavier base. Next there was a substantial variation in the amount of smearing from a given set of conditions. This resulted from the uncertainty in attaining and maintaining a constant speed in a confined parking area. Finally, little irregularities in the road caused jiggling of the camera during the exposure thus skewing the image. These problems were solved by selecting areas with a longer approach so that speed was in a steady state at the time of the exposure, and taking six exposures for each set of conditions in a given run, in the hope of obtaining at least one good photo out of six.

Our most useful sequence during that period was taken at about 60 feet and 1/30 sec with the results shown in Figs. 11, 12, and 13 at nominal car speeds of 15, 22, and 30 mph. One can see that at a high enough speed the vertical arrow completely disappears. On the other hand, at the highest speed the degree of smearing is too much. Also, there is an extra white line at the bottom and the trail below is too bright. Despite these minor problems this seemed to be the sign that the witness had photographed, and renewed efforts were made to ob-



Fig. 14. Photo of the sign at the first turnout at Willamette Pass ca. Jan., 1990.

tain a photograph of this sign (from a moving vehicle) which would be undisputably close to the original. However, the minor differences proved difficult to eliminate and persisted despite numerous changes in conditions.

Before spending any additional effort to close the gap between the initial results and the famous photo, it seemed appropriate to verify that all the details of the sign were correct. Accordingly, arrangements were made to get a current photo of the sign at the site". When the photo arrived (Fig. 14) it was compared with the fabricated sign (see Fig. 10) and the only significant differences were that the post of the sign at the actual site was wider and quite a bit duller than our fabricated sign post, probably because most of the paint had peeled off. There was no way to establish what the state of the paint on the sign post was in 1966 so that it seemed fair to adjust the brightness of the post to whatever was needed to get the proper effect. The width of the post was increased to the 4" as called for by the specifications for a 1966 sign (currently 5" posts are used) from the Salem sign shop. Finally, the top of the post that was protruding was modified to make it more rounded as it would have been at the time of the event with an inch or two of snow on the sign.

After making these changes a new series of runs was made which incorporated the notion that the original camera had been out of focus and that lighting conditions were different from anything that had been tried so far. In April 1990, on the tenth run we were able to generate the photo shown in Fig. 15. This has all the features of the original photograph including the substantial



Fig. 15. Photo of the sign in Figure 10 after minor modifications taken from a moving car at a nominal speed of 15 mph and 60 feet away. Foothill College parking lot ca. April 1990.

suppression of the bottom white line and the vertical arrow. This was accomplished through choosing a cloudy day, deliberately defocussing the camera, and underexposing the film. The photo was taken with black and white Panatomic-x film at a speed of 15 mph at 60 ft., with an exposure time of 1/16 sec and with the f stops deliberately set at up to two stops below what the light meter requested. It was my belief that we had reached the point of diminishing returns and that this was as close as we could get to duplicating the original photo in a reasonable amount of time.

A Comparison of the Newly Generated Photo With the Original

While there are still minor differences between the two photos such as that the dark bands in the original photo are somewhat sharper and darker and the "vapor trail" is more diffuse, it is worth pointing out that in addition to the overall similarity of the two photos there is one very crucial detail which appears in both photos. On the extreme right of the object, seemingly connecting the two lower light bands, is a faint vertical white arrow. This is clearly a residual image from the white vertical arrow on the sign at Willamette Pass and most certainly shows that this sign was indeed what the witness photographed. Significantly, although this feature had been noticed during Phase I, it had been interpreted by me as an extension of a tree (which coincidentally appears just below the right side of the object) showing through the edge of the object.

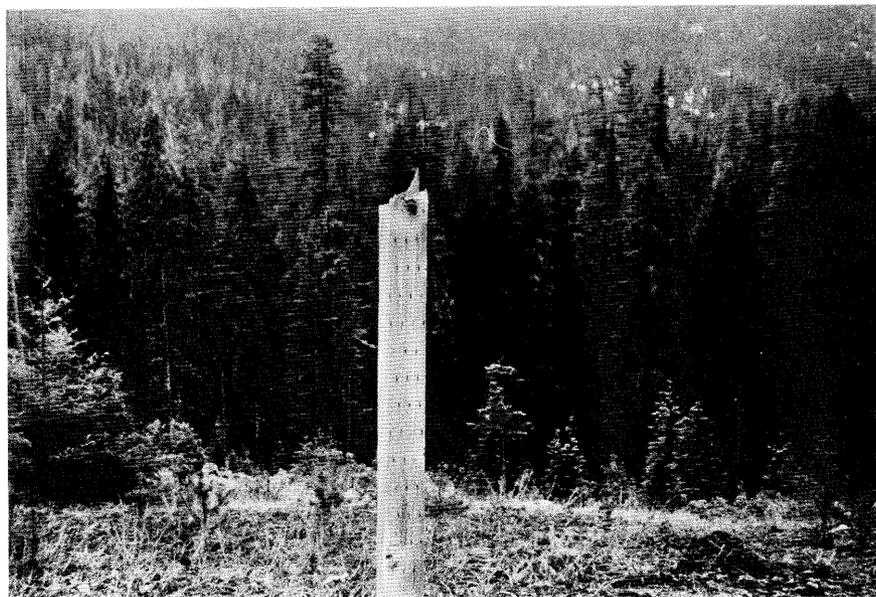


Fig. 16. Photo in the second turnout at Willamette Pass of the broken post which normally supports a sign identical with the sign in Figure 14 ca. April 1990.

A Final Trip to Willamette Pass

There were still some lingering questions about the two sites and how the actual event might have unfolded. Accordingly, we packed the camera equipment into the car and headed up to Willamette pass for a final look. One objective was to verify that there was a sign at the correct distance and a second objective was to take photographs at the actual site of the actual sign if possible.

We arrived at the site in the early morning and it was dark, drizzling and wet. At the first site going south from Eugene we found the sign that we expected. We then measured its distance from the road where a passenger would be in a car driving south and found the distance to be about 40 feet. Moreover, the turnout was not deep enough to have a sign any further away from the road. Recall that we had established earlier that a distance of 58.6 feet was required to obtain the image size measured on the negative. This clearly ruled out the first site as the one where the photograph was taken. Despite this we attempted to take pictures from our moving vehicle and found that in addition to being at the wrong distance the sign was also at the wrong height.

We proceeded down the road to the second site and found no sign at all, despite the fact that one should have been there according to our information. However, we did find the stub of a 5" by 5" post identical with the post supporting the sign at the first site but broken off presumably by a snow plow as the technicians at the Salem sign shop had mentioned did happen occasionally. A

measurement of the distance from the passenger side of a car which would be traveling south on the road to the remaining stub revealed a distance of about 60 feet. Fig. 16 is a photo of the post stub as of April 1990. We conclude that this post or one like it at this very spot supported the sign of the photograph.

A Review Of What Was Established

As a result of this investigation we have shown that the object in the photo was indeed a specific sign in a lookout point. This was accomplished by establishing that:

- 1) The only sign that could have been in the lookout point was a specific sign identifying Diamond Peak in the distance.
- 2) There is usually one of these signs at the correct distance of 60 feet from a southbound car on the road (consistent with the image size on the original negative) at the second turnout.
- 3) Such a sign when snowcapped and photographed from a moving car 60 feet away yields images which contain all the significant features of the original photograph.

A Reconstruction of the Events

Background

While it is impossible to know precisely how or why this event happened it is of some interest to speculate on the sequence of events that led up to it.

The Taking of The Photographs - A Possible Scenario

The witness most probably took the first two pictures at the first site just as he described. However, instead of taking the third photo as he stated, he got tired of waiting in the cold and got back into the car. They proceeded south with his wife driving and as they passed the second site he noticed that the clouds around Diamond Peak were lifting, and he snapped a picture from the passenger side of the moving vehicle. From the results of our experiments it is most likely that the camera was set at 1/30 of a second and that they were traveling at about 30 mph. (It is also possible that the camera was set at 1/50 of a second and that they were traveling at 50 mph). He may not have noticed the sign or if he saw it he may have wanted to frame the sign so that the vertical arrow was pointing right at Diamond Peak.

The Photograph

When the photos came back from the processor over one month later, the witness noticed how puzzled his wife was about the object in the photo. After examining the photo himself, he evidently decided that he must have taken a picture of a UFO. For reasons known only to the witness, at some point he started to claim that he actually saw the top portion of the photo first in his

viewfinder and then with his naked eye moving silently and swiftly from left to right.

A Non-Premeditated Hoax

It is likely that the witness never originally intended this as a hoax at the time he took the photo. Indeed, since he captured the picture in one passby and it took over 100 tries to approximate his photograph one would have to conclude that the photograph was taken by chance. Since he properly identified the location this would also seem to rule out a deliberate premeditated hoax. This would not, however, rule out the possibility of an instantaneous decision to hoax when he first saw the photo.

For whatever reason, once he started down this path he never wavered.

Implications for UFO Research

Investigator Polarization

In this investigation progress was hampered for some time because the author stayed in a pure belief mode. What is especially revealing is that the author remained temporarily oblivious to an abundance of evidence that should have signaled something was wrong. If anything can be learned from this, it is that UFO researchers need to be more diligent in applying the principles of scientific research. As is well known but not always applied, this means that one must be dedicated to the truth and be willing to accept the results of an inquiry without personal bias.

Witness Reliability

This work demonstrates that our criteria for judging the reliability of a witness are inadequate. In this investigation the witness was judged on the basis of his high level of education, his level of attainment and special training in the military, his professional success, and his desire for anonymity. All these factors contributed to a highly credible witness according to conventional wisdom, and were key to the author stubbornly staying in the belief mode. It is not obvious that anything can be done about improving this situation. If a witness with these credentials cannot be believed then it appears that the concept of advancing a field on the basis of witness reports may be flawed.

The Effect Of Witness Reliability On Research Results

In the case of statistical studies of the UFO phenomenon there is a danger that by simply tabulating the number of sightings with a particular attribute, such as time of day or some observed side effect, any conclusions that are drawn may be skewed by the possibility that the overwhelming majority of the data points may be false or inaccurate. This problem may be addressed by choosing only cases with uncoupled multiple witnesses. While this would

greatly reduce the size of the statistical samples it might also increase the chance of meaningful results.

In the case of alleged contacts with and abductions by extraterrestrials we have a much more serious problem. Because of the very nature of the field the evidence is gathered predominantly from the perceptions of single witnesses. Even assuming that the majority of these witnesses believe what they are saying, there is the problem of determining if some or all of their descriptions are generated by internal rather than external stimuli. The task of formulating valid criteria for believing such witnesses may be formidable if not impossible.

Footnotes

1. The first investigator who interviewed the witness on behalf of any UFO organization was Paul Cerny. From his notes and correspondence with the witness, he provided me with information about the witness and the event.
2. The Nixon references listed below were published in journals affiliated with The National Investigations Committee On Aerial Phenomena, Inc. (NICAP) which ceased to exist ca. 1980.
3. Tom Gates and Paul Cerny who was at that time regional director of MUFON.
4. The photo processing and enlarging was performed in his darkroom by Zev Pressman, a professional photographer and associate. He continued to contribute his skills throughout the course of this investigation whenever special photographic processing was required.
5. The digitization had been done at Cal. Tech. in Pasadena, California by Dr. Robert Nathan.
6. The requirement of a black area somewhere in the direction of observation in order for a mirage type effect to occur was pointed out by Prof. Peter Sturrock during discussions with him ca. 1981.
7. From a time very early in the investigation, Al Reed who had been contributing artists' renditions of various UFO sightings to the UFO community suggested that the object might be a road sign but the suggestion was ignored at that time since the author was still in a strong belief mode. Mr. Reed also contributed his time and the use of his Technical Publications facility to generate all the transparencies used for the oral presentation at the 1981 CUFOS conference.
8. The driver for these initial experiments was Lois Joan Wieder.
9. Lucretia Z. Sarles who at that time was a member of the Oregon Traffic Safety Commission.
10. Darryl Austin was particularly helpful in this matter.
11. For these and subsequent experiments the vehicle driver was Adam Wieder.
12. Melanie and Michael Greenberg were enlisted to drive down to the first lookout point from their home in Eugene, Oregon, and they took several pictures of the sign at that location.

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LETTERS TO THE EDITOR

Vallee Comments on Book Review "Revelations: Alien Contact and Human Deception"

A few minor discoveries triggered by the publication of *Revelations* (which were reviewed in a recent issue, JSE Vol. 7 No. 1, page 99) may be of interest. Commenting on the sensational claims of Bill Cooper, regarding scientists who could substantiate the existence of aliens on earth, my book had observed that no physicist by the name of Krill or no botanist by the name of Mendoza could be found. I offer the following corrections here in the interest of historical accuracy.

1. One of my readers has found a physicist named Krill. He is quite human, his full name is Jerry A. Krill, he obtained his Ph.D. in electrical engineering at the University of Maryland and he has received a patent for a high-power, low-loss circular waveguide design. His papers on electromagnetic scattering have appeared in the *IEEE Transactions* and in the *Journal of the Optical Society of America*. I hasten to add that this particular Dr. Krill has no relationship with, and indeed no interest in, the UFO controversy in general or Bill Cooper's activities in particular.

2. Another one of my industrious readers, Bill Jones, has nailed me for stating that there was no Dr. Mendoza. Such a person has been discovered in *American Men and Women in Science* (Physical and Biological Sciences, 14th edition, R. R. Bowker Co., 1979). Born in Mexico City on 5 July 1909, Guillermo Mendoza received his Ph.D. in zoology from Northwestern University in 1937. Any delightful speculation that his presence there might have overlapped with Dr. Hynek's (or mine, for that matter!) must be discounted since he left Evanston in 1943. He spent most of his career at Grinnell College in Iowa. However, according to the December 1991 issue of the *Ohio UFO Notebook* "he was not a botanist. The index *Biological Abstracts* notes eleven article by Dr. Mendoza from 1937 through 1967. A review of *Science Citation Index* from 1965 through 1978 notes eleven additional references. All of these articles are zoological in nature."

In response to inquiries, Dr. Mendoza has repeatedly indicated his puzzlement as to how his name had been linked to the UFO problem, especially in connection with the human-like entities popular in the current "crashed saucers" literature, adding "obviously, aliens developed on extraterrestrial bodies cannot very well have human or human-like characteristics."

Jacques Vallee
1550 California St. #6L
San Francisco, CA 94109

Comments on James Wilson's Letter to the Editor

I would like to respond to Mr. James Wilson's letter to the editor ("Are memories of alien abduction recollections of surgical experiences?") which appeared in JSE Vol. 6, No. 3 (p. 291). While I think Wilson may be on the right track to explain *some* UFO abduction reports as vestiges of vaguely remembered outpatient surgery, I also think he's missed the mark by overlooking the obvious. In the process, he's forced to draw some rather tortured and tenuous parallels. In comparing the abduction experience with contemporary out-patient procedures, for example, he suggests that instances of reported telepathic communication on the part of the abducting aliens can be "explained," or equated with, the fact that "surgical masks obscure lip movements." It seems much more logical to interpret "telepathic" communications as a literal metaphor for an interior dialogue, i.e., a voice simply heard in the abductee's head, and most probably of the abductee's own making.

Similarly, for "female abductees [who] recall seeing babies," he offers the suggestion that "women patients awaiting admission often look at newborns in the hospital nursery," which seems wholly gratuitous at best. I leave it to medical professionals (and out-patients) to weigh in with their own experiences, but it strikes me as highly unlikely that most – if any – patients would pass through the hospital nursery for a casual peek on their way to or from surgery or any other treatment. Moreover, Mr. Wilson offers no compelling reason as to *why* all these haphazard procedures should later coalesce (or is it coagulate?) into the allegedly coherent sequence of events reported by many abductees, which includes the extraction of sperm and ova, resulting in so-called "missing" fetuses and, ultimately, the production of an alien hybrid baby.

There is, however, a specific out-patient procedure that contains many of the criteria outlined by Mr. Wilson, and that of course is the act of abortion. While it is true that modern medical science has progressed to the point where the average abortion no longer requires anaesthesia, local or general, abortion of whatever difficulty has an inherent psychological component to it obviously absent from say, having a gall or kidney stone removed, which is typically a matter for relief. This psychodynamic component may, or may not, involve a crisis of conscience, including lingering feelings of shame, remorse and/or guilt, which may surface months or even years after the event. The abduction experience, then, could be a dynamic internal drama aimed at absolving personal guilt.

To see why this may be so, it's first interesting to note that the "standard" abduction scenario as originally outlined by folklorist Thomas Bullard in 1987, has in fact undergone a profound change in the intervening years. At that time there was not a single instance of a hybrid baby in any of the nearly 300 cases in the global UFO literature that Bullard analyzed. The same year, however, also saw the publication of Budd Hopkins' *Intruders*, and it is here that "missing" fetuses first surface and that Bullard's general medical examination takes on sexually-charged overtones, including the forceful extraction of sperm and

ova, and even actual physical rape. In addition, what was once a general "tour" of the alleged UFO's technical innards, now frequently includes a nursery, or incubatorium, where hundreds of hybrid babies with a sickly, "premature" appearance may be housed.

Intruders was slightly preceded in the same year by Whitley Streiber's *Communion*, which also contains a strong sexual component, followed by the same author's *Transformation* (1988), and finally historian David Jacob's *Secret Life* in 1992. In the same interval, estimates of the annual worldwide abduction rates have exponentially swollen from the hundreds and thousands to the millions.

Read phenomenologically, the "average" abductee tells a story of being floated from their bedroom or automobile late at night, and usually in solitary circumstances, into a relatively small and evenly lighted "operating" room inside a larger object (the UFO). Here they are subjected to medical procedures that focus on the extraction of genetic material (sex). Later they may be abducted again, shown a hybrid baby (the "missing" fetus) and asked to physically nurse it.

In the same vein, of course an abortion *is* a case of a "missing" fetus, both figuratively and literally. Interestingly, the Grays, the aliens allegedly conducting the abduction and presumably engaged in genetic engineering, are often explicitly described in terms of human fetal imagery, from their slim, smallish limbs and oversized craniums and eyes, to their smooth, babylike skin and absence of obvious genitalia. Within acceptable realms of "dream" psychology and symbolic interpretation, it's probably not too far-fetched to see the Grays as psychic projections assuming the literal form of the aborted fetus. Metaphorically, and to engage in a bit of hyperbole perhaps, they are "avenging angels" (since they come from "heaven"), re-enacting the "sins" of their parents, i.e. abduction and abortion. Put another way, they are the "souls" of aborted fetuses, "Gray" because they are asexual and as yet physically unformed or complete, gray being the absence of any "color" (personality, sexual orientation, etc.) whatever, including black and white.

The hybrid baby can then be seen as the embodiment of guilt itself. It isn't really dead, after all. In fact, the "missing" fetus lives on – in heaven and aboard a "mother" ship yet! Significantly, the only way it can be nursed and seen again is to be reabducted. In other words, abductions ending in the production of a hybrid baby can conceivably be seen as an attempt to expiate or alleviate individual guilt over the act of abortion. Such an interpretation would theoretically provide a psychological motive for undergoing repeat abductions. As unpleasant as the experience might be, it's less unpleasant than "knowing" that your fetus is still alive out there somewhere.

I submit that the theory can also be tested to a certain degree, too. For instance, it predicates that the abduction experience is a response to a crisis of conscience over the act of abortion. Individuals less conflicted about abortion should show fewer abductions per population than those highly conflicted. Alternatively, individuals who do report repeat abductions should score signifi-

cantly higher in terms of conflicted attitudes toward abortion than individuals who don't report abductions at all. Theoretically, they should also have more of a "history" of abortion than their counterparts. Culturally, societies with a higher rate of conflicted attitudes towards the abortion of the fetus should demonstrate higher abduction rates.

The key here, of course, is internal *conflict*. Both Spain and Italy, for example, are predominantly Catholic countries, yet they also have the two lowest ratios of members-per-family in all of Europe. This suggests that either Spaniards and Italians have mastered the rhythm method, or else they have learned to psychologically adapt as a society in general to Papal prohibitions against birth control pills and abortions. America, by contrast, is notoriously conflicted over the abortion issue, and America is also home to the "modern" abduction phenomenon. If abductions are predominantly a physical, extraterrestrial phenomenon, then one needs to explain why the Grays seem to "prefer" Americans over other nationalities and gene pools. One must also ask why, if millions of Americans are abducted by aliens annually, the "evidence" for this massive invasion from outer space tends to surface mainly in the commercial UFO literature? Our Air Force may be good at covering up a lot of things, but they aren't *that* good.

Even if a conflicted American psyche isn't the original *cause* of the abduction and hybrid-baby experience, the psychological and phenomenological resonances between abortions and abductions can't be readily dismissed. Now that the abduction "template" exists as a cultural artifact, who's to say who can and can't latch onto it, and for whatever reasons?

Dennis Stacy
Editor, MUFON UFO Journal
Box 12434
San Antonio, TX 78212

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BOOK REVIEWS

Quasars, Redshifts and Controversies by Halton Arp. Berkeley: Interstellar Media, 1987, 198 pp. (hardback). ISBN 0-941325-00-8. \$19.95 (available from The Sourcebook Project, P. O. Box 107, Glen Arm, MD 21057).

In the early 1960s the first quasars were recognized and named: objects in the sky that look like bluish stars except for their peculiar spectrum. Unlike that of a star, the spectrum is characterized by very broad emission lines that are strongly redshifted with respect to the normal frequencies at which the elements radiate. Soon many more of these objects were found, each with its own, and different, redshift. Since the work of Edwin Hubble in 1924 it was known that ordinary galaxies are also redshifted (except for a few of those closest to us, like the Andromeda nebula). The further away from us, he found, the larger the redshift.

When Hubble announced this discovery, it was immediately interpreted in terms of the Doppler effect: the other galaxies are moving away from us, the faster, the further away they are. The universe expands. This interpretation was also immediately endorsed by Einstein, whose theory of gravitation predicted that the universe either expands or contracts, a static one just balanced between expansion and contraction being unlikely (though not impossible).

Against this background, the redshifts of the quasars discovered 40 years later were interpreted in the same way. This is called the cosmological interpretation: using Hubble's relation, the distance of a quasar can be derived from its redshift. But there appeared to be a problem: the redshifts of quasars were much larger than those of the most distant galaxies measured at the time. Several quasars are now known with redshifts ("z") greater than 4, meaning that the wavelength of light we receive from them is five times longer than from the same atoms measured on Earth. So quasars had to be very distant, many of them near the edge of the presently visible universe, and therefore enormously luminous to be visible from Earth. Also, their spectra were quite unlike those of normal galaxies. Were these objects really comparable to galaxies? Couldn't they be something new? Could not the redshifts be caused by something else? Other possibilities were quickly raised: these things, whatever they were, could be at the bottom of a deep gravitational potential (causing gravitational redshift, as described by Einstein's theory of gravity), or maybe the photons from the object had somehow "aged" by an as-yet-unknown agent on their long journey to us (the so-called "tired light" hypothesis).

Though a majority of the astronomical community favored the cosmological interpretation from the beginning, by analogy with galaxies, other possibil-

ities were socially acceptable in these early days, at least in informal discussions. This soon changed and the community divided, asymmetrically, into two camps. As more and more observational details became known, the majority became convinced of the cosmological interpretation, in particular when objects with properties intermediate between those of quasars and ordinary galaxies (the so-called Seyfert galaxies) were studied in more detail. The minority view, that quasars and their redshifts were really something different, became less and less tolerated. A controversy was born that lasts until today.

The book reviewed here describes the arguments for the minority view in the debate. In the first eight chapters it reviews a number of independent observations collected by the author and others that are in conflict with the cosmological interpretation. The tenth chapter gives the author's view on the "sociology" of scientific controversies, illustrated by his own experience as well as a few other disturbing cases in the history of astronomy. The last chapter briefly discusses possible theoretical interpretations of the anomalies. This chapter draws attention especially to the theories of Hoyle and Narlikar, in which the universe expands but not from a big bang. The main thrust of the book is on the observations, however, which are carefully kept apart from possible theoretical interpretations.

The controversy started when the author of the book, a well-known and highly respected observer who had made several important discoveries with the telescopes on Mt. Palomar, became convinced around 1966 that quasars are not randomly distributed on the sky. Instead, there were some groups of quasars, containing objects with different redshifts, associated with a *nearby* ordinary galaxy of low redshift. If these quasars are associated with a nearby object they have to be nearby themselves, the reasoning went, and hence their large redshifts are not cosmological but would have to be due to some new effect. (This is not quite as straightforward as it might seem, though, since apparent associations could also be caused by a gravitational lensing effect. Both sides agree, however, that this effect is far too small to explain Arp's associations.)

The significance of these associations was doubted by many contemporary astronomers. A higher density of identified quasars at some place in the sky does not mean that the actual density is unusual there, the argument goes, because some places in the sky are observed much more carefully than others. Nearby galaxies are intensively studied objects, and unusual bluish objects that happen to be in a field studied for other reasons may or may not attract the observer's interest, depending on his inclination and the nature of his study. In practice, it is impossible to quantify how intensively a particular area of the sky has been searched, and so it is difficult to assign a meaningful statistical probability to an association of relatively infrequent objects like quasars. This objection is raised by the "establishment" against Arp's associations which, at face value, are undoubtedly quite striking.

In addition to these associations of quasars with galaxies, the book describes other anomalies, all inconsistent with the cosmological interpretation. This is not the place to describe them all in detail, but here are three of them.

1. The quasars associated with galaxies sometimes occur as "jets", i.e. they concentrate in one direction with respect to the galaxy, as if they had been ejected from it (which is, indeed, the explanation suggested in the book). In these groups, as well as for quasars in general, those with the highest redshift tend to be the faintest. In Arp's interpretation, this is an "ageing" effect: he speculates that quasars may start their life as faint, high-redshift objects that mature into bright ones of low redshift. The high-redshift quasars would thus have been ejected recently from their parent galaxies. The conventional interpretation is that high-redshift quasars are faint just because they are distant; its adherents argue that Arp's need for the additional assumption of an "ageing" phenomenon speaks against his view.
2. Arp finds that the large-scale distribution of quasars across the sky is not as uniform as it should be if their distances were cosmological. Here we have again the difficulty that existing quasar catalogues are the result of highly selective, non-random sampling, so that the observed distribution cannot be used at face value. But why not take a random piece of sky and sample it in a truly unbiased way? This has indeed been done, by Arp himself as well as by others, and he shows some of the results in chapter 5. These are results about whose statistical significance the reader himself can make a fairly independent judgement. The need for large unbiased samples of quasars exists also in conventional cosmology and quasar research. The large amount of telescope time and work needed for such projects has made them unpopular until recently. Projects of this type, one of them using a telescope dedicated for the task and equipped with an automated data-collection and quasar-identification system, are currently under way. One may hope that they will settle the debate about the uniformity of the distribution of quasars.
3. Evidence that some nearby galaxies have anomalous redshifts (i.e. deviating from Hubble's relation). Arp shows several clusters of galaxies, especially unusual "disturbed" galaxies, where one or more have a redshift that deviates strongly from the others. Now a certain spread in the redshifts inside a cluster of galaxies is always observed, corresponding to random velocities of typically a few hundred km/s, and believed to be due to the mutual attraction of the galaxies in the cluster by the force of gravity. As expected in this interpretation, the velocities are higher in the more massive and the more compact clusters. Deviations exceeding the velocity of escape from the cluster cannot be explained in this way, however. In the conventional view, these must be due to chance coincidences of a background object with a foreground cluster. This view is supported

by the fact that the high-redshift galaxy usually appears smaller and the stars in it (if they can be resolved individually) fainter. Arp acknowledges this, but also finds evidence of a physical connection: bridges of gas, and evidence of mutual disturbance. He takes the view that the contradictory evidence here should be an incentive for further study, because it might lead to the discovery of fundamentally new physics.

4. Evidence for *quantization* of redshifts inside groups of galaxies. Smaller clusters of galaxies like our own local group contain many small companion galaxies in addition to a few big ones. Plotting a histogram of their measured velocities (from the red- or blue-shifts), W. Tifft noted a periodicity: the velocities occur in multiples of about 72 km/s. Arp confirms this effect. If real, this is perhaps the most disturbing anomaly, because it requires much more than some new cause of redshifts. Even if such a new cause produced, say, redshifts at multiples of 72 km/s, the random velocities of the galaxies in each other's gravitational fields should still completely smear out that periodicity. The opposition from the establishment is particularly severe here, and centers again on the statistical significance of the data. The histograms are shown in the book, and the reader may judge for himself.

The last kind of phenomenon discussed in the book is the jets seen emerging from many galaxies. They are seen especially in radio observations, but some are even visible in normal light. Spectacular pictures like those of Cynus A and the M87 jet feature in all popular books on astronomy. These observations are not central to the controversy, since the difference between Arp's view and the conventional one is only partial in this case. Both sides agree that one is here witnessing the ejection of gas from the nuclei of galaxies. Controversial is only Arp's view that the jet phenomenon is related to his observations of quasars expelled from galaxies.

As with anomalies in other fields, the debates surrounding Arp's findings rapidly get bogged down in arguments about statistics. This starts with disagreement about what null hypothesis should be tested. In the case of associations of quasars with galaxies, for example, the conventional view is that one should test the hypothesis that quasars are distributed uniformly, whereas Arp argues that this masks the most interesting effects and that one should test the hypothesis separately for quasars of redshift around $z=1$, where he finds the most striking effects. Not making use of this fact, he says, is throwing away the most important piece of information, the piece that could tell us something new about physics. All just artifacts of *a posteriori* statistics, the majority replies; and so on. This kind of debate will sound familiar to readers of this journal. It is important to remember, though, that *accidental* associations of objects in the sky, of high *apparent* significance, do occasionally appear within conventional astronomy. A famous recent case is that of Sco X-1, the brightest steady X-ray source in the sky. It is a binary containing a neutron star, not too far from us in our own galaxy. Since it is also a radio source, its position

can be determined with extremely high precision. Its radio map shows two blobs, symmetrically on either side of the central source: Sco X-1 emits jets (the only source of its kind known to do this). A significant literature with models of these jets followed. Then, after years of observation the central source had moved away from its position between the blobs, and it became clear that this was a chance coincidence with a background object, one of the numerous double-lobed extragalactic radio-sources. The a priori chance of such an alignment was computed to be 1 in 100,000. A colleague once told me of a periodicity he found in a time sequence of X-ray data, at a formal level of significance of 9 times the standard deviation (σ) of the background. It disappeared on closer examination. Hundreds of plates and CCD frames are scanned every day by trained eyes eager to find something unusual worth publishing; this has to be taken into account when significance levels are quoted. In fields of astronomy where large amounts of data are processed routinely (by computer), high standards for accepting something as real have been found necessary. In radio and X-ray maps of the sky, apparent sources that stick out less than 5 to 7 σ above the noise are usually ignored, for example.

The conventional and the minority views of quasar redshifts are each self-contained little universes. Both parties have the feeling that their picture, however incomplete, just "hangs together", and discussion between the two has virtually ceased. One may wonder why quasar redshifts have turned into a controversy like this, while other spectacular phenomena in astronomy, for which the current explanations are controversial even inside the "establishment", have not done so. For example, there are the gamma-ray bursts: short bursts (typically a second) of gamma rays from well-defined directions in the sky (at a rate of about one per day), not (yet) associated with radiation at other wavelengths. Here it is not even known if their sources are as close as nearby stars or as far away as the edge of the universe. The debate is lively, but the possibilities discussed are all inside the bounds of known physics. Another instance is the so-called "missing mass" problem. The universe as a whole, as well as clusters of galaxies, and even galaxies themselves behave as though they contain ten times more mass than can be identified with stars, gas clouds, and the other forms of visible mass. The possibilities discussed include neutrinos, black holes, very small stars and exotic kinds of particles considered in some theories of elementary particles.

The chapter on "The sociology of the controversy" is recommended reading, especially for astronomers. It is not pleasant to be reminded of past mistakes, and sobering to see how strong is our emotional attachment to our view of the universe. But such reminders are necessary, if only to help us maintain a healthy culture of scientific exchange.

The book contains many beautiful pictures of the galaxies involved in the anomalies. It also has a very useful glossary of technical terms, but I suspect that non-astronomers will still be forced to skip over some of the discussion as too technical. The book clearly addresses the author's professional colleagues as well as the general scientifically literate public. The style of the text is very

readable and elegant. It is spiced with Arp's pleasant sense of humor but avoids polemical digressions. Only occasionally does the wording betray bitter feelings about his treatment by the astronomical community. Considering that this treatment has not been very kind, often polemical and sometimes rather personal, the book is impressive testimony that a controversial point of view can be defended without falling into the trap of descending to the level of one's opponents.

H. C. Spruit
Max Planck Institute for Astrophysics
Garching, Germany

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Advanced Aerial Devices Reported During the Korean War, by Richard F. Haines. Los Alto, CA: LDA Press, 1990. 75pp. ISBN 0-9618082-1-7 (available from LDA Press, P.O. Box 880, Los Altos CA 94023-0880).

This is a presentation of data on 42 UFO sightings in or near Korea from 1950 through 1954. Thirty-one are official reports by American pilots taken from military records and ten are ground observations taken from Project Blue Book files. One is an interview, conducted 36 years later, with a GI who fired on a UFO in 1951. The descriptions of the events vary considerably in length and detail. The sighting reports are preceded by a brief description of both sides' combat planes and their capabilities.

Bradley C. Canon
University of Kentucky
Lexington, KY 40506-0027

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Secret Life: Firsthand Accounts of UFO Abductions by David Jacobs, Simon and Schuster, NY, 1992, 336 pp.; ISBN 0-671-74857-2, \$21.00.

In 1975 David Jacobs published *The UFO Controversy in America*, a widely acclaimed classic (for instance, described by Arthur C. Clarke as "one of the few volumes ever published on the subject that is worth reading"). For that reason alone, *Secret Life: Firsthand Accounts of UFO Abductions* would be an important addition to the literature; it is the more so because American ufology is currently being polarized between those who take claims of abduction seriously and those who dismiss them. In view of this polarization, JSE is publishing two reviews, by writers who hold different beliefs on the matter.

It seems fair to say that both reviews acknowledge that the evidence is not compelling; but there is no consensus over what to do about that, where to look for some way of breaking the impasse. Philosophers of science have long recognized that one must hold some sort of hypothesis simply as a spur to investigation, a heuristic, a way of choosing among the various available directions. In anomalistics, we are in such uncharted territory that the common guides to scientific plausibility may not serve. For example, I cannot agree with one reviewer's contention that replication is a tenet of scientific inquiry: certainly, demonstrated replicability may force a scientific community to accept the reality of the phenomenon, but on the way to replication inquiry must proceed by whatever the best available means happen to be or seem. And we need to be chary, too, about experts' assertions that something is impossible, even something like hybridization between humans and aliens; as Arthur Clarke's First Law states, "When a distinguished but elderly scientist states that something is possible, he is almost certainly right. When he states that something is impossible, he is very probably wrong".

*Henry Bauer, Book Review Editor
Virginia Polytechnic Inst. & State University
Blacksburg, VA 24061-0212*

First Review of *Secret Life*

The UFO abduction experience is one of the most puzzling anomalies of the twentieth century. Individuals have memories, often recalled under hypnosis, of being taken from ordinary contexts, such as cars and bedrooms, and being brought on board what appears to be spacecraft. They are subjected to odd physical procedures, which typically center on the reproductive organs. While on these "craft," they also experience unusual mental states, including telepathy. These abduction experiences appear to start early in life and to continue throughout the life span. They cause a very large amount of mental anguish and suffering on the part of those who experience them, and their impact on family life can be devastating. On the other hand, curiously, some persons who claim abduction experiences seem to view them (and their abductors) in a positive light. According to a recent study carried out by the Roper organization, symptoms which point to an abduction history are widespread in the American population, and 2% of the population are estimated as abductees.

The bizarre qualities of the experience, its striking lack of correlation with ordinary human experience, and its similarity to popular-culture images all make one doubt that this could really be going on. Surely this is the result of mental illness? Surely abductees are psychotics or are experiencing some form of hysterical contagion? How could this be happening without huge numbers of UFOs being sighted by the Air force or ordinary citizens?

But studies of abductees have shown that they are not psychotic, that they are not particularly fantasy-prone, that hysterical contagion simply does not

fit the contours of the experience. Psychiatrists who have dealt with abductees are for the most part as puzzled by the events as the patients who report them. We face a phenomenon which is disturbing, widespread, and completely unexplained.

About a year ago one of my close friends, a person I have known for ten years, suddenly told me that she was an abductee. We were sitting around the dinner table, and I almost fell out of my chair. "How many times have we talked about this? Why didn't you tell me?" I wanted to know. She said very simply, "I didn't want to be made into one of your subjects." When she was young, she used to wake up face down in the grass for no obvious reason. She has grown to hate the smell of grass. As an adult she would wake up with bright lights suddenly shining in her second floor window. She is not alone. I have spent a lot of time talking to abductees, listening to their stories, trying to comfort them, and trying to understand their experiences. I have often been impressed with the great courage and fortitude that people have shown in the face of the most disturbing invasions of their lives and families.

Now David Jacobs has chronicled the inner world of abductees in his book *Secret Life*. Jacobs is a professor of history at Temple University in Philadelphia. Interested in reports of abductions, Jacobs learned to hypnotize and used it to elicit repressed memories. In his book he talks about the contours of these repressed experiences, when they begin, the odd medical and mental procedures, the removals and returns, the experiences on board what appear to be spacecraft, the strange scars and the mental suffering that so many (but not all) abductees experience. A surprising discovery by Jacobs is the recurrent experience of abductees of having a grey alien staring into their eyes while they experience a mind scanning procedure, or while they experience intense sexual feelings.

The focus of most alien interventions appears to be the reproductive organs, and collection of sperm and egg samples seems to be an invariant part of adult abductions. Taken at face value, these accounts suggest that aliens are engaged in a gigantic genetic-engineering program. This is very hard to accept. So are the pregnancies which appear to be started and terminated by alien intervention. A good deal of what Jacobs has written about is hard to accept. Nonetheless, this is what his subjects report. So are they relating things which really happened or things which they have imagined? Obviously we can deal much more easily with the latter possibility.

In a later chapter of the book, Jacobs confronts these experiences with psychological theories and shows that most of the theories which attempt to explain abduction experiences as mental aberration or social contagion fail. The abduction experience is hard to explain because it is so multi-faceted. Whole families, including several generations, appear to be involved. Interlocking networks of testimony show that people living with or near abductees possess fragmentary memories of the abductions of their spouses, neighbors, and friends. Some cases include physical traces of the abduction event. Significant

psychological changes follow abduction experiences which show that something has had a big impact on the person's life. Jacobs details these and many related matters.

Jacobs is not the first researcher to write a book about abductions. Two earlier books by Budd Hopkins, *Missing Time* (1981) and *Intruders: The Incredible Visitations at Copley Woods* (1981) helped to open up this remarkable area of research. And there have been many others, largely drawing on Hopkins' and others' research. But Jacobs' book shows the incredible pains that an historian can take to get the details right. He has pushed hard to get at the underlying bedrock of the experiences.

This is a landmark book, but it does have a few flaws. One of them is Jacobs' failure to connect his work to the psychological literature. While Jacobs can be commended for his straightforward approach, there is a lack of psychological sophistication in dealing with experiential issues. While his attention to detail in the experiences is outstanding, one would like to know more about a variety of psychological issues, including individual differences, coping strategies, and perception. The small number of footnotes suggests that a lot more connective research might have been done.

Another flaw is the concentration on the common elements of the experience. Abduction phenomena are messier than the book depicts. A greater range of beings is experienced. There is more connection with psychical and other paranormal phenomena than Jacobs acknowledges. Perhaps discussion of such phenomena will come in a later study.

One criticism sure to arise is the use of hypnosis to collect the data. While I do not view hypnosis as skeptically as some do, I think this is an issue that requires more discussion. Nonetheless, in a minority of abduction cases (about 10%) hypnosis is not necessary to elicit the memories and testimony gained under hypnosis largely agrees with testimony obtained without it.

Finally, one has to admire the courage it took to write a book of this kind. Whatever the abduction phenomenon appears to be, it cannot be ignored. But someone has to be the champion of these odd encounters, and David Jacobs has staked his reputation on the veracity of these accounts. One hopes he has not committed professional suicide. Academia takes its time in rewarding pioneers, and little sympathy is given to them in the mean time. It is now incumbent on other UFO researchers to scrutinize the facts and conclusions at which Jacobs has arrived and to move us closer to a resolution of this intriguing and frightening subject.

Ron Westrum

*Departments of Sociology and Interdisciplinary Technology
Eastern Michigan University, Ypsilanti, MI 48197*

Second Review of *Secret Life*

I eagerly anticipated the publication of *Secret Life*, hoping for a scientifically oriented work which would depart from the anecdotal vein used by pioneer researcher Budd Hopkins, but I was bitterly disappointed.

Jacobs' book assumes the ETH (extraterrestrial hypothesis) to be correct, and supports the ideas proposed by other researchers that our visitors have a very well defined program which includes genetic experiments aimed at creating a hybrid species, in spite of the fact that some scientists in the biological disciplines have expounded on the impossibility of such an endeavor.

Although Jacobs has done a labor of love in gathering the recollections of numerous alleged abduction victims, he does not offer any convincing evidence that we are not dealing with subjective events. As in all books on abductions, the emphasis is on the narratives obtained from the victims under hypnosis. The author recognizes the absence of hard evidence and describes attempts to obtain it, as for instance, using video cameras. The details reported for such attempts strongly suggest that the subjects themselves could have been instrumental in the resulting failures.

The main point in support of the objective reality of abductions is that the narratives provided by witnesses unknown to each other include "exact and minute details previously known only to a few UFO researchers." The obvious interpretation is that the abductees are describing the same objective reality, a series of independent episodes comprising a large-scale genetic program. It is quite possible, but not indisputable, that this could be the case.

However, there is another common element underlying the whole research: the investigator himself. He did not seek the witnesses, rather the witnesses sought him or were referred to him by other abduction enthusiasts, such as his friend Budd Hopkins. I am not insinuating that the investigator influenced his witnesses, but the possibility can't be ignored, as the one-on-one contacts extended over lengthy periods of time. For instance, "Melissa Bucknell" has 30 hypnotic sessions, and since each session lasted between 3 and 5 hours, a conservative estimate of the contact time yields 90 hours, more than sufficient for two persons to know and influence each other, even if unconsciously. To this, we must add the intercourse necessary to set up a TV camera (p. 259), and the numerous telephone contacts.

As we are told, the research involved 39 witnesses claiming to have had two or more abductions, and 22 having had only one, 61 in total. Of those, 9 are primary witnesses, in the sense that the transcripts of their hypnotic sessions, representing 354 or more contact hours with the investigator, are extensively quoted in the book. In addition, we have 10 subjects quoted one or two times, and "Melissa Bucknell", whose transcript appears only once, although the repeated mentioning of her name throughout the text makes her also a star witness.

In short, we don't have a large pool of subjects: the whole research rests on the testimony of a limited number of witnesses, all having prolonged contacts

with the investigator. The possibility of undetected and unintentionally influencing a group of about 10 persons can't be discarded, thus providing an alternative explanation for the similarity of the narratives, which certainly are not identical. This could only be resolved if independent parties could study the original tapes.

A tenet of scientific inquiry is the replication of results. In the field of UFO abductions this is impossible, not only because the original protocols are not available – as deemed necessary for the protection of the witnesses – but also because the secrecy about their identities goes beyond reasonable bounds. As a result, we know only the ages and present occupations of the 39 witnesses having had two or more abductions. As individuals, they remain in a limbo, and any attempt to assess and evaluate their stories fails for lack of information. We know next to nothing about their education, mental and specially physical health, an essential ingredient if we are going to understand why those particular individuals were selected for a breeding program. Neither do we know anything about their daily lives, their families and their adjustment to society, all crucial factors for drawing conclusions about their credibility.

As a result, the characters in this book are ghost figures performing on a darkened stage. They might be very real to the researcher, but he has not managed to convey that sense of reality to the readers. A couple of examples will suffice: Patti, age 23, returns to her bed after perhaps hours of absence (p. 211), and elicits no curiosity from husband Roy who in the interim has moved onto her side of the bed, a rather incredible situation. Or we have Will Parker reporting two abductions many years apart, accompanied on each occasion by his wife at the time. One expects to find the corroborative affidavit of at least his present wife, Nancy, but she is no more than a name. These are disturbing omissions, subtracting luster from the work, as the reader is left wondering why those women did not come forward to confirm the events.

A shortcoming of the book, as in previous works on the subject, is the glaring omission of numbers. We are told how many hypnotic sessions were necessary to extract the information from each individual, but we can only estimate how many contact hours between investigator and subject were required to that end. More importantly, we are told only that the subjects listed (by assumed names) had multiple abductions, but the particulars, i.e., how many abductions each one reported, are omitted. And this includes the star Melissa, who claimed a daily abduction during some period of time (p. 258), a most extraordinary statement deserving more than one line of text! This is a serious flaw from a scientific viewpoint, making it impossible to look for relationships between the number of abductions and such variables as the contact hours with the researcher or the amount of information obtained.

A refreshing novelty is the *Abduction Scenario Matrix* (p. 330) which systematizes the different characteristic events reported by the abductees. Unfortunately, the essential ingredients to validate it are lacking. Presumably, and we are even told so, the described procedures are well defined parameters of the abduction phenomenon, repeating from one incident to the next. But for

each of those characteristics the frequency of its appearance in the sample is missing. For instance, how many times is sexual arousal (in the Mental-Primary box) reported? Or, how many of the witnesses have claimed implants (Physical-Primary box)?

The significance – if not the credibility – of those events would be completely different if the incidence turned out to be 80%, or merely 5%. Those numbers can't be obtained from the text because the narratives of a few individuals (the primary witnesses) strongly predominate. Thus, the repetitive character of the incidents is not firmly established. And, as a matter of fact, the total number of subjects is not even clear, although one could assume there are 61.

Curiously, the abductees say little, if anything, about the presence of an actual craft prior to the main event, and although the narratives are rich in details about the internal arrangements of the UFO, practically no information about its external characteristics and behavior are provided.

This has two immediate consequences:

1. We have no compelling evidence that the UFOs involved in abductions are identifiable with those described in the literature, whose characteristics are well-known.
2. The size of those crafts must be considerable, to accommodate the facilities described by the abductees, which include nurseries, very large rooms containing tables for 50 to 100 babies, and display tubes for 60 to 70 fetuses. Diameters of hundreds of feet are quoted (p. 82).

Such large craft would increase many fold their chances of detection, while in fact the number of credible reports have decreased with the years, and even in the heyday of UFO waves the sighting of very large craft were few and far apart. Moreover, today radar coverage makes it almost impossible for vehicles from outer space to approach undetected, to land, and take off from practically anywhere. How could this be?

This particularity, as well as two other details mentioned by Jacobs, suggest the possibility of a hypothesis that has been around for quite awhile, but has been systematically ignored by the proponents of the ETH and others, namely, the Parallel Universe Hypothesis, which postulates that the origin of UFOs is another three-dimensional universe parallel to ours in a superior dimension. The transfer would require a "window", a threshold, which the operators could place at will anywhere (and hence witnesses are not safe even in Ireland!) and the actual passage would entail a period of disorientation and confusion, which is indeed reported by Jacobs' abductees. The victims will not really pass through closed windows, as reported, but will be transferred through a singularity between those universes located adjacent to the actual window. True, the witnesses describe being taken into the air, but also indicate a vertical motion and no sense of weather (p. 54) while this is occurring, both meaningful de-

tails. I suspect that this significant inference has totally escaped the attention of the author of this study.

If anything, this book has brought into focus the difficulties with abduction claims, and the absolute lack of supporting independent evidence. As such it is a valuable contribution.

*Willy Smith
UNICAT Project
Longwood, FL*

Response to Westrum and Smith

The problem with publishing a book about the abductions is that there are precious few people who understand the problems and difficulties of abduction research and can therefore make informed judgements on a book about the phenomenon. The two reviewers represent examples of the informed and the uninformed. Ron Westrum is a dedicated researcher with impeccable academic qualifications and a depth of knowledge about abductions that few scholars possess. Westrum has written important articles about UFO research and anomalies in general and has helped to define the parameters of abduction research. He has dedicated himself to the rigorous and systematic pursuit of knowledge. Smith is not an abduction researcher and has limited knowledge of the subject at best.

I toyed with the idea of addressing Westrum and ignoring Smith. But, I have decided to deal with Smith's somewhat confused critique because for the lay individual who has no knowledge of the abduction phenomenon, Smith's review might seem as if it had substance. I will not answer all of the criticisms, some are trivial, others are confused, bizarre, and nonsensical.

First, I would like to thank Ron Westrum for his kind and generous words. I would, however, like to make a brief comment about his statement that the abduction phenomenon is "messier" than I appear to believe. The abduction matrix upon which the book is based, constitutes a typical or common abduction scenario. This means that a myriad of atypical experiences have been left out. Westrum is correct in saying that the totality of the abduction phenomenon is messier. It is not a natural phenomenon. It involves intelligent choices with a variety of complications and permutations. We are still in the process of discerning its parameters. Researchers are bound to find many other events that happen to abductees. So far, I have found no reason to expand my matrix significantly. If, however, I were to expand the matrix, I might add some category about "extra-vehicular activity," but that is for the future.

And now for Smith. Smith first takes me to task because I accept the idea of a genetic breeding program even though "some scientists" (unnamed) have said it was an "impossibility." I am afraid that neither Smith nor "some scientists" know what is possible or impossible in the abduction phenomenon. It is

important to stay close to the testimony rather than try to force it into a preconceived structure. One does not adhere to Smith's type of speculation if the evidence mounts for a different position. Nor does one deny the evidence because some unnamed scientist says it can not be possible. If this were true, there would be no Society for Scientific Exploration.

Smith comments on the lack of hard evidence, ignoring the myriad of physical evidence including soil samples, physical sequelae, implants, stains, and so forth that have been recovered in relation to abduction activity. Presumably he wants some sort of artifact from on board a UFO. If he had looked at pp. 259-60 closely, in which I delineate the reasons why there are no artifacts, he would have found the reasons for the lack of hard evidence of this nature. For Smith, the inability to capture video images of an abduction suggests that the abductees are committing a hoax. There is absolutely no evidence for this and Smith's allegation is completely unfounded and based entirely on guesswork. He, of course, presents no evidence for a hoax.

Smith finds negative significance in the fact that abductees come to me and not vice-versa. I follow the ethics of mental health professionals and I do not believe in solicitation. When abductees do seek me out, I explain as much as I can about the possible consequences of revelation so that they can be forewarned and forearmed. I am not interested in soliciting abductees and possibly putting them on a life-changing pathway of discovery without their knowledge of the repercussions. *They* must want to find out what happened to them, my curiosity is not enough.

Smith says, with absolutely no evidence of justification, that I might have "influenced" the abductees to say what I want them to say because of lengthy contacts with them. For example, Smith points out that I had thirty sessions with Melissa yielding ninety hours of contact time plus a few hours of time to set up the TV camera in her apartment. I am puzzled by Smith's equation of time with influence. Spending time interviewing someone is something that is absolutely essential for both therapy and investigation. I suppose that Smith would prefer that I spend little or no time with each abductee so as not to "influence" them. However, following along with Smith's curious argument, Melissa would be the most influenced of all the abductees in my study, and I would have used her throughout the book. In fact Smith inexplicably calls her a "star" witness. Actually, Melissa figures very slightly in the book. I mentioned Melissa several times in the first chapter (as my first abductee investigation) and a few times in one of the later chapters, that is all.

One of Smith's few comments that has some validity is that we need to know more about the abductees' physical and mental health (Smith is obviously unaware of the blind psychological studies of some of Budd Hopkins' abductees, or the studies of fantasy prone personalities conducted by the Center for UFO Studies, or the studies conducted by June Parnell at the University of Wyoming, or those of Ken Ring at the University of Connecticut). For Smith this is "an essential ingredient if we are going to understand why those

particular individuals were selected for a breeding program." This may or may not be true. At present, except for the intergenerational link, the phenomenon seems random. But, what is needed is funding to conduct a complete physical and genetic investigation of each abductee. No one has attempted this yet, and it is certainly something that needs to be done in a very systematic way. Knowing what the abductees do with their time on a daily basis is not, as a confused Smith suspects, going to help in discerning their credibility. However, if Smith had bothered to look at a list of their occupations on pp. 326-327, he would have had a good idea of what they do.

For Smith, it is impossible to replicate my results with the abductees because my "secrecy" about them "goes beyond reasonable bounds." He complains that his attempt to evaluate their stories fails for lack of information about the abductees. Smith peculiarly equates confidentiality with secrecy. Anyone who has a thorough knowledge of abduction hypnosis and of the abduction phenomenon can replicate my results with most abductees. One does not need to have access to the abductees with whom I have worked. In fact, I have been getting reports from researchers around the country who have been finding the same procedures and overall structure of abductions. Replicating results using my abductees is pointless and ridiculous.

Smith seems intent upon interpreting the thoughts and actions of the abductees. For example, Patti returns to bed and her husband (called "a ghost character" by Smith, and who, incidentally, is a physician) exhibits no curiosity. For Smith, this is "a rather incredible situation." He is apparently unaware of the situation in which her husband might have thought that she returned from going to the bathroom. More to the point, if Smith has read p. 55, in which I discuss how those close to an abduction event are rendered unconscious and/or immobile while the abduction of another person takes place, he would have understood what happened.

Smith finds a shortcoming of my book "as in previous works on the subject" because of a lack of statistics. Smith is apparently unaware that my book and Hopkins' 1981 *Missing Time* are the only primary comparative studies ever done. Aside from that, Smith is unaware that one of the problems that abduction researchers have had in understanding the parameters of the abduction phenomenon is that it is extremely difficult to obtain valid numbers of frequency. For example, do all men have sperm taken from them each time they are abducted? The difficulties with solving this simple problem are that men will often skip over this part because of embarrassment or because they are "not supposed to remember." This is most common during the first one or two regressions. Furthermore, men often do not even know what is happening to them when this procedure is performed. Often the memories of the event are purposely clouded in their minds by the aliens. Therefore, discovering the actual frequency of each procedure is extraordinarily difficult and sometimes impossible. Much of the problem has to do with standardization of methodology. Once the methodology is standardized, then the frequency of specific

procedures might be more easily discerned, and statistics might be meaningful for all researchers.

For some reason Smith finds it important that abductees say little or nothing about the presence of actual craft. Obviously, *all* the abductees see a craft as they are about to enter it. They see the inside of the craft as well. This is made quite clear in the text. I did not dwell on the outside shape of UFOs. However, I do talk about the inside of them at length all through the book. After making this curious criticism, Smith displays a distinct lack of knowledge of the properties of the UFO phenomenon that researchers have compiled over the past forty-five years when he says that radar would detect them because some are very large. He is apparently unaware that the size of the object has nothing to do with whether radar will detect them. After this "insight", Smith veers wildly into speculations about a parallel universe that the beings come from. When abductees pass between the two universes, they would, according to Smith, report "confusion and disorientation." He boasts that his insight is "a significant inference," that "has totally escaped" my attention. I am, of course, aware of a lengthy list of speculations about the beings' origins. However, readers of the book will notice that at no time did I attempt to speculate about where the beings came from, be it Mars, a parallel universe, or Cleveland.

David M. Jacobs
History Dept., Temple University
Philadelphia, PA 19122

Society for Scientific Exploration

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Center for Space Science and Astrophysics,
ERL 306
Stanford University
Stanford, CA 94305-4055

Prof. Henry Bauer, *Vice President*
Department of Chemistry
Virginia Polytechnic Inst. & State
University
Blacksburg, VA 24061-0212

Prof. Laurence W. Fredrick, *Secretary*
Department of Astronomy
P. O. Box 3818
University of Virginia
Charlottesville, VA 22903

Prof. Robert Jahn, *Vice President*
PEAR, D334 Engineering Quad
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University of Virginia

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East Albuquerque, NM 87115

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Dallas Diagnostic Association
Santa Fe, NM 87501

Ms. Brenda Dunne
Engineering Anomalies Research
School of Engineering & Applied Science
Princeton University
Princeton, NJ 08544

Dr. Edwin C. May
SAIC
5150 El Camino Real
Suite B-31
Los Altos, CA 94022

Dr. H. E. Puthoff
Institute for Advanced Studies at Austin
4030 Braker Lane West
Suite 300
Austin, TX 78759

Dr. Dean I. Radin
Dept. of Psychology
Univ. of Edinburgh
Edinburgh, EH8 9JZ Scotland

Prof. Ian Stevenson
Dept of Behavioral Medicine
and Psychiatry
University of Virginia
Charlottesville, VA 22908

Dr. Jacques F. Vallee
1550 California St., #6L
San Francisco, CA 94109

Prof. Jessica Utts
Division of Statistics
University of California
Davis, CA 95616

A REVIEW OF NEAR DEATH EXPERIENCES

MICHAEL SCHROTER-KUNHARDT

Psychiatrisches Landeskrankenhaus Weinsberg, 74189 Weinsberg, Germany

Abstract—Near death experiences (NDEs) have been reported throughout time in essentially all cultures. The contents of modern NDEs is independent of gender, age, and profession. The frequency of occurrence is estimated to lie between 10 and 50 percent of all near-death situations. This frequency could be higher still, perhaps even 100 percent, were it not for the dreamlike and dissociative character of the experience and the amnesia-prone participation of the temporal lobe causing a clear tendency to forget the NDE. A number of similar elements are common to NDEs, such as an out-of-body experience (OBE) in which the physical body and its surroundings are observed from various external vantage points, often from above. Numerous cases exist in which the reality of the OBE-observation can be independently verified, by external conditions, situations, people, objects, etc. Even previously non-religious ND experiencers subsequently show a markedly decreased fear of death and a corresponding increase in belief in life after death. Certain elements of NDE-like experiences can be induced by, for example, electrical stimulation of the right temporal lobe or the use of hallucinogenic substances. It is possible that hallucinogenic transmitters (and endorphins) of the brain itself play a role in the NDE. Nevertheless, there are NDE-elements, such as the frequently reported life-review and certainly the acquisition of external, verifiable information concerning the physical surroundings during the experience, that cannot be explained by physiological causes. Wish-fulfillment, death-denial or other defense mechanisms of the brain are also not adequate explanations. The large body of NDE data now accumulated point to genuine evidence for a non-physical reality and paranormal capacities of the human being.

Introduction

To this day mainstream science ignores, rejects or isolates paranormal and religious (mystical) experiences which threaten scientific biases and the "common-sense conception" of the world. This suppression correlates with the denial of death, which also threatens all human efforts, and especially with the scientific devaluation of religions and their conviction of life after death.

The psychiatrist and psychotherapist Stanislav Grof, who once prepared incurable cancer patients for their deaths by evoking near-death like experiences with LSD (3), comments on this official suppression:

In connection with our success- and efficiency-oriented philosophy, aging and dying are not integrated parts of life, but a defeat and a painful reminder of our limits in controlling nature. Dangerously ill or dying people in our culture are considered, and see themselves, as losers.

Contemporary medicine is a slave to technical procedures and overspecialized body-mechanics and has forgotten the holistic aspect of real healing. Its conception of dying is dominated by the effort to overcome and postpone death at any price. Fighting for the mechanical prolongation of life, the quality of the patient's last days and his psychic and spiritual longings do not receive enough attention. We see the tendency to shut out the old and dying people from family and daily life and to pack them off in nursing homes and hospitals, where human contacts are compromised by complicated instruments: oxygen chambers, infusion tubes, monitors of vital functions, etc. (4, p.7-8; my transl.).

Meanwhile, accompanying dying people is a new scientific field of business; but professionals in this area seldom systematically address the point of whether there is life after death. This question seems not to be answerable scientifically, therefore it is left to the dying individual or the priests. But in recent years the (completely unsuspected) records of near-death experiences (NDEs) have shown that there is a scientific, neurobiological basis for the belief in life after death. Especially the growing paranormal capacities of the dying suggest the existence of a time- and space-transcending, and therefore immortal, soul. But what are the results of the NDE- research?

Elements of the NDE

As a consequence of modern resuscitation-techniques NDEs have become more and more frequent; popular publications (11; 12, 13; 14; 15; 16 etc.), lectures and workshops — especially from Elisabeth Kübler-Ross — have attracted the interest of many people. About ten years ago science began to examine this phenomenon systematically. Participating in the "International Association for Near-Death Studies" (IANDS), which was founded in 1977, well-known scientists — especially psychologists, psychiatrists and medical doctors of other specialties, parapsychologists, philosophers and scientists of religion — began their studies (14; 17; 18; 19; 22; 41 etc.). The psychiatrist, Bruce Greyson (University of Connecticut), is president of the American parent-IANDS, which publishes the quarterly "Journal of Near-Death Studies" (JNDS). Worldwide the IANDS has more than 1,000 paying members and a growing number of scientific study groups (20a-i; 21a-m; 22a-x; 151a-i).

Meanwhile roughly one hundred studies have been published, mostly done by psychiatrists, psychologists, cardiologists and pediatricians (11; 12; 14; 20a-i; 21a-m; 22a-x; 23-31; 42; 152; 153; 156; 157; 158, 159, etc.). They showed that up to one third of all people who were nearly dead have had an NDE. A representative poll of the well-known Gallup Institute confirmed these numbers: about 34% of all adult Americans who at sometime in their life were resuscitated have had an NDE (32).

Studies comparing content and frequency of so-called *deathbed-visions* in different cultures have had similar results: From 20 to 67% of conscious dying subjects saw deceased human beings and/or landscapes from beyond and experienced heightened sensations. (35-36). Other facts support these figures: Paranormal NDE-elements, for example, are quite frequent even in normal

consciousness; up to half of the normal Western population is estimated to have experienced them in their lives (22b-g; 37-40). Especially the frequency of spontaneous (i.e., not experienced in conditions of imminent danger) out-of-body-experiences (OBE) is quite similar to the NDE-incidence: approximately 28% of the population of Western societies. OBEs actually resemble NDEs in many aspects and are their most important component (22b-c; 37; 139).

Other facts speak in favor of a still higher incidence of NDEs. They also illustrate some special results of the research work in recent years:

- 1) Fearing disbelief (as having hallucinated) many NDEers keep their experiences secret, even from their family members (13; 15; 17; 24; 41; 153).
- 2) In European hospitals nobody systematically inquires about NDEs; only in Holland have some hospitals participated in a study (13; 42; 152; 153).
- 3) Like dreams, NDEs are quite often dissociated activities of the brain and can only be remembered when the normal consciousness has access to them (22a; 30; 43; 44; 118; 147; 149). Actually in Western societies there exists a tendency to take refuge with unconsciousness when confronted with the fear of death. Just this reaction psychodynamically is nothing else than a dissociation. Since dissociation normally is associated with amnesia, we are amnesiac for most of our dreams and NDEs (22b, 22v, 22w). Therefore some NDEers remember their experience only under hypnosis (22d) or months to years later, when they come into contact with an appropriate association (for example another NDEer) (22d-e; 43).
- 4) Temporolimbic epilepsy sometimes activates NDE-elements and is also accompanied by an amnesia of variant depth (22a; 108; 148). Since the NDE neurophysiologically seems to be connected with the temporolimbic region, an amnesia can be expected. This is confirmed by the fact that we observe a significant increase of psychical (psi or ESP) capacities after the NDE, which are possibly triggered by or located in the temporolimbic area (22a; 45).
- 5) The fact that also those people who were nearly dead without having a NDE experience an increase of psi-capacities signals a forgotten/suppressed (partial) NDE even in their cases (22b).
- 6) Perhaps particularly negative NDEs are suppressed (6; 21n; 55; 155).
- 7) Most NDEs are of short duration; their remembrance should therefore be expected to be poor because research has shown that NDE-remembrance increases statistically significantly with the duration of the experience (220).
- 8) Especially old people, who are predisposed to near-death situations, have a reduced capacity to perceive during the NDE and to remember it (220).

- 9) The accuracy of the memory of NDE-details decreases statistically significantly with the time between the NDE and its remembrance (22e; 22o).
- 10) Certain personalities are embarrassed about the occurrence of an NDE. They fight against imminent death and so restrain their consciousness from experiencing an NDE (22b; 29; 30).
- 11) Many people with life-threatening illnesses are under the influence of anesthetics or psychotropic drugs. These medicaments interfere with perception during an NDE or block it completely (13; 22o; 41). Altogether these facts demonstrate that possibly the majority of those who once (or several times) were nearly dead experience an NDE.

Now, are there any personality traits that predispose some people to experience an NDE? Astonishingly, sociological, demographic or psychological variables do not influence the occurrence of the NDE (13; 17; 18; 22d; 31; 41; 43; 48; 57, etc.). Also, even very young children experience NDEs (32). NDEs and OBEs do not signal any psychopathology; people with mental diseases do not experience them more frequently than normal human beings. On the contrary NDEers and OBEers are possibly mentally healthier as a group (18; 41; 43; 69, etc.).

And when do NDEs happen? NDEs are experienced at the moment of real or anticipated death, i.e., before biological death (17; 21a; 22f). They are triggered by various situations such as accident, life-threatening illnesses, suicide attempts, operations or births (13; 22b; 25; 27; 34; 43; 49, etc.) which do not influence the content of the NDE (41; 43). Quite often the NDEers are clinically dead, i.e., without heartbeat and respiration. Some NDEs occur during an isoelectric EEG (13; 17; 22g; 50), some others even in the morgue (after giving up resuscitation) (15; 51). This is possible because the definite moment of biological death cannot be exactly determined and the latter quite often is insufficiently diagnosed (13; 43; 52-54; 154). Finally up to 37% of NDE-like experiences occur in non-life threatening illnesses/accidents or are not associated with physical illnesses/accidents (21m).

Actually the contention that death means mere unconsciousness is purely theory, because this can neither be experienced nor verified (110). The last thing we know from a dying person is his NDE! Only the brain knows exactly the "point of no return" (of biological death). Pictures are the primary language of the brain, representing all somatic and psychic processes. The brain is able to control these pictures effectively (via biofeedback, imagination or autogenetic training). The NDE as a visual experience provides the most objective information about the imminent moment of biological death!

What does the NDEer experience directly before his irreversible death? In a more or less chronological order and a descending order of frequency the NDE consists of the following elements:

- 1) Increase of mood with feelings of euphoria, happiness, joy, well-being, ease, etc. (25; 29; 49; 51; 56; 57; 324, etc.).

- 2) Out-of-body-experience (OBE) with the dying person looking down on his physical body. His rational consciousness continues working during this state and sometimes undertakes different tests to verify this new existence. Even blind people can see during the OBE — and their perceptions can be verified! Upon leaving the body the OBEer suddenly loses any pain; as an OBEer, he can pass through and see through physical objects and sometimes verifiably read the thoughts of other people (13; 17; 43, etc.).
- 3) Entering a tunnel-like dark transition zone (13; 29; 49, etc.).
- 4) Perception of a golden light which emanates infinite love, evoking enormous happiness in the NDEer. He sometimes merges with this light and then has the mystical feeling of omniscience and all-oneness (11; 17; 22d; 22h; 48; 49; 51; 156; 157; 159, etc.).
- 5) Perception of a heavenly or hellish landscape (11; 22d; 22m; 56; 58; 155, etc.).
- 6) Encounter with deceased relatives, religious figures or beings of light with whom the dying person communicates telepathically; these figures often initiate the dying person's return (22b; 22m; 41; 159, etc.).

During this or the above mentioned earlier stages, certain other NDE-elements may appear:

- 7) Experience of a life-review with known and unknown verifiable details of one's life, whereby the NDEer observes himself acting and feeling again all specifics of a situation, including those of all other participants. This entails an unequivocal ethical evaluation of all thoughts, words and deeds under a normative measure of love (19; 26; 27; 29, etc.).
- 8) Precognition: parts of the OBEer's or the world's future are seen (13; 21b; 22p; 41).
- 9) Different temporal perception: time slows down and simultaneously thought and picture frequency speeds up (17; 49).
- 10) Sometimes, almost from the beginning, some kind of (heavenly) music is heard (11; 29; 49).

Some other rare elements, not covered here are described in NDE literature. The number of details of an NDE correlates with its depth, i.e., with the imminence of actual or expected death (17; 24; 41; 59). At the end of his experience the NDEer has to return into his body. This happens very quickly. Often he is reluctant to return (11; 13; 30; 43; 58, etc.).

With a frequency from about 0 to 25% the *NDEs* are *negative*: The NDEer also experiences an OBE and a dark transition zone, but under unpleasant feelings of fear or panic. He then encounters bad forces or beings and enters a hellish environment (6; 13; 17; 21n; 22g; 22x; 25; 49; 51; 55; 128; 155, etc.).

Surprisingly extensive changes in personality can occur after a NDE. The NDEer can go through such a revolution of values and opinions that after-

wards he is at odds with his environment, where his old values are still operative. Divorces, career changes and a significant psychosocial stress can result. He can experience a phenomenon called "culture-shock," which is well known from people who immigrate into a new culture (13; 22i; 220). Some NDEers actually develop depressions from being forced to return to this "vale of tears" called earth (22i; 220).

What are the new positive values NDEers bring with them?

- 1) In different controlled studies a statistically significant decrease of fear of death (as the supposed end) was found, which was an effect of the NDE and not merely a consequence of having been nearly dead (13; 14; 15; 17; 27; 61; 156; 157; 158, etc.). At the same time a decrease of neurotic (life) anxieties is observed (18; 27).
- 2) After an NDE, all NDEers are absolutely certain that there is life after death (11; 12; 13; 17; 18; 22b; 60; 158, etc.). One observed consequence: some NDEers mourn less because they know that there is life after death(41).
- 3) We found a distinct increase of religiousness, consisting in the above mentioned two points and the real knowledge of the existence of (a) God (and other religious beings) which is often obtained by contacting religious beings (of light). Furthermore, a priority of religious/ethical values in this life and the life beyond emerge from an NDE (13; 17; 22i; 25; 61, etc.).

These are the following values:

- 4) Unconditional love for all human beings and all things (13; 17; 22i; 22f; 51, etc.);
- 5) More harmony, tolerance and sympathy with other humans together with a high evaluation of human relationships (17; 18; 220; 27, etc.);
- 6) Engagement in social-charitable activities (17; 61, etc.);
- 7) Turning away from materialistic, external or superficial values, prestige and competitive struggle lose importance (13; 18; etc.);
- 8) Higher evaluation of the self together with more joy of life and more self-reliance (18; 19; 27, etc.);
- 9) Enhanced perception of the brevity and preciousness of one's own lifetime (13; 27, etc.);
- 10) Higher evaluation of the harmony with nature (13; 18, etc.);
- 11) A feeling of higher responsibility for one's life, resulting especially from the life-review (13; 17; 19, etc.);
- 12) Higher esteem of knowledge of oneself and wisdom (13; 17; 18, etc.);
- 13) Distinct increase of psychical (PSI or ESP) capacities, especially of healing abilities (17; 22b; 41 etc.);
- 14) Higher evaluation of noetic qualities (17; 41, etc.);
- 15) Healing of psychic or psychiatric diseases, especially of addictions (13; 17; 27; 41; 141, etc.);

- 16) NDEs have shown to be the best prevention against suicide; in particular the real knowledge of a life after death and the firmly established religious/ethical values resulting from the life review seem to contribute to this effect (17; 27; 29; 30; 43; 61, etc.);
- 17) Sometimes a complete turn-around of criminals is observed (13; 17; 20a; 21b, 21c; 21d);
- 18) Finally the NDEers develop a feeling of being elected and become a kind of missionary for the knowledge and values learned through their NDEs (17; 19; 27; 30; 43, etc.). Actually all of these changes are consequences of the NDE (49). These changes seem to correlate with the extent of the NDE and degree of imminent death (17; 18; 22b; 49).

Most astonishing is not just the frequency and structural similarity of all NDEs in the United States and Western Europe, independent of sociological, demographic and psychological variables, but that *similar experiences with the same effects have been made for thousands of years across completely different cultures* (17; 34; 58; 68; 123-125; 129). The Gilgamesh epic, the oldest written testimony of mankind, contains a near-death experience:

Gilgamesh... began... his search for the other world. A long time afterwards he discovered behind the oceans at the edge of this world the river Chubur, the last barrier before the kingdom of the dead.

Gilgamesh left the world and crawled through a dark endless tunnel. It was a long, uncomfortable way... but at last he saw light at the end of the dark tube. He came to the exit of the tunnel and saw a splendid garden. The trees carried pearls and jewels and over all a wonderful light emitted its rays. Gilgamesh wanted to rest in the other world. But the sungod sent him back through the tunnel into this life.

There he met Enkidu, who at first had experienced misfortune. Thousands of mag-gots had molested him in another part of the other world. They had buried themselves painfully into his body, until there was left only a shadow without flesh. Finally a friendly god gave him back his body in order to be able to leave the hell and tell his friend Gilgamesh of the horror of hell in full detail. (62, p.8; my transl.).

Also the medieval Christian-Catholic religion recognized the NDEs. The first case records stem from Pope Gregory the Great (5th Century A. D.) (17; 58).

The Chinese and Japanese Amida-Buddhists were more focused on the enlightening NDEs and their artificial induction via meditation; but they also knew hellish NDEs. These Buddhists compiled the second NDE-case collection in the 7th Century A.D. Comments in this collection sometimes reach the level of the near-death research of our days (22j; 22k).

Finally, the NDEs of modern non-Western/Non-Christian cultures are essentially comparable with those of the industrial countries in so far as they are interpreted as other-world-contact and result in an intensified religious life. This applies for example to the inhabitants of Papua New Guinea with some interesting differences: tunnel and light-phenomena (sometimes also the OBE) seem to be absent, heaven and hell correspond to their different religious concepts, and the life-review is replaced by a trial. The NDE content is shaped dif-

ferently, according to the particular religion, culture and mentality (17; 64; 65; 66; 67; 68).

The American anthropologist, D. Shields, found that 95% of 57 non-Western cultures today are familiar with OBEs, the most important component of the NDE (43). Interestingly enough, no review of a former life is described in the NDEs of culture believing in reincarnation. This could be an indication that the concept of reincarnation is a misinterpretation of the contact with the (former) life of another deceased human being (143; 144).

How can these universal, similarly structured and interpreted experiences of so many human beings throughout time and all cultures be explained? At first there is a clear indication, that all conceptions of God originate in the NDE: it is a fact that the most common NDE-element — the light of the other world — becomes a *sungod* for Gilgamesh, a divine Bodhisattva for the Amida-Buddhist, the God of love (and of light) for the Christian and even causes the unbelieving scientist or rationalist to believe in divine forces.

Historical Perspective

With the minds of all NDEers throughout the history of mankind interpreting the NDE unequivocally as proof for life after death, could this be reality? Is this similar interpretation together with the similar structure and the similar effects — i.e., the changes of personality in the direction of an enforcement of the religious/ethical aspects of man — not good evidence for a biological (and genetical) basis for the NDE and thereby all (mystical) religious experiences? I will attempt to answer these very important questions by first falsifying the arguments of the skeptics.

- 1) NDEs are not a sign of a psychic disorder of the NDEer (see above).
- 2) NDEs are not products of a larger capacity for imagination on behalf of the NDEer. Actually neither the NDEers nor the OBEers have a larger capacity for imagination than the general population (22d; 22i; 22k; 72; 75; 76; 77, etc.).
- 3) Before the NDE, NDEers (most likely) do not have a higher hypnotizability or influence susceptibility than the general population. The latter possibility only increases after the NDE (22f; 43; 74, etc.).
- 4) NDEs are not the result of previous knowledge about the NDE. Even children who have never heard about NDEs have similarly structured near-death experiences. Sometimes these children are too young to even speak, and therefore could have never heard of NDEs (13; 16; 22d; 24; 25; 43; 78; 324, etc.). Even if the children have learned from certain (religious) expectations of their parents, their NDEs do not correspond to their parents' beliefs (6; 15; 17; 22c; 22i; 49; 65; 324, etc.). Different studies have actually shown that most NDEers do not have any prior information about NDEs (17; 18; 43; 49; 57, etc.). Even those who have previous knowledge of NDEs do not necessarily experience a correlation between previous beliefs and the actual content and shapes of their NDE (57).

- 5) NDEs are not the result of fulfilling a desire. Desires are always idiosyncratic while NDEs and have many common elements, independent of the belief-system of the NDEer (13; 17; 43; 48; 51, etc.). In many cases the NDEs evidently do not correlate with the desires of the experiencers (13; 16; 221; 22m; 34; 41; 49; 50, etc.). NDEs are also experienced in those cases where the patients are sure to recover again, i.e., against their own desires and expectations (6; 34).

The verifiable OBE-perceptions of resuscitation and other objects and events can be differentiated from previous personal conceptions (15). Quite often the NDEer did not know or know about the deceased ones who appeared in the NDE, so that could not have been a desired experience (22d; 51; 78). Fulfilling a desire is usually a flight from reality but a NDEer having an OBE is confronting himself with a frightful situation (13; 18; 50). Besides, just a flight from reality would never result in such massive positive changes of personality (13; 17; 50). Particularly the culture-shock phenomenon contradicts the supposition of fulfilling a desire, because we usually only desire pleasant things. Finally a psychological explanation can only say something about the mechanism but nothing about the reality of an experience (25; 57; see below).

- 6) The NDE is not merely an archetype of the Jungian collective unconscious. This model is only a controversial undemonstrable theory and therefore explains something unknown by some other unknown thing (13; 43; 47; 79). In dreams for example, we do not find an NDE-archetype. This also could not explain the verifiable OBE-perceptions (13). Interestingly, C. G. Jung himself had an NDE which changed his life and strengthened his belief in life after death (34; 43). Possibly the other world is even a source of all of our symbols (17).
- 7) The NDE is not a birth-recall. Actually the NDE is just the opposite of a birth experience: an easy-floating trip through the tunnel does not resemble the painful passage through the birth-canal. The obstetrician is experienced as a threat and not as a wonderful light. Birth is always painful while the peak experience of an NDE is characterized by painlessness. The birth hypothesis also cannot explain the appearance of deceased people. In general perception during birth is by no means as differentiated as perception during a NDE. Finally, cesarean sections should also entail other NDEs that have not been observed. Altogether, the birth hypothesis reduces all religious/mystical experiences to kinds of birth recall that are surely untenable (13; 18, etc.).
- 8) Psychological theories cannot say anything about the objective reality of the NDE. Many psychologists forget this when they try to reduce the NDE to purely theoretical contentions (25; 50; 47; 79). Concerning the most important theories, the following must be said: Palmer postulates that the imminent death threatens the body and self-concept of a human being. The OBE then reestablishes self-identity via primary process. This theory can neither explain the perspective above nor the verifiable

perceptions during the OBE (73). Especially the fact that the OBEer regards the threatening thing (i.e., the seemingly dead body) itself without fear, is the opposite of what could be expected if the NDE were merely a flight away from an awful reality. When do we want to flee into the direction of danger? Actually NDE-OBEs are rather blocked by fear of one's own death (50). To explain the mystical NDE-elements as regression into the state before ego-differentiation is already phenomenologically wrong, because the whole NDE is experienced with a completely intact ego-identity. Also the concept of depersonalization cannot explain the NDE which by definition is not depersonalization (13; 17; 25; 28; 43, etc.).

- 9) The NDE (OBE) cannot be explained by subliminal perception. This is defined as acoustic perception of emotionally important and especially threatening information during operations or in coma. The concomitant helplessness and feeling of distorted reality of one's own experiences in this state contribute to the appearance of psychophysiological disturbances afterwards. Even pain is suddenly again perceived (15; 53; 80; 81; 82; 83; 84). Contrary to this, the NDE (OBE) is characterized mainly by optical perceptions of important and less important things in a state of very rational (OBE) control and absolute certainty of the reality that one sees. During this experience the NDEer experiences complete painlessness. The psychic aftereffects are primarily positive (see above).
- 10) The statement that NDEs contain perceptions of (this and another) reality cannot be falsified. The reason is that the current definition of a hallucination is grounded on an antiquated reality-concept (of a simple realism) (77; 116). Also, the perception-psychological assumptions of this definition after which reality is imaged in the brain via our sense organs are wrong (22a; 77; 86; 87, etc.). The fact is that we do not even know the neurophysiological correlates of hallucinations (77; see below). We know reality only by its image in our brain; this image however is very selective, dependent on our state of consciousness. This reality is never objective. Therefore in the conventional psychiatric sense all perceptions are illusions. Physical and epistemologically standard NDEs, however, open a reality which exists as surely as nuclear particles or the feeling of love exist (43; 76; 77; 86; 87; 88; 89; 90; 91; 92, etc.).
- 11) The verifiable perceptions of the NDEer's real surroundings, the thoughts of people present, his own past, the imminent death (in a scenic form) and sometimes also of the future are by definition not hallucinations. These perceptions are first of all perceptions of the external and bodily reality which are already in the terminology of conventional psychiatry no hallucinations (70; 77; 79).

The other NDE-elements can also be distinguished from usual hallucinations and also therefore from dreams which by definition are hallucinations in the sleeping state (76-77; 86):

- a) While they are always individually unique, NDEs show a surprising similarity (6; 15; 77; 87, etc.);
- b) Hallucinations of mentally ill people can clearly be distinguished from the contents of an NDE (24; 34; 50; 65; 93); 80% of hallucinations have a negative content while about 90% of the NDEs are positive (34; 94);
- c) Usually it requires long-lasting hallucinations to have a psychopathological effect (77; 132) while just one short NDE may entail great changes of personality in the direction of mental health;
- d) Scientists who have experienced hallucinations differentiate clearly between NDEs, dreams and hallucinations (22a; 49; 43; 51, et ~.);
- e) The OBE is no heautoskopik hallucination (13; 43; 50, etc.), sometimes it can be verified experimentally (43; 50; 70);
- f) The fact that at the moment (or hour) of death dying people sometimes can be seen far away from their deathbed by living, mentally healthy human beings, indicates a kind of real appearance (35) which can be compared to the appearance of deceased ones in the NDE. This also correlates with the imminent death of the NDEer. If the NDEer sees living persons in this phase of his NDE, their appearance correlates quite often with the imminent death of the seemingly living person (13; 16; 78). The fact that the unknown deceased one looks the way he looked when he was living also indicates a paranormal process and possibly an appearance of a ghost (13; 35, etc.);
- g) The existence of an elevated hallucination index does not increase the frequency of NDEs (34; 50; 65).

The intercultural, inter- and intra-individual differences between NDEs whose content, interpretation and effects clearly correlate with culture, religion and (therefore) mentality of the experiencer do not speak immediately for their hallucinative character. There could simply be other different worlds. A correlation does not say something about the cause (17; 22d; 22j; 22k; 58; 64). Scientifically this problem cannot be solved as easily as some people want (see point 10):

It seems reasonable to assume that in ancient times those who suffered a near-fatal injury or became seriously ill and appeared dead, but later revived bearing spectacular accounts, would have been regarded uncritically as revealing something of the hidden mysteries of death. This raises the intriguing possibility that some and perhaps much of the folklore imagery of the after-life could be derived by NDEs, and that cultural expectations not only determine NDE imagery but are themselves also derived from it (57, p. 612; my italics).

In the light of results of parapsychological research on the mechanisms of extrasensory perception (ESP) (95; 96; 97) an intermediate position might be correct: the other world's pictures of NDEs consist of a mixture of individual hallucinations and true ESP, the latter representing — perhaps still on the level

of human images — the different "mansions in the house of God" that Jesus mentioned (John 14, 2).

In any case the astonishing uniformity of the near-death experiences of mankind refers to the importance of religious values and other worldly conceptions of humans. This uniformity and many other facts even speak for a biological (and genetic) base of (this) religious experience and therefore religiosity in general. What do we then know about the neurophysiology of the NDE — and can this knowledge help our understanding of the meaning of this universal experience?

In principle the significance of neurophysiological findings is limited. They are only correlates of the NDE which cannot say anything about its objective reality nor its meaning for the NDEer. Actually it is not clear if the neurophysiological correlates, for instance of schizophrenia, are cause or consequence of this disease. Then each physiological correlate of the color red, for example, is only secondary; we would not understand it without knowing first what red is. Finally we cannot reduce one perception (of the color red or the appearance of a deceased person) to another (neurophysiological) perception because both are only perceptions. So physically and neurophysiologically neither the color red nor the deceased person exist; colors, forms, smells, joy, love and pain are unknown in these disciplines, like the NDE they (seem to) exist only in our brain. If we would know all neurophysiological processes of the perception of the rising sun, nowhere would a light emerge. We would only remain in the description of material particles and fields of energy (22a; 22n; 43; 45; 50; 77; 85; 98).

Neurophysiology of the NDE

In so far as neither the physiological base of hallucinations nor the complex states of consciousness like schizophrenia, depression and fear (not to mention love or dreams) are really known after years of research (22a; 77), our knowledge of the neurophysiology of NDEs is very small, particularly because NDEs have only been discussed and investigated for a short time.

What we know is the following: *certainly the NDE is based on a functioning brain*, working in an Altered State of Consciousness (ASC) during the NDE. Especially the psychical (PSI or ESP) capacities are increased in this state of mind. Indeed an isoelectric EEG does not exclude discharges of deeper brain structures (13; 99). Actually many NDEs are experienced by persons only experiencing imminent death, i.e., not clinical death, and therefore without damage to brain functions (21a; 21m). Furthermore, some elements — especially the PSI components — of the NDE are more or less common for human beings who are not at all near death; some can be caused by hallucinogens (LSD, ketamine) or electrical stimulation of the brain (3; 17; 55; 87; 100; 101; 102; 140).

Neither hypoxia nor hypercapnia are necessary to cause an NDE. However both are often present (in a combination) and the latter can cause some NDE-

elements artificially. Actually NDEs can be found with normal, increased or decreased $p\text{CO}_2$ and $p\text{O}_2$ (3; 15; 17; 18; 21a; 21m; 22a; 22f; 25; 43; 57, etc.).

We do not know NDE-specific transmitter-constellations. There are only two important assumptions. First: *Endorphins/enkephalins* could play a role; but the evidence is contradictory. These substances on the one hand do not have an hallucinogenic effect (13; 15; 22a; 22n; 59; 91; 103; 118; 146). On the other hand they participate in many important experiences of man so that their activation during the NDE is not exceptional (22a; 22n; 87; 91; 103). Assumptions concerning the participation of *serotonin* are safer. LSD and ketamine (possibly also hypercapnia), for example, inhibit neurons in the midbrain which contain serotonin; this inhibition again activates the temporolimbic system whose epileptogen discharges could be the common final pathway of all neurophysiological mechanisms (22a; 22f; 22n; 24; 101; 102; 104).

Actually the temporolimbic region contains numerous endorphin/enkephalin receptors (22a; 22n). Furthermore, the long-term memory may be located here (105). Then the electrical stimulation of the temporal lobe could evoke fragments of a life review, *déjà-vu* phenomena and also an OBE (17; 22a; 22f; 43; 87; 100; 106; 148). In a small case study, OBEs, *déjà-vu* phenomena and other psychical (PSI or ESP) elements of NDEs were associated with possible temporal lobe symptoms (PTLS). Quite often, patients with these symptoms hear sounds which sometimes resemble the initial sounds of NDEs. Even in a religious conversion, feelings of blessedness and all-oneness are described in connection with temporal lobe symptomatology (15; 22a; 26; 43; 45; 85; 98; 100; 106; 107; 108). NDEers *in* and *after* the NDE show a significant increase of psychical (PSI or ESP) capacities which are possibly located in the temporolimbic system (see above) (22a; 22b). Finally antiepileptic medication like sedatives and hypnotics often interfere with the genesis of NDEs that can be explained by their influence on the limbic system (15; 22c; 220; 57; 104; 109).

However, this interference does not always take place. NDEs have occurred under the influence of these medications (22h; 29; 43, etc.). We should not forget that all meaningful human behavior is connected with the temporolimbic region. Its activation during the NDE is nothing special (22a). Finally, temporal lobe epilepsies usually do not have any similarity with NDEs; their symptoms, on the contrary, are emotionally negative, idiosyncratic, and uncontrollably automatic. Furthermore, the NDE does not show all the sensorial, motorial, autonomic and (gustatory, olfactorial, haptic and thermic) hallucinative symptoms of the temporal lobe epilepsy. The visual hallucinations of this disease are visual disturbances and not intact images as in NDE perceptions. An isoelectric EEG is inconceivable with a temporal lobe epilepsy but has been observed during some NDEs (13; 15; 17; 18; 22a; 50; 100; 108; 110, etc.). *So NDEs are not temporal lobe epilepsies; some indices however point at a special participation of the temporolimbic system.*

By no means can the NDE be psychopathologized on the grounds of the above mentioned neurophysiological assumptions. While some critics main-

tain that the NDE is a dysfunction of the brain, this would entail a dysfunctional, confused/incoherent and individually different experience which has destabilizing, disintegrating and psychopathologizing effects for the experiencer. But the NDE is just the opposite: it constitutes a completely unexpected peak capacity of the human brain and has psychohygienic/psychotherapeutic effects which exceed those of many psychotherapies.

In the light of their complexity, their relative uniform structure and their enormous efficiency it can be postulated that the NDE neurophysiologically consists of a controlled, selective activation of certain (though mainly unknown) biologically founded hierarchical neuronal structures (22a). However the fact is that quite different causes (i.e., acute bronchial asthma, postpartal embolism, intracerebral bleeding, operation, birth, coma or pure psychological expectation of imminent death) with different neurophysiological correlates always produce very similar NDEs. This indicates the participation of a determined brain-structure otherwise we would expect an individually different unstructured organic psychosyndrome.

Furthermore, the fact that hallucinogens can produce NDE-like experiences under certain psychic and therefore neurophysiological conditions (setting) (3; 21c; 102; 140; 142) confirms the existence of a specific neuro-biological involvement in the NDE. The psychiatrist Stanislav Grof experimented with LSD-induced NDE-like experiences on incurable cancer patients. The patients, like the normal NDEer, lost their fear of death, became more positive in their outlook, came out of depression, and experienced a release of pain (3). That is the reason why in many cultures hallucinogens taken during the ritual of initiation are also used to assist entrance into the world of the gods, religious experiences of death and the other world (13; 17, etc.).

Just as synthetic morphine-antagonists proved the existence of endogenous opiates and their neurons and receptors, the effects of LSD and other hallucinogens are a clear indication for the existence of endogenous hallucinogens. Scientists have just discovered, isolated, coded and cloned the gene which produces the receptors for hashish in the human brain (198) — a strong indication for the existence of endogenous hashish, which has been discovered just some weeks ago (162). Also derivatives of tryptophan, the precursors of serotonin, are potent psychedelics (178; 150). The release of these endogenous hallucinogens during an NDE then has to happen within a complex hierarchical, neuronal structure which produces at the moment of death the specific and complex pattern of the NDE.

The fact is that the mystical quality and the effects of the NDE can be compared with the religious-mystical experiences of all cultures of mankind and therefore constitutes the continuously reproduced base of the other world-conceptions of all religions (3; 57; 68; 102; 106; 111; 112; 113; 114; 115; 116; 117; 118; 119; 120; 121; 122; 123; 124; 125; 126; 133; 134; 135, 160; 161, etc.) and the neuro-biologically-based core of all religious experiences on the whole (17; 43; 57; 87).

Moreover, the implicit statement of nearly all religious experiences to represent the reality of another world cannot be falsified by neurophysiological correlates. Even if someone would label all of these religious experiences as a psychopathological, antiquated concept of reality and a definition of hallucinations based on this concept, the universal occurrence and psychohygienic effects of these experiences with whole systems of meaning (i.e. religions) founded upon them would always demonstrate the opposite (17). Dr. V. M. Neppe, Director of the Division of Neuropsychiatry at the University of Washington School of Medicine, sums it up as follows:

... these results... may imply that there is an organic base which allows the experiencing of an endogenous or exogenous reality which others, by virtue of their more conventional pattern of functioning, may not be able to experience... the same common pattern of functioning that predisposed the percipients to gustatory or haptic hallucinations deriving from within the brain may allow the experiencing of a different kind of reality deriving exogenously (i.e., outside the brain) and manifesting as SPE (subjective paranormal experience, my suppl.) (45, p.11 - 12).

On the other hand in the "British Medical Journal" the psychiatrist L. Appleby concedes:

Explanations have included the spiritual, the psychoanalytical, and the purely neurological, all sharing only one attribute: each requires a form of faith. And, though the features of the near death experience are reproduced in drug-induced states, this points to a physiological substrate rather than to their etiology (98, p. 976).

Concluding Remarks

As a human experience (127) the NDEs really demonstrate the maximum capacity of the brain. In most cases we observe an exceptional increase of psychical (PSI or ESP) capacities (50; 55; 62; 130; 131; 136; 137; 138). This sudden and completely unexpected increase of extrasensory and extracorporeal perception just directly before biological death indicates clearly that the brain prepares the dying human being for another life, a life beyond the body and consequently one with extrasensory perception. That is the neuro-biologically founded interpretation of the NDE, confirmed by the brain of all NDEers.

Therefore, all other (reductionistic) interpretations, products of the normal waking consciousness as only one state of mind, might be more or less rational illusions which have to be left behind when death is coming. At this point an altered state of consciousness and understanding begins.

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Analysis and Discussion of the May 18, 1992 UFO Sighting in Gulf Breeze, Florida

BRUCE MACCABEE

Fund for UFO Research, Mt. Rainier, MD 20712

Abstract—A professional TV crew traveled to Gulf Breeze, Florida on two occasions in the spring of 1992 at a time when sightings were occurring on a weekly basis. On each occasion anomalous lights were observed and videotaped passing through the Gulf Breeze skies. This paper reports briefly on the first sighting and concentrates on the analysis of the second sighting when the TV crew used a special "high power" camera. During the second sighting the lights were observed from two locations allowing for triangulation and a subsequent estimate of the spacing between them (about 10 ft). They were moving in an early rectilinear path at a speed exceeding 20 mph before they faded out. A discussion of the hoax hypothesis involving pyrotechnic devices and incandescent light sources is presented. It is shown that the sightings, if not of "real" UFOs, constitute a hoax of considerable ingenuity, expense and persistence. This sighting was just one of about 170 which have occurred in the Gulf Breeze area during 1990-1992.

Introduction

Starting in November, 1990, residents of Gulf Breeze, Florida began seeing unusual lights in the sky, sometimes hovering but often moving with, across or against the wind. Although they were usually red when first seen, sometimes they would initially appear white and then turn red. Very often they would turn from red to white and flash rapidly before disappearing. Sometimes after the white flash they would return to red before disappearing, as happened during the May 18, 1992 sighting. (During one sighting the light changed from red to white to red to white and back to red before disappearing.) Sometimes they would drop glowing material or lights. These lights have been given the generic name "Red/White Lights" or RWLs. In a number of sightings revolving pairs of lights and even rings of light (ROLs) have been seen and videotaped while moving through the sky. On Sept. 6, 1991 a ROL was observed (and videotaped) as it slowly flipped over and blocked background stars while moving slowly through the sky. In another case (Nov. 5, 1991) a complex array of lights consisting of an elliptical bottom, a nearly horizontal arc of five lights above that and a single light above the arc, was photographed. In a couple of sightings the RWLs have turned into ROLs or vice versa. Many of these previous sightings have been reported in detail elsewhere (1; 2; 3; 4; 5; 6). (This author witnessed the appearance, slight movement through the sky and subse-

quent disappearance, after 70 seconds, of a ring of eight white lights on Sept. 16, 1991, in the company of about 30 other people.) (4)

There had been well over 100 sightings when, on the night of March 14, 1992, several groups of witnesses at various locations in Gulf Breeze saw at least six RWLs one night, with three at one time in sufficiently close proximity to give the impression of being geometrically arrayed. During this sighting witnesses flashed lights at the RWLs and in at least one case there was a rather convincing response in the form of a similar set of flashes by one of the RWLs. (7) (This was documented on videotape.) Ten days later Jim Moore, a reporter from KHOU TV (Houston, TX), and a film crew arrived in Gulf Breeze to do a story on the Gulf Breeze sightings. Jim joined the Gulf Breeze Research Team (GBRT) and other observers at Shoreline Park. One RWL appeared and then a second light immediately adjacent appeared. These lights were seen revolving around one another, without swinging back and forth, for about 2.5 minutes. The revolution rate was 75 sec/cycle (0.013 Hz), as determined by the camera used by the TV crew and confirmed by Bruce Morrison's videotape made at the same time (Morrison is a MUFON investigator who has video documentation of over 150 sightings). The pair of lights disappeared and about 30 seconds later a second (or the same) RWL appeared, a single light this time. A million candlepower spotlight was flashed on and off at the light. It turned white and its own pulsation rate suddenly changed from slow to fast, seemingly in response to the flashing of the spotlight, before it, too, disappeared. The spacing between the pair of revolving lights, as recorded on Bruce Morrison's videotape, was about 5 mm on a 14" TV monitor used for the video analyses discussed in this paper. The actual spacing between the lights could have been calculated had there been a triangulation that night. Unfortunately there was no triangulation (all the witnesses were at one observation site) so it was not possible to calculate the distance to the lights.

However, a "repeat performance" on April 3, was triangulated with reasonable accuracy. Bruce Morrison again videotaped the sighting and again the maximum spacing between the images was about 5 mm. The two lights were revolving, without swinging back and forth, at a rate of about 90 sec/cycle (0.011 Hz) and were estimated by triangulation to have been 3 to 4 miles (about 16,000 to 21,000 ft) from the camera. As explained in Appendix A, the image spacing corresponded to an actual separation of 10 to 12 ft. Hence if these two lights were attached to a single, revolving object it had a substantial size.

Having "seen the light" on March 24, Jim Moore's interest was heightened considerably and his TV station arranged to have him return to Gulf Breeze during the week of May 18 when the "Intruders" TV mini-series (about a psychiatrist who discovered some of his patients were abductees) was shown nationally on CBS. This time he had a special "high power" black and white (B&W) video camera as well as the standard color camera. His film crew set up on the pier at Shoreline Park on the night of May 18 and waited along with some members of the GBRT. The remainder of this paper is a discussion of the

May 18 sighting. Numerous technical analyses have been done which support the conclusions stated in the main text. For the convenience of non-technical readers the analyses are not presented in the text but are in a series of Appendices at the end of the paper.

The May 18,1992 Sighting

While the KHOU TV crew was waiting at Shoreline Park, other members of the Gulf Breeze Research Team, including chemist Ray Pollock, were at the Bay Bridge site somewhat over a mile north (see map). The "Bay Bridge Watchers" were the first to see the light appear at 10:28 p.m. The red light/object was first seen at 75° azimuth and somewhat less than 26" elevation (the exact value was not recorded). According to Pollock's measurements, over the next 4 minutes it moved southwestward to 97° azimuth and 26" elevation where it disappeared (see map). Soon after the light appeared a witness using binoculars saw some whitish, filmy material fall from the red light. Later it seemed to turn white and divide and then return to red so that there were two lights side-by-side. Subsequently one light disappeared and finally the second light disappeared. Several photos taken by Mr. Pollock, using a 35 mm camera with a 500 mm telephoto lens and ISO 1600 film, near the end of the sighting produced pictures showing two, side-by-side tiny, very slightly smeared red images. The spacing varied from picture to picture, but the maximum spacing was 0.4 mm. This spacing corresponds to an actual spacing as projected onto the line of sight from the camera to the objects (i.e., perpendicular to the line of sight) 0.8 ft for each 1,000 ft of distance (see Appendix B). This information is used later in the calculation of the spacing between the lights.

Meanwhile, the KHOU TV crew, waiting at Shoreline Park, did not immediately see the RWL. However, about 45 seconds after Pollack saw it, they saw it and began filming with the special B&W camera (8). They continued filming with this camera until the end of the sighting, about 3 min., 16 sec. later. About 1 3/4 minutes after starting the B&W video, or about 2.5 minutes after Pollack first saw it, one member of the TV crew started filming with a high quality color TV camera with a telephoto zoom lens. Thus, during the last 1.5 minutes of the 4 minute sighting both cameras were running simultaneously. (Although the cameras weren't electronically synchronized, voices were picked up simultaneously by both cameras and this made it possible to synchronize them.) The color camera showed that during the last 1.5 minutes the lights were red.

The TV crew did not keep track of the azimuth and elevation during the sighting. However, for 15 seconds at the beginning of the color camera video segment (2.5 minutes into the sighting) and before he zoomed in for a close-up view, the cameraman showed a wide field of view picture which recorded nearby street lights at Shoreline Park. The azimuths of the street lights were subsequently measured from the location of the video camera. Using these azimuths and the video imagery I have determined that the azimuth of the RWL was about 51° and the elevation was about 15". This 15 second segment showed

the object traveling to the left 16.5 mr and upward 12.2 mr (9). Unfortunately no one at Shoreline Park measured the ending azimuth immediately after the sighting. However, on the day afterward MUFON investigator Arthur Hufford interviewed several of the Shoreline Park witnesses and they agreed that the lights moved from right to the left from their point of view and that the final azimuth was about 42° . This information is used below to help estimate the travel path of the lights.

The Video Imagery

The B&W videotape of the sighting begins with a single, large, bright, unfocused, nearly round image with some dim, filmy material falling downward, confirming the testimony of a Bay Bridge observer that it dropped some glowing material. (The video also showed very faint glowing material falling downward a couple of more times during the sighting, but, unlike a typical flare or pyrotechnic device, material was not falling continuously.) During the next 30 seconds, as the crew focused the camera, the image shrunk to one or two and occasionally as many as three bright "blobs" of light (roundish, overexposed images) that merged together at their edges (they don't completely overlap; if they did the image would appear as one "blob"). The fact that the image consisted of multiple "blobs" rather than a single featureless "blob" indicates that there were at least three sources of light so close together that the camera could not completely resolve, i.e., separate, them. The video did not record any smoke or vapor at any time during the sighting.

About one minute into the B&W video a second bright light appeared at the left of the original one. At this time the lights turned white, according to Jim Moore, and then a few seconds later returned to red. (The color change did not show up on the B&W camera, of course.) There was no rapid leftward motion of the image of the second light, as there would have been had it separated or "fissioned" from the first and then moved to the left. The second light just appeared in one frame as if it were turned on by a switch. It immediately dropped some faint, glowing material and then faded in and out several times over the next second, brightened and then the image began a steady clockwise revolution about the image of the first light. In the first 5 1/2 seconds after its appearance the image of the second light moved to the right and merged with the top part of the image of the first light. Then it dimmed and moved farther to the right and down in a rapid continuous circular motion until, after another 5 1/2 sec, it was at the right side of the first light. It continued the revolution, but more slowly, so that about 17 seconds later it was below and to the right at about the 5 o'clock position relative to the first light. By this time it was also very dim. Then, over the next minute it revolved counterclockwise (upward) slowly so that at about 2.5 minutes into the B&W video (3 1/4 minutes into the sighting) it was level with the first light. At this point the spacing was 22 mm on the 14" TV screen. (This is also the time when the color camera showed two red lights side by side. The images were spaced 3.5 mm apart, indicating that

the focal length of the B&W camera was about 6.3 times greater than that of the color camera.) It continued to move upward slightly to a location at about the 2:30 o'clock position. Several seconds later the original light, now on the left, ejected a single dim light that fell straight downward. As explained in Appendix F, I have estimated that the ejection velocity was about 2.85 m/sec. (This is discussed in more detail below.) Over the next 7 seconds the original light dimmed and brightened randomly and then faded out. About 20 seconds after that the second light (formerly on the right of the original light) ejected a dim light straight down, with an ejection velocity of about 4 m/sec and then it began to dim and brighten randomly. Finally it, too, faded and disappeared, about 3 minutes and 16 seconds after the video began.

The objects which were ejected downward were too dim to be recorded by the color video camera. That camera did, however, show the left hand (original) light fading and going out and then the right hand one fading and going out.

Analysis of the Video and Photographic Data

Reconstruction of the Track and Speed of the Lights by Triangulation

There is not enough direct evidence to allow a complete determination of the track (path of travel) through the sky. However by combining some educated guesswork with the measured azimuth values it is possible to construct a straight track that is reasonably consistent with the available data. This track has been used to (a) estimate the overall average speed, (b) to estimate speeds at several points along the track and (c) to estimate the distance to the lights near the end of the sighting in order to calculate the actual spacing between them.

Azimuths were measured from the Bay Bridge site only at the beginning and end of the sighting (75° and 97° respectively) and, from the Shoreline Park site, at 2.5 minutes into the sighting (51°) and at the end of the sighting (42°; see map). The 75, 97 and 51 degree azimuths are probably accurate to a degree or better. The 42° azimuth is based on the witness' recollections and could be off by several degrees. However, for the purpose of estimating a track I assume that these azimuth values are accurate. (The overall conclusion is not very sensitive to slight variations in these azimuth values.) The 97 and 42 degree azimuths established the end of the track. The initial azimuth from the Shoreline Park location was not measured so it must be estimated from the video itself. Fortunately this is possible to do since, superimposed on the up, down and sideways jitter of the image caused by camera vibration, there is a continual slow, leftward motion (in agreement with the witness' recollection that the light moved generally southwestward). (Note: Although the images jittered about, there was no regular oscillation, as would be expected if the lights were hanging suspended below some supporting object.) The leftward image motion as a function of time has been used to measure the azimuth angle change rate (AACR). The change in the AACR with time during the video has been used to

estimate the initial azimuth by the "trial and error" method described in Appendix C. It was difficult to accurately measure the AACR because each time the image moved somewhat to the left, the cameraman turned the camera to (approximately) recenter the image on the screen. The cameraman never let the image drift for more than 3 seconds and usually for less than two seconds, except at the beginning of the color video when the camera was stationary for 15 seconds. Nevertheless, by measuring the angle change rate for short durations (1-2 seconds) near the beginning, middle and end of the B&W video I have found that the AACR increased from about 2.5 to about 6.5 degrees per minute. (10) The beginning of the color video showed that when the lights were along the 51° azimuth the AACR was about 3.8°/min.

Appendix C shows how some simple assumptions about the motion of the lights (straight path, constant speed) make it possible to estimate the track of the lights as projected onto the earth's surface. As a check of the track obtained in this way I measured (on the map) the change in angle that corresponds to motion along the track during one minute of time centered where the 51° azimuth crosses the track. I found about 3.3°/min which is reasonably close to the rate measured directly from the color video, about 3.8°/min, assuming that the track is at least approximately accurate. As a further check I found that the 51° azimuth crossed the track approximately at the 2.4 minute point (between the numbers 2 and 3 on the map), and this compares favorably with the estimate that the color video camera recorded the lights as being along the 51° azimuth at about 2.5 minutes into the sighting. Similarly, the B&W video, at a time estimated to be 95 seconds into the sighting, showed an AACR of about 3.2°/min, whereas a direct measurement on the map of the AACR during one minute of time centered on the 90 minute location (between points 1 and 2) gave about 2.5°/min.

I do not claim that this is the exact projected track of the lights. The fact that the AACRs at the 51° azimuth and at the 1.5 minute location do not agree exactly with the AACRs measured directly on the video shows that there is some inconsistency, suggesting a possibly curved path and/or changes in speed. However, the path is useful for distance and speed estimation purposes. The azimuth of the path is 245° and its length was about 2.7 miles.

The altitude of the lights could also be estimated. According to Ray Pollock, at the end of the sighting the elevation was about 26°. The map showed that at the end of the sighting the horizontal distance from the Bay Bridge site to the lights was about 1.32 miles. An angular elevation of 26° then gave an actual height of about $(1.32 \text{ mi} \times \tan 26^\circ = 0.64 \text{ mi} \Rightarrow) 3,400 \text{ ft}$ and a radial (slant) distance from the observers of $(1.32/\cos 26^\circ \Rightarrow) 1.47 \text{ mi} = 7,750 \text{ ft}$. The angular elevation along the 51° azimuth from Shoreline Park was determined from the color video image to be about 15°. Using the track on the map, the distance was about 2.67 miles, so the elevation at that point on the track was about $(2.67 \times \tan 15^\circ \Rightarrow) 0.715 \text{ mi} = 3780 \text{ ft}$, about 380 ft higher than at the end of the track. Hence the lights were traveling very slightly downward and, since the distance from the end of the track to the intersection of the 51° azimuth is

about 1.09 mi = 5770 ft, the angle downward was about equal to $\{\arctan([3780-3400]/5770) =\}$ 3.80. If this downward slope were constant from the beginning of the track then the lights began at an altitude of about $\{[3,400 + (380/5770)(2.7 \times 5,280)] =\}$ 4,340 ft and dropped about 940 ft while traveling 2.67 miles horizontally. If the lights actually followed this track they traveled about 2.7 miles in 4 minutes which is equivalent to an average speed of about 40 mph in the southwestward direction (against the prevailing gentle wind).

This average speed can be compared with the speed estimated directly from the color video during the first 15 seconds assuming that the track on the map is at least approximately correct. During that 15 seconds the azimuth changed by about 0.95" and the elevation angle changed by about 0.7". By trigonometric or graphical analysis one can show that, if the lights were actually traveling along the path shown on the map, then these azimuth and elevation angle changes mean that the lights were at an altitude of about 3,780 ft and were traveling on a slant path downward at about 5.5° and at a speed of about 47 mph. A similar analysis using B&W video data taken about 1.5 minutes into the sighting, using the track on the map showed that the lights were about 4,340 ft high and traveling downward at an angle of 5.4" and at a speed of 56 mph. An analysis to be presented below suggests that at the end of the path the lights were traveling at about 20 mph. Hence it may be that the lights were actually decelerating (in which case the path shown on the map would not be correct).

The Spacing of the Lights

At about 3 1/4 minutes into the sighting both cameras were showing two lights side by side at apparently the same altitude and of apparently the same intensity. The color camera image spacing was 3.5 mm +/- 0.1 mm. Video imagery of the moon has provided the angular spacing calibration for the color camera, 0.2 mrlmm (see Appendix D). By multiplying the measured image spacing by the angular spacing calibration value one finds that the angular spacing of the light images was $(3.5 \text{ mm} \times 0.2 \text{ mrlmm}) = 0.70 \text{ mr}$.

The approximate location of the lights 3 1/4 minutes into the sighting, assuming that the lights traveled along the track shown on the map, was found just to the left of the 3 minute mark (see the triangle on the track). This position was about 0.5 mi = 2,700 ft before the end of the track. The location was about 2.14 mi = 11,300 ft, measured horizontally, from Shoreline Park and 1.79 mi = 9,450 ft, measured horizontally, from the Bay Bridge site. The elevation at this location was slightly higher than at the end of the path: $3,400 \text{ ft} + 2,700 \tan 4.2 = 3,600 \text{ ft}$. Therefore, the slant ranges to the lights (the actual distances from each of the cameras to the lights) were 2.245 mi = 11,860 ft from Shoreline Park and 1.91 mi = 10,110 ft from the Bay Bridge. Multiplying the slant distance from the Bay Bridge by the angular spacing of the images, 0.8 mr (see Appendix B), the spacing of the lights, as projected onto the line of

sight, was about $(10,110 \text{ ft} \times 0.8 \text{ mr})/1,000 = 8.1 \text{ ft}$. At the same time the KHOU TV camera indicated a projected spacing of $(11,860 \times 0.70 \text{ mr}/1,000) = 8.3 \text{ ft}$. These numbers are surprisingly close considering the considerable differences in equipment used and the difficulties in estimating the actual path of the lights. Furthermore, one must take into account the fact that the two groups of people were viewing the lights from directions that were about 41° apart in azimuth so they were having different perspective views. Appendix E shows that the spacing was actually about 8.8 ft. Of course the calculated length depends upon the assumptions described above and upon the accuracy of the measured azimuths. Other assumptions would lead to slightly different paths for the lights and different locations 47 seconds before the end of the sighting. However, these other locations probably would not be far from the location specified here (the triangle on the map), so the calculated spacing would not be greatly different. Considering the possible variations it seems quite certain that the spacing was between 7 and 11 feet. This spacing is comparable to that calculated for the spacing of the two lights seen on April 3, 10 - 12 ft (see Appendix A).

Since the angle A (see Appendix E) was about 18.5° the imaginary line between the lights was nearly perpendicular to the line of sight to the KHOU TV crew. Of more interest is the observation that the bisector of the lights lay almost exactly parallel to the path of travel, i.e., the imaginary line connecting the lights was almost exactly perpendicular to the path as shown in the map.

Distance and Speed Calculated by Another Method

Approximately two seconds after the two lights were side by side at the same apparent altitude, i.e., about 45 seconds before the end of the sighting, the original light ejected, straight downward, a dimmer light which dropped out of the view of the camera. Subsequently it faded out. About 31 seconds after the ejection by the first light (about 14 seconds before the end of the sighting), the second ejected a small light straight downward which dropped below the field of view of the camera. (These falling lights faded out before reaching the ground.) Then it, too, faded out. By plotting the positions of the falling lights as a function of time and fitting them to the gravitational fall equation, $y - y_0 = V_0 t + (1/2)gt^2$, it was possible to determine the initial velocities. The surprising thing is that it was also possible to estimate the distances to the lights! The method, probably of interest only to mathematicians and physicists, is described in Appendix F. The distances, calculated in this way, provide no information on the direction to the lights because no ground reference lights appeared in the video field of view. Therefore I have drawn arcs on the map which cross the estimated track. The arc most distant from the Shoreline Park site, which corresponds to the distance 2 seconds after the lights were side-by-side, does not intersect the track at the triangle on the map. Therefore, this method indicates that the distance at the time when the lights were side-by-side was closer to the camera than the triangle would indicate, providing fur-

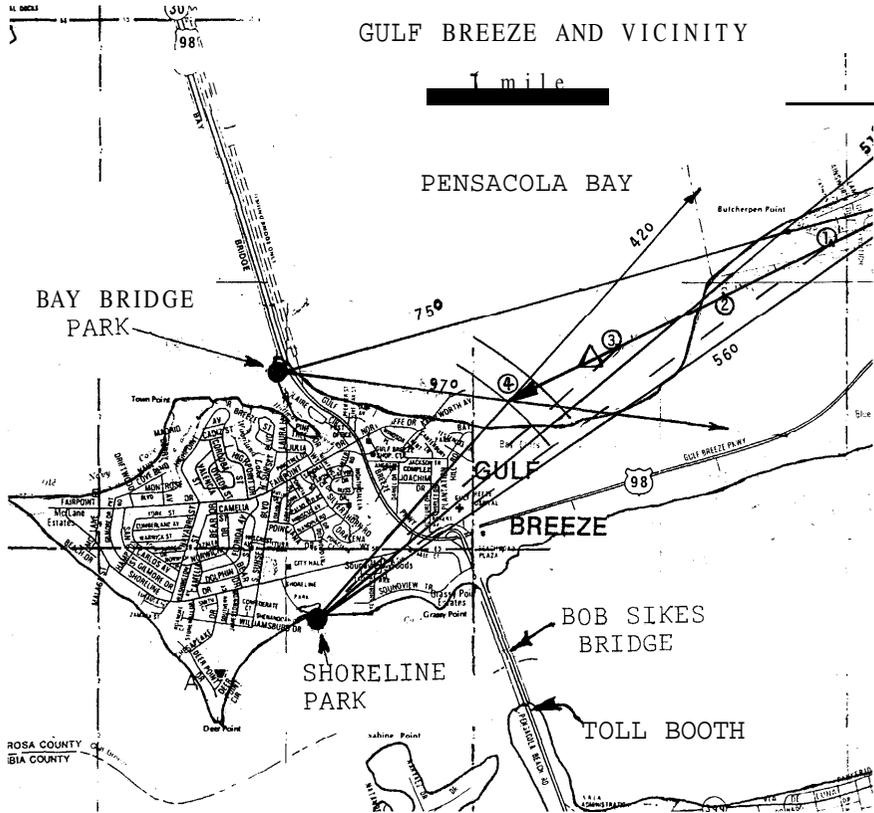


Fig. 1.

ther evidence that the track on the map is not exactly correct. The greater arc crossed the track at a horizontal distance of about 1.72 mi from Shoreline Park. At this time the slant range was about 1.92 miles (see Appendix F). Multiplying this by the angle between the lights, 0.7 mr, yields a spacing of $(1.92 \times 5280 \times 0.7)/1000 = 7.1$ ft as projected onto the line of sight. Taking into account the perspective effect (Appendix E) the spacing was about 7.5 ft. Thirty-one seconds later, when the second light dropped a smaller light, the slant distance was smaller, about 1.75 mi.

The slant distances given above, 1.92 mi and 1.75 mi, the end segment of the track on the map (which provided an estimate of the direction the objects were moving) and the time between the events, 31 sec, can be combined to give the speed. Projecting the slant distances onto the ground yields 1.79 mi for the larger arc and 1.63 mi for the smaller. Hence the lights were moving closer to the camera at the rate of about 0.16 miles per 31 sec or $(0.16 \times 3,600/31 =)$ 18.5 mph, as projected onto the 42° azimuth from Shoreline Park (see map). If they traveled along the track on the map then the actual distance traveled was greater than 0.16 mi, since they were not traveling straight toward the camera.

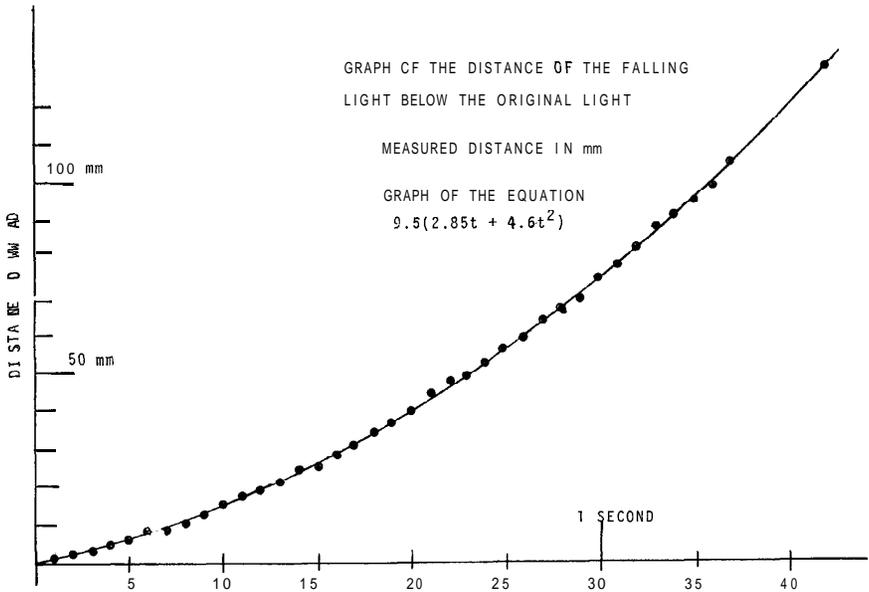


Fig. 2.

The track on the map runs at an angle of about 22° to the line of sight at point (4), so the distance would be $0.16/\cos 22 = 0.17$ mi in 31 sec, corresponding to about 20 mph. This calculated value is about one half of the average speed of about 40 mph calculated from the track length and the 4 minute sighting duration as described above, and it is less than half of the speeds calculated at earlier times by the methods described above (47 mph between points 2 and 3 and 56 mph between points 1 and 2 on the map). The final low speed indicates that the lights were slowing down as they approached the end of the track. If this were so, then the constant speed assumption used above to calculate the path in the map would be wrong and some other path would be needed. (I have not tried other paths because of the inherent uncertainties in the data and because, if one allows for curvature and variable speed, the number of possible tracks becomes very large.)

Further Characteristics of the Lights

The initial image in the B&W video appeared to be several light sources very close together which were revolving around one another. Then a second light appeared to the left of the first. Assuming that the path shown on the map is approximately correct, when the second light appeared, 11 seconds into the sighting, the lights were about 3.1 miles away and spaced by about (17.5 mm on the TV screen; $3.1 \text{ mi} \times 5280 \text{ ft/mi} \times 17.5 \text{ mm} \times .0317 \text{ m/r/mm} = 9 \text{ ft}$ (projected onto the line of sight)). The second light subsequently made a partial orbit of about 200° around the first in a clockwise direction to a location at

about the 5 o'clock position. At this time, about 138 seconds into the sighting, it was about 2.8 miles away (using the path on the map) and was spaced from the original light by about 9 mm on the TV screen; $2.8 \times 5280 \times .0317 \text{ m/m} = 4.2 \text{ ft}$ as projected *onto the line of sight*. It then reversed its revolution, finally ending up at the right side of the first and at the same apparent altitude, as discussed above. A few seconds later the first one ejected something downward and then faded, and subsequently the second light did the same. During the sighting the video showed that the lights dropped glowing matter or objects at the very beginning of the video, when the second light suddenly appeared, and at the very end. On a couple of other occasions very faint glowing material seemed to be emitted downward. However, the emission of material was occasional, not continuous throughout the 3 1/4 minute video.

Discussion

So, what were these lights? It would be "nice" to get a definitive answer. Unfortunately, all one can provide are arguments for or against hypothetical explanations. The conventional simple explanation is that the sightings have all been hoaxes in which the lights were road flares supported by balloons drifting through the sky. However, this explanation does not have "smooth sailing." In favor of the "red flare" explanation is the red color, the tendency for the brilliance to decrease in a fluctuating manner near the end of the sighting and the fact that glowing material fell downward occasionally. Against the flare explanation is the fact that the emission of burning matter was not continuous, but only occurred several times (video of flares showed that they continually drop glowing material while emitting sparks and smoke). Also against the road flare explanation is the fact that no evidence of smoke was recorded by the video, nor was any seen by the witnesses. The lack of smoke might be explainable by assuming that instead of a simple road flare a special pyrotechnic composition was used. However, this assumption of a special pyrotechnic composition increases the difficulty of the assumed hoax.

Although no spectrum was recorded during this sighting, in a previous case (Feb. 7, 1992) the spectrum of an RWL was recorded using a diffraction grating in a camera. Using the same camera and film a few hours later the spectrum of a road flare was recorded. Analysis of the film shows that there are two important differences between the RWL spectrum and that of the flare: (a) the blue to green ratio for the RWL is much greater than the same ratio for the flare, and (b) the red portion of the RWL spectrum does not show line spectra, whereas the flare spectrum does have several lines (spectral "lines" are particular frequencies where there are relatively large amounts of radiation). The semi-quantitative accuracy of the flare spectrum obtained on Feb. 7 was confirmed by comparison with published spectra of military red flare pyrotechnic mixtures (which rely on the element strontium to produce the red color). The lack of lines in the Feb. 7 RWL spectrum suggests that it was not any type of pyrotechnic device, since these devices create plasmas which generate increased radiation at frequencies that are characteristic of the elements which

are burned in the pyrotechnic. Of course, this does not prove that the May 18 sighting was not a pyrotechnic device, although the similarity between the sightings suggests that the results of the Feb. 7 experiment should be considered in evaluating the pyrotechnic explanation for the May 18 sighting.

Certainly this was not just a pair of pyrotechnic devices such as marine flares shot from a gun, nor was it a pair of parachute flares because (a) the duration was too long, (b) such flares fall downward in an obvious way and (c) such flares do not change from red to white and back to red. The next best guess would be a pair of special pyrotechnic devices suspended at some distance apart under a balloon. However, balloons move at the mercy of prevailing breezes and they rise as the weight of the pyrotechnic is reduced by burning, unless a special controlled device is included which can allow the balloon to leak slowly as the pyrotechnic burns. But the RWL, according to the evidence presented previously, actually traveled at a "good clip" into the prevailing wind and dropped downward as it traveled. Also, an object suspended below a balloon, whether on a long or short tether, will swing back and forth in an essentially random way. However, no swinging was noted. Thus motion into the wind and the lack of oscillatory motion of the lights rules out the "simple" balloon hypothesis.

Although no noise was heard (nor has any been reported in any sighting), one might suggest a motorized model plane or blimp. Such a device could move into the wind at some sizeable speed. Ignoring for the moment the expense of such a device, let us pursue this hypothesis a bit farther. Since the light images do not exhibit any swinging motion, one may assume that the structure supporting the flares was substantial and had some form of aerodynamic stabilization. Since there were two lights it would require at least two flares, the second one designed to come on after the first (or perhaps it didn't ignite in time). They were separated by at least 7 ft, yet they maintained coherent motion indicating that they were linked together mechanically as they traveled into the wind. One might imagine a 7 ft long suspension member supporting two flares and supported at its center by the hypothetical motorized blimp. This does not explain, however, how the flares could revolve around one another as did the lights in the video.

Even a motorized blimp or model plane (with a quiet motor) would require some directing mechanism to keep it from flying in circles. A blimp would not go into a crash dive, of course, but model planes tend to do that if they are not controlled. A small military type drone aircraft might do the trick. But either radio control or an "autopilot" would be necessary to direct the vehicle over the "target area." Also, unless one can afford to lose such devices, radio control would be necessary to get the vehicle to return to its owner after the flares had burned out. (Balloons are cheaper of course, and do not need to be retrieved, but balloons can't move against the wind. Incidentally, no balloon/flare debris has been reported in the Gulf Breeze area.)

Needless to say, this "conventional explanation" is not very appealing, espe-

cially considering that this was only one of a number of sightings of pairs of lights traveling through the sky (consider March 14 and April 3, referred to at the beginning of the paper). In May alone there were 16 sightings, with 11 in a row. During the year and a half of sightings there were about 170 RWLs seen. Where does one get all these "motorized blimps?"

Aside from the difficulty in creating the objects that fly through the sky carrying one or more pyrotechnic devices, there is also the risk factor. What if something went wrong? Considering that many of these sightings are many minutes long, the amount of pyrotechnic material must be substantial (the amount of pyrotechnic material would be measured in pounds). Such a device landing on a house or in a field of dry grass, etc., could cause substantial fire damage. Alternately, an object of substantial size at a several thousand foot altitude is a hazard to aircraft. If these are flares as has been claimed, then certain government agencies such as the Coast Guard, the FAA and perhaps even the local police are guilty of nonfeasance of duty by not pursuing and arresting the person(s) responsible for the flares. At the very least the person(s) responsible for flying pyrotechnic devices could be guilty of "reckless endangerment" as well as breaking the law against sending up flares when there is no emergency.

An alternative source of the lights, namely incandescent light bulbs with color filters, has also been proposed. However, this hypothesis is subject to many of the criticisms leveled against the pyrotechnic hypothesis and also some new ones: the lack of stability under a balloon; the need for a motorized support to move rapidly through the atmosphere against the wind; the weight of batteries needed to power the bright lights for many minutes; the requirement for a means to change from white to red momentarily (e.g., a white bulb next to the red bulb, operated by some control circuit); the inherent danger in allowing a device of substantial size to float over a populated area and through an aircraft landing area.

Conclusion

Although some high quality cameras were used to videotape and photograph the RWL that was seen on May 18, the observations were not carried out with the precision of a scientific experiment. Hence it has not been possible to accurately determine the track of the RWL, nor has it been possible to determine the exact nature of the RWL. However, the analysis and discussion presented above does allow one to conclude that lighted objects of substantial size were observed traveling over populated areas of Gulf Breeze on May 18 and, by extrapolation to other similar sightings, on many occasions during 1991 and 1992. Simple hoax methods have been proposed to explain the sightings, including flares and incandescent lights carried by balloons. Numerous arguments against the simple hoax hypothesis have been presented. It has been shown that if these lights were hoax devices then they constitute an "ingenious" and expensive hoax (possibly involving more than one person). Considering that sightings of this nature began in November 1990, the long dura-

tion and number of the sightings would indicate a hoaxer (or hoaxers) with a persistence that is unprecedented in the history of UFOlogy. It would also indicate a rather reckless disregard of the safety of the community.

On the other hand, if these sightings were not hoaxes, then they represent some new, as yet unrecognized phenomenon related to the long-standing UFO mystery.

(Note added in proof: At the time that this report was written sightings were continuing in Gulf Breeze. However, as of November 1992, there have been no RWL sightings since July 13, 1992. The MUFON observers have continued their nightwatches, however, so the ending of the sightings is not a result of lack of observation. A few UFO-type events of a different nature have occurred since July 13. These will be reported elsewhere).

Acknowledgements

I thank Bruce and Ann Morrison, Ray Pollock, Arthur Hufford and Ed Walter of Pensacola/Gulf Breeze MUFON and also Jim Moore of KHOU, CBS TV in Houston, TX for providing the information that was needed to carry out this analysis.

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7. Hufford, A, MUFON Sighting Report for Florida Case 92-98, July, 1992 (private correspondence); this sighting involved 57 known witnesses at 7 locations around Gulf Breeze and included photographs and video.
8. They used a Sony SSC-M370 CCD camera attached to a small telescope consisting of a Tamron 200-500 mm, f/5.6 zoom lens with a 1.6 X teleconverter for an effective optical focal length of about 900 mm at about f/10. When the image is displayed on a 14" TV monitor for analysis the image is magnified by a factor of about 35 which means that the effective focal length is about $31,500 \text{ mm} = 31.5 \text{ m!}$
9. The measured spacing of the street lights is 10 degrees. The image spacing is 78 mm on the 14" monitor corresponding to an angle calibration of 2.22 mr/mm. The light moved 7.5 mm to the left and 5.5 mm upwards during 15 seconds.
10. These angle change rates were estimated by graphing the horizontal position of the image as a function of frame number for a second or so at several times during the video.

Appendix A: The April 3 Sighting

Bruce Morrison's videotape from April 3 shows a maximum spacing of the light images of about 5 mm on the 14 inch monitor that was used for this analy-

sis. This spacing corresponds to some particular angle between the lights. If one knew the angle one could multiply the angle in milliradians (mr) by the distance and divide the product by 1,000 to obtain a good estimate of the spacing as projected onto the line of sight, i.e., as seen in perspective (the distance perpendicular to the line of sight). (Note: if an angle is given in degrees, multiply it by 17.4 to convert it to mr. This estimation method becomes increasingly unreliable as the angle grows beyond about 300 mr.) The angular spacing between images on the monitor was determined by using the calibration factor for Bruce Morrison's video camera. The calibration factor was found, using a light source of known angular size (the moon, 9.2 mr), to be about 0.12 mr/mm. Multiplying the maximum spacing of the images (5 mm) by the calibration factor, 0.12 mrlmm gives the angular spacing as 0.6 mr. Multiplying this by the estimated distance, 16,000 - 21,000 ft, yields a spacing of about 10 - 12 ft.

Appendix B: Ray Pollock's Photo

Dividing the spacing between the images, 0.4 mm, by the camera focal length, 500 mm, gives the angle between the images, 0.8 mr. Multiplying this number by the distance to the object in feet and dividing by 1,000 gives the spacing between the lights in feet as projected onto the line of sight, i.e., a distance between the lights as measured perpendicular to the light of sight to the camera. (If C is the angle between the line of sight and a line, S , joining the lights, then the projected spacing is $S_p = S \cos C$, where \cos is the cosine function; C is 90° when S is perpendicular to the line of sight.) Thus, if the lights were 1,000 ft away the projected spacing would be 0.8 ft; if it were 2,000 away the projected spacing would be 1.6 ft, etc. The projected spacing is always equal to or less than the actual spacing so the actual spacing could have been greater than 0.8 ft ($S = S_p / \cos C$ and the value of $\cos C$ is 1 or less).

Appendix C: The Path of the Lights

The basic assumptions are (1) that the lights moved at a steady rate, and (2) that they moved along a straight path. Any other assumptions will make the motion of the object more complicated (e.g., curving, accelerating, decelerating). Next, I assumed that during the first minute (including the 45 seconds before the B&W video started) the Azimuth Angle Change Rate, as seen from Shoreline Park, was 2° per minute (counterclockwise) and that during the last minute the AACR was $6.5^\circ/\text{min}$. Next, I carried out the following operations using trial travel paths: (1) I drew a trial azimuth line from the Shoreline Park site until it crossed the 75° azimuth at the right side of the map (this is the trial starting point of the track of the lights); (2) following the assumption that the lights traveled in a straight line, I drew a straight line from the trial starting point to the intersection of the 42° and 97° azimuths (the end point of the path as projected onto the surface of the earth); (3) following the assumption of constant speed, I divided the path into four equal segments corresponding to the four minutes of the sighting; (4) I drew lines from the Shoreline Park site to

the 1 minute and 3 minute marks on the path (see the circled numbers and dashed lines on the map); (5) I compared the change in angle between the (dashed) line to the 1 minute mark and the trial azimuth (e.g., 56") with the required change in angle during the first minute (2 degrees); (6) I compared the angle between the (dashed) line to the 3 minute mark and the 42° azimuth with the required change in azimuth during the last minute (6.5"); (7) if I found that the angle changes measured in steps (5) and (6) did not match the required angle changes I rejected the trial azimuth and drew another trial azimuth line. I repeated the above procedure until I found an azimuth that obeyed the conditions just listed. This azimuth, 56°, is shown as the starting point of the track of the RWL. (This trial and error procedure was much more sensitive to the change in azimuth during the last minute. That is, trial azimuths were rejected generally because they did not meet the 6.5°/minute drift rate during the last minute.)

Appendix D: The Projected Spacing Between the Lights

The spacing of the images on a 14 inch monitor was 22 mm for the B&W and 3.5 mm for the color video. Hence the effective focal length of the B&W camera was about 6.3 times greater than that of the color camera. The effective focal length of the color camera was determined from the image size of the moon (about 9.2 mr). The diameter of the moon image is 46 mm which gives a calibration of about 0.20 mr/mm for the color camera and, since the spacing of the two lights was about 6.3 times greater on the B&W video, $0.216.3 = 0.0317$ mrlmm for the B&W camera. The effective focal length in meters is the inverse of the focal plane calibration in mr/mm (which is equivalent to radians/meter). Hence the effective focal length of the color camera was about 5.0 meters and that of the B&W camera was about 31.5 m. (Note that this includes the "blowup factor" created by displaying the image on a 14" TV monitor which is much larger than the size of the focal plane of the TV camera.)

Appendix E: The Actual Spacing Between the Lights

By assuming that the spacing, S , between the lights was greater than 8.3 ft and that the bisector of the lights (a line perpendicular to the imaginary line connecting the lights) lies between the two viewing directions, one can solve two equations which make the calculated apparent spacings completely self consistent: $A + B = 41"$ and $(S1/S2) = \cos A / \cos B = 8.318.1 = 1.025$, where A is the angle between the bisector and the line of sight to the KHOU crew, B is the angle from the bisector to the GBRT, and S , the actual spacing, is given by $S = S_1 / \cos A$. (I have ignored the slight effect of the difference in angular elevations from the two sites.) The two equations are solved to a sufficient accuracy for $A = 18.5^\circ$ and $B = 22.5"$. Hence the actual spacing was about $S = 8.3 / \cos 18.5 = 8.8$ ft.

Appendix F: Ejection Velocity and Distance Estimated from the Gravitational Free-Fall Equation

After observing the video of the falling lights several times I realized that, if they fell under the acceleration of gravity, then it should be possible to determine how far away they were by fitting a graph of the position as a function of time to the gravitational fall equation. After I worked through some theoretical calculations I found out how to adapt the gravitational fall equation to the video imagery. One must first realize that positions of the image of the falling light (on the TV screen) are related to positions of the actual falling light at some distance from the camera by the equation $Y^S - Y^S_0 = K(y - y_0)$ where $K = (F/S)(\cos E)$ and $y - y_0 = V_0 t + (1/2)gt^2$ (distances, y , are positive downward). The values of $Y^S - Y^S_0$ are the image distances downward from the upper (ejecting) light to the falling light at various times. F is the effective focal length of the camera (including the blowup factor of the TV monitor), S is the slant range from the camera to the falling light, E is the angle of elevation of the viewing direction, V_0 is the ejection velocity and g is the acceleration of gravity (9.8 m/s^2 or 32.2 ft/s^2). The factor $\cos E$ projects the fall distance at anytime, assumed to be straight down (air drag is ignored) onto the sighting line direction. F was determined to be $31.5 \text{ m} = 31,500 \text{ mm}$ (see preceding discussion) and E is the estimated angular elevation at the end of the sighting, about 21° from the Shoreline Park site. The only unknown quantities are S (in m) and V , in m/sec. Figure 2 is a graph, for the first case of the ejected lights, of the distance between the lower (ejected) light and upper (ejecting) light, i.e., of $(Y^S - Y^S_0)$. The dots are measured distances on the TV screen in mm and the solid line was generated by the equation $(Y^S - Y^S_0) = K(V_0 t + 4.9t^2) = (9.5 \text{ mm/m})(2.85 \text{ m/sec} \times t + 4.9t^2)$. (For mathematicians: there were two unknown constants in this quadratic equation, K and V_0 . They were varied independently to achieve a good fit to the data.) The initial velocity was 2.85 m/sec . The value of K could vary by as much as 0.1 mm/m (i.e., $K = 9.5 \pm 0.1$) corresponding to about 1% accuracy in the fit to the data. Since $K = 31,500 \cos 21/S$ we can solve for S and find $S = (31,500 \cos 21)/(9.5) = 3095 \text{ m} = 1.92 \text{ miles}$. The horizontal component of this slant distance is $S \cos E = 1.79 \text{ mi}$, which is the length of the greater arc drawn on the map. A similar graph was made of measured distance points for the second light that fell downward and it, too, was well fitted by the above equation but with different values of V , and K . In this case $V = 4.2 \text{ m/sec}$ and $K = 10.4 \text{ mm/m}$, which corresponds to $S = 2820 \text{ m} = 1.75 \text{ mi}$ and a horizontal distance of 1.63 mi (the shorter arc on the map).

Selection Versus Influence in Remote REG Anomalies

YORK H. DOBYNS

Princeton Engineering Anomalies Research, Princeton University, Princeton, NJ 08544

Abstract—A large body of remote human-machine interaction data has been collected in a protocol structurally similar to that used for experiments in remote perception, with somewhat comparable anomalous results. This suggests that the effects seen in the former could be attributable to a selection or sorting process on a reservoir of unperturbed data, rather than to any remote influence on the machine behavior per se. Fortunately, the statistical consequences of these two modalities are clearly distinguishable within the available empirical data. When properly evaluated by Bayesian hypothesis-comparison methods, the experimental results overwhelmingly favor the direct influence hypothesis over any selection mechanism.

Background

A substantial body of experiments in remote human/machine interaction involving various types of random event generator (REG) devices has displayed significant correlations between operator intentions and the mean values of the output distributions of the machines, despite considerable spatial separations between operator and machine (Dunne and Jahn, 1992). These results are not only consistent with those produced under local conditions, but also bear some similarity to those of precognitive remote perception (PRP) experiments, where individuals have demonstrated a statistically significant ability to acquire information about locations remote in distance and time (Dunne, Dobyns, and Intner, 1989).

From one point of view, the remote REG experiments could be regarded as a task in remote influence on the machine's behavior that automatically incorporates a number of controls that are unavoidably lacking in the local procedure. For instance, the physical absence of the operator from the laboratory effectively eliminates any possibilities for operator deception, while the possibility of experimenter influence is precluded by the experimenters remaining blind to the operators' intentions until after the data are generated and recorded. From another perspective, the future outcome of a remote REG session, as recorded in a logbook and in a computer file, could be regarded as a form of simple PRP target in which the operator attempts to identify the highest and lowest of the three recorded mean values. This alternative hypothesis becomes more plausible in view of certain dissimilarities of results using different REG noise sources. While electronic diode sources display statistically significant

anomalous yields in both local and remote studies, the various pseudo-random sources show significant yields only in the remote experiments. If the remote interaction is driven by a remote perception effect, to which the nature of the noise source would be irrelevant, the presence of anomalous yield from both sources would be a natural result. (The contrast between remote and local performance on the pseudo-random experiments would then suggest different mechanisms for those effects, but that issue is beyond the scope of this paper.)

Invoking such a selection model may seem simply to be handwaving to explain one anomaly with another. However, these two models make sufficiently different statistical predictions that they can be distinguished with some confidence even on a rather modest dataset. It should be noted that this analysis is directed only peripherally at the issue of the existence of a real effect. The central question addressed is not the credibility of the effect per se, but rather the consistency of the effect with a particular class of mechanisms, which must be the first step in the production of useful theoretical models.

Selection versus Influence

For purposes of this discussion, the *selection* model denotes the hypothesis that the operator's efforts do not in any way affect the operation of the device, but that the operator is able by unspecified means to assign the three intentions of each experimental session in accordance with the actual values that will subsequently be produced. The *influence* model proposes in contrast that the behavior of the device is actually different under the various experimental intentions. In both cases the assumed target of the experiment is the mean of the experimental data, and the anomalous result is the deviation of that mean from null-hypothesis expectation. As shown in Fig. 1, the salient difference in the two models lies in the relationship between the mean shifts in the data and the relative rankings of the intentional runs. Under the selection model, the behavior of the device is undisturbed and any alteration in the mean values of, for example, high runs must derive from their preferential origin as the highest of three randomly generated values. In the influence model, the differential ranking of the high intention relative to the others is driven by, rather than driving, the underlying difference in distributions.

Statistics of the Selection Model

The theoretical distribution of run-terminal scores is essentially normal, and all following discussion will assume the scores have been reduced to standard normal deviates. Since the selection model assumes the effect to be driven by the assignment of intentions to the three runs of each tripolar set, the crucial issue is thus the distribution of the highest, lowest, and midmost of a set of three normally distributed values. The probability density for x drawn from a standard normal distribution, conditional upon x being the highest of 3 such draws, is:

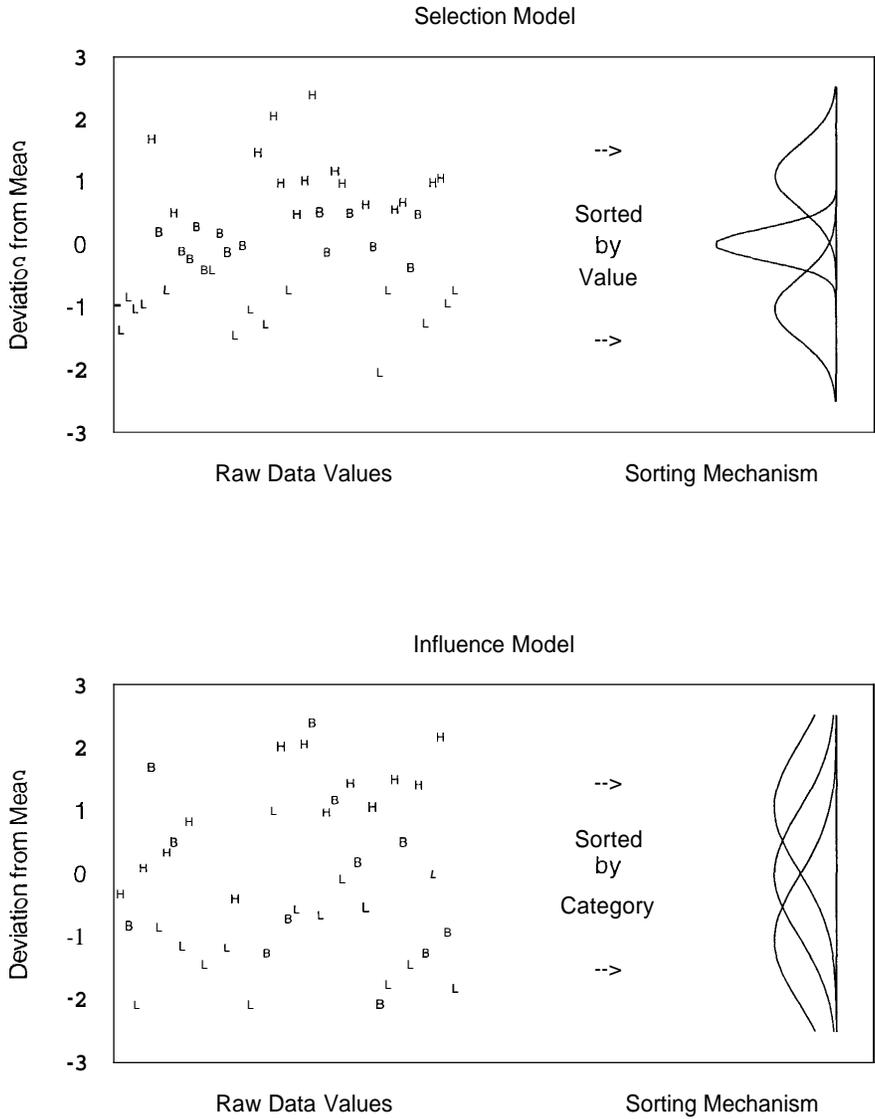


Fig. 1. Exaggerated Illustration of Differences in Models.

$$p(x) = f(x)F(x)^2 / \int_{-\infty}^{+\infty} f(x)F(x)^2 dx, \tag{1}$$

where

$$f(x) = (1/\sqrt{2\pi})e^{-x^2/2} \quad F(x) = \int_{-\infty}^x f(t)dt$$

are the probability density and cumulative probability distribution of the standard normal function. The numerator of (1) is the joint probability of a value x appearing and of two lower values being drawn from the same distribution; the denominator simply normalizes the expression so that the total integral of p is 1. Integrating the moments of (1) numerically, one finds that the highest-of-three distribution has the mean $\mu_H = 0.8463$ and the standard deviation $\sigma_H = 0.7480$. The lowest-of-three distribution by symmetry has the same standard deviation and opposite mean, and the middle-of-three has $\mu_M = 0$, $\sigma_M = 0.6698$. If these three distributions are recombined in equal proportions, one of course retrieves the parent standard normal distribution. (This is equivalent to saying that if you are required to label one of three numbers from a standard normal distribution as "high", but have equal probabilities of choosing the highest, lowest, or midmost of the three for the label, over many runs your choices of "high" will themselves follow a standard normal distribution.)

The essence of the selection model is the supposition that the operator is able to choose the highest of the three runs to be labeled as "high," and/or the lowest to be labeled as "low," with some probability greater than the chance 1/3. For the moment, consider only the operator's declared high intention, which actually turns out to be the lowest run of the three a fraction p_L of the time, the highest run a fraction p_H of the time, and on the remaining occasions, $p_M = 1 - (p_H + p_L)$, the middle run. Given the three distributions just described, it follows that the distribution of high runs has the mean and variance:

$$\mu = p_L \mu_L + p_M \mu_M + p_H \mu_H = \mu_H (p_H - p_L),$$

$$\sigma^2 = \sum_{x \in L, M, H} p_x (\mu_x^2 + \sigma_x^2) - \mu^2 = \sigma_M^2 + (\mu_H^2 + \sigma_H^2 - \sigma_M^2) (p_H + p_L) - \mu^2. \quad (2)$$

These equations may be inverted to give the selection frequencies needed to produce a given mean and variance:

$$\begin{aligned} p_L &= \frac{1}{2} \left(\frac{\sigma^2 + \mu^2 - \sigma_M^2}{\sigma_H^2 + \mu_H^2 - \sigma_M^2} - \frac{\mu}{\mu_H} \right); \\ p_H &= p_L + \frac{\mu}{\mu_H}; \\ p_M &= 1 - (p_H + p_L). \end{aligned} \quad (3)$$

Obviously, similar relations can be derived for the low and baseline efforts. It should be noted, however, that all of these equations will produce p 's that are valid probabilities only for a limited range of μ and σ . No possible selection scheme can produce $\mu > \mu_H$, for example. Figure 2 illustrates the region in (μ, σ) space that can be potentially accommodated by a selection model.

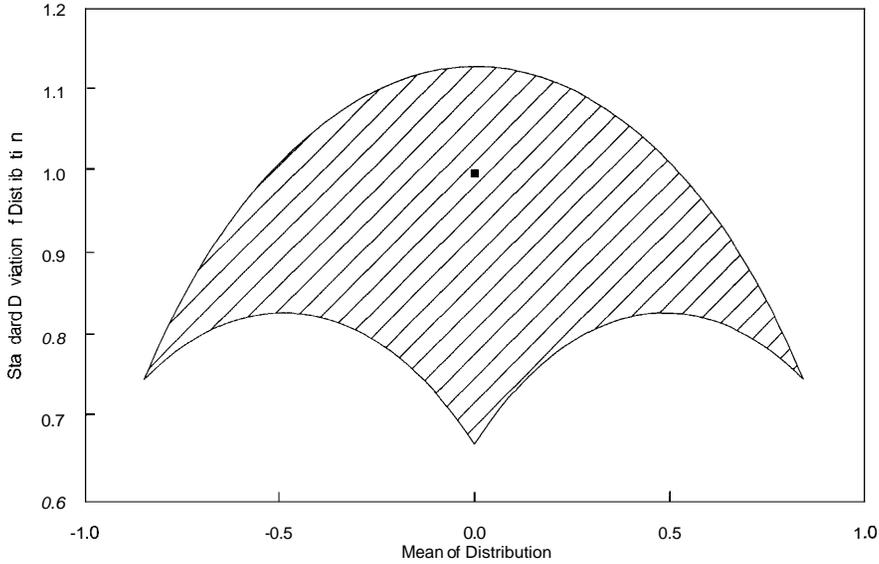


Fig. 2. Region of Possible Parameter Values for Selection Model.

Statistics of the Influence Model

The prior section showed how the rankfrequencies p_H, p_I, p_M for any intention can be derived, under the selection model, from the mean and variance of the scores in that intention. Rank frequencies can likewise be derived for the influence model. If presented with three datasets whose empirical mean and variance estimates are $m_1, m_2, m_3, s_1^2, s_2^2, s_3^2$ respectively, the frequencies with which a value drawn from distribution 1 will be highest or lowest are:

$$\begin{aligned}
 p_{1H} &= \int_{-\infty}^{+\infty} F\left(\frac{x - m_2}{s_2}\right) F\left(\frac{x - m_3}{s_3}\right) \frac{1}{s_1} f\left(\frac{x - m_1}{s_1}\right) dx, \\
 p_{1L} &= \int_{-\infty}^{+\infty} \left[1 - F\left(\frac{x - m_2}{s_2}\right)\right] \left[1 - F\left(\frac{x - m_3}{s_3}\right)\right] \frac{1}{s_1} f\left(\frac{x - m_1}{s_1}\right) dx, \\
 p_{1M} &= 1 - (p_{1H} + p_{1L}).
 \end{aligned}
 \tag{4}$$

Consistency Criteria

There are two constraints on the values of the rank frequencies. The first, that every run (and therefore every run with a particular intentional label) must be assigned some rank, is automatically satisfied by the formulae given above; it does, however, mean that only two of the three rank frequencies for any intention need be calculated. (This ignores the possibility of ties. Ties are present

but infrequent in the actual data, and are most easily dealt with simply by discarding that relatively small number of tripolar sets.) The second is that every run, whatever its rank, must be assigned an intention, and therefore the proportions of (for example) highest-ranked runs assigned to high, low, and baseline intentions must also add up to one. This appears to present three more constraint equations on the rank frequencies, but in fact one of them is redundant. Since this places a total of five constraint conditions on the nine rank frequencies, determining four of the rank frequencies will suffice to determine the entire system. The calculations detailed below fix the matrix of rank frequencies by computing the four values R_{HH} (probability with which the high intention has the highest rank), R_{HL} (probability that the high intention has the lowest rank), R_{LH} and R_{LL} as the key values that determine the matrix.

A final consistency criterion applies to the normalization of the data. Since the selection model assumes the standard normal distribution for the source, the consistency constraints on the rank frequencies actually impose a condition on the means and standard deviations of the three intentions such that, when they are recombined, the composite distribution must have exactly $\mu = 0$, $\sigma = 1$. Therefore, in order to make valid rank frequency calculations in the selection model, the data must be normalized to their own, overall statistics across the three intentions (rather than to the theoretical performance of the machine). The influence model does not impose such a condition, since it will make exactly the same predictions for relative rankings regardless of what normalization is used for the raw data.

Statistics of the Actual Data

The remote REG database consists of 494 tripolar sets, of which 398 were generated using a microelectronic noise diode source and 96 using some form of pseudo-random device. All systems were extensively calibrated and failed to deviate significantly from the theoretical distribution in those calibration runs. Four of the experimental sets, all generated with the diode device, contain ties and are excluded; when the remaining 490 sets are normalized to their own collective mean and standard deviation, the results take the following form:

| Int. | Mean | Std. Dev. | N Highest | N Middle | N Lowest |
|------|----------|-----------|-----------|----------|----------|
| H | 0.08825 | 1.03035 | 180 | 167 | 143 |
| B | -0.03619 | 0.97074 | 159 | 156 | 175 |
| L | -0.05207 | 0.99413 | 151 | 167 | 172 |

Interpreting the Observation

Using the relations developed earlier, we can construct predicted rank frequencies from the means and standard deviations of the observed data, for either the selection model or the influence model, for comparison with the observed rank frequencies. Since these predictions are quite different, the observation should provide better support for one hypothesis than the other.

The most straightforward way of assessing such evidential support is by an elementary application of Bayes' theorem of conditional probabilities. This asserts that the relative support an observation gives to two competing hypotheses is simply the ratio of the probabilities or "likelihoods" of that observation under each hypothesis. For each intention we are confronted with, in essence, a three-way choice experiment conducted n times. The three possible outcomes are observed n_1, n_2, n_3 times respectively ($n_1 + n_2 + n_3 = n$). The probability of this observation under a hypothesis p_1, p_2, p_3 concerning the probabilities of the individual outcomes is

$$P[n_1, n_2, n_3 | p_1, p_2, p_3] = \frac{n!}{n_1! n_2! n_3!} p_1^{n_1} p_2^{n_2} p_3^{n_3}. \quad (5)$$

The combinatorial factor depends only on the observations, and therefore cancels out in the probability ratio between different hypotheses on the same observation. If the two hypotheses predict (p_1, p_2, p_3) and (q_1, q_2, q_3) respectively, the ratio of relative support is

$$B(p/q) = \frac{p_1^{n_1} p_2^{n_2} p_3^{n_3}}{q_1^{n_1} q_2^{n_2} q_3^{n_3}} = \left(\frac{p_1}{q_1}\right)^{n_1} \left(\frac{p_2}{q_2}\right)^{n_2} \left(\frac{p_3}{q_3}\right)^{n_3}. \quad (6)$$

The aggregate likelihood of the hypothesis over all three intentions may be calculated by repeating the individual likelihood calculation for each intention, and the total likelihood ratio will simply be the product of factors such as (6) above for each of the three intentions. The above discussion could as easily have been framed in terms of the assignment of intentions to ranks, rather than of ranks to intentions; this amounts to considering the columns rather than the rows of the 3×3 rank frequency matrix first, and leads to the same final result for the overall odds ratio.

A Complication

The foregoing discussion makes a tacit assumption that in calculating the predictions of each hypothesis, we know the exact distribution statistics of the three intentions' run scores, when in fact the observation only estimates these parameters. Because the selection model's predictions in particular are strongly dependent on these distribution parameters, our calculation of relative support will be erroneous if we pretend that the observed values are exact. This may be illustrated by considering an extreme example. Suppose that the selection model is in fact true and that, moreover, the highest run in a tripolar set is being assigned to the high intention with 100% efficiency. Suppose further that, due to statistical fluctuations, the actual mean of the high dataset is slightly higher than its theoretical population value $\mu_H = 0.85$, as will happen by chance about half the time. The selection model cannot accommodate so high a value at all, and thus has a likelihood of exactly zero regardless of the observed rank frequencies, if the observed value is taken as an exact population

value. Since this model is true by hypothesis, the calculation must itself be invalid, and the error lies in taking the measured value as exact.

Therefore, to complete the application of Bayesian hypothesis comparison, we must consider that the observation on the mean and standard deviation of each intention defines a probability distribution on the population values giving rise to the measurement. Calculating the aggregate likelihood of each hypothesis thus becomes slightly more involved. The previous section showed how to calculate a likelihood from a set of rank frequency observations and theoretical predictions. Earlier sections showed how to predict rank frequencies from distribution statistics for both selection and influence models. Taken together, these results give us a functional mapping from distribution statistics onto likelihoods for each model; that is, either the selection model or the influence model can proceed from a set of means and standard deviations, and the associated rank frequency observations, to a final likelihood.

To implement this approach, let $L(\mu_h, \mu_l, \sigma_h, \sigma_l)$ be the function that describes the likelihood of a set of distribution parameters given the observed rank frequencies. (L is of course a different function for the two hypotheses.) The actual distribution parameters are known only probabilistically from the observations. The observed value m_h for the mean of the high distribution, for example, produces a normal likelihood distribution $p(\mu_h|m_h)$ for the actual value of the mean. Therefore, the overall likelihood of the hypothesis, given both the observed means and standard deviations and the observed rank frequencies, is

$$P \propto \int \int \int \int d\mu_h d\mu_l d\sigma_h d\sigma_l p(\mu_h|m_h) p(\mu_l|m_l) p(\sigma_h|s_h) p(\sigma_l|s_l) L(\mu_h, \mu_l, \sigma_h, \sigma_l). \quad (7)$$

Evaluating this four-dimensional integral is somewhat tedious, but amenable to standard numeric quadrature techniques. Formula (7) is expressed as a proportionality rather than an equation because we have not troubled with normalizing the assorted probabilities and likelihoods appropriately. Since we will be calculating this quantity twice, using the two different functions L required by the two models, and taking the ratio, overall normalization is unimportant so long as the same normalization is used for each hypothesis.

Analysis Results and Interpretations

In summary, the procedure for comparing the experimental support for the selection model versus the influence model involves three steps:

1. Compute the probability distributions for each of the model parameters $\mu_h, \mu_l, \sigma_h, \sigma_l$ that follow from the corresponding observed values m_h, m_l, s_h, s_l .
2. Calculate for each model the weighted integrals (7) of the likelihoods of the observed rank frequencies over the range of possible model parameters.

3. Take the ratio of the likelihoods to determine the odds adjustment factor B between the two hypotheses that follows from the observation.

When this procedure is applied to the values given in the section "Statistics of the Actual Data," one finds that the odds ratio is 28.9 to 1 in favor of the influence model. In other words, given only the observation and no prior information concerning the relative plausibility of the two models, one would conclude that the influence model is about 29 times more likely as an explanation of the facts than the selection model. This factor is also the numerical adjustment that would be applied to a preexisting odds ratio between the two hypotheses, e.g. from prior experiments or theoretical considerations, in computing the effect of the current experiment on the overall relative credibility of the hypotheses.

It should be noted that while this sort of odds ratio is not directly comparable to a traditional p-value, it is clearly imposing. Consider a simple, one-parameter test against a null hypothesis that the parameter has a certain value, in which the possible measurement error is normally distributed. If a Bayesian hypothesis test returns odds of 28.9 in favor of the null, it is easy to show that the classical p-value must be no larger than $p = 0.0095$, and may well be considerably smaller. (This best case assumes that the alternate hypothesis is the "maximum likelihood hypothesis," namely that the observed value is exactly the actual value. Any other alternative hypothesis, to be favored over the null by a factor of 28.9, requires an observation such that the classical p-value is considerably less than 0.0095.)

As a check on the validity of the analysis, the procedure was repeated with two synthetic datasets, wherein the source of deviations between intentions was known by construction. In the synthetic "influence" data, a uniform mean shift was applied to the high intention, and an opposite one to the low intention, of magnitude similar to that seen in the actual data. The analysis on the synthetic data returned an odds ratio of 31.4 in favor of the influence model. The second dataset was constructed by selection, that is, by generating random tripolar datasets and preferentially choosing those that were in the "right" rank order between intentions, again in such a way as to approximate the effect size seen in the actual data. The odds calculation procedure on these data produced odds of 0.00018 favoring influence, or in other words of 5500 to 1 in favor of selection. The test calculation thus appears to discriminate effectively between the two idealized mechanisms. Equally important, the quantitative scale of the odds ratio for the actual data is almost as large as for the ideal "influence" case, indicating that the evidence in favor of the influence model is about as strong as one could reasonably expect, given the size of effect and the amount of available data.

Further Considerations

It was noted in the introduction that the statistical test here used does not address the evidence for the existence of any effect, versus the null hypothesis that all differences between intentional groupings are due to statistical fluctua-

tion. That issue has been thoroughly addressed elsewhere (Dunne & Jahn 1992). In fact, applying the test to datasets that, by construction, contain no effect, yields strong odds (ranging, in a modest Monte Carlo database, from 8.5 to over 100) in favor of the influence model. It is easy enough to see why this should be so. In the completely randomized Monte Carlo calculation, the differences between datasets are due to random fluctuations and therefore display the same rank-frequency character as real population differences. Since selection is not taking place, the selection model is strongly refuted, and in the two-hypothesis test this manifests as evidence in favor of influence.

It might be suspected that the exclusion of the tied datasets is in some way prejudicial. This has been compensated for in two ways:

- (1) In the construction of the artificial datasets above ties were discarded in exactly the same manner; and
- (2) an earlier analysis, in which ties were less awkward because consistency criteria were not taken into account, found that including the tied runs changed the final odds ratio by only about 10%.

As noted above, the 490 (non-tied) tripolar runs were produced by two different types of noise source, one of which is only pseudo-random. Selection models are superficially more appealing than influence models for the pseudo-random data, since it is difficult to imagine how a deterministic string of pseudo-random values might be "changed." Thus, one might expect that in the pseudo-random data, at least, the observation would favor the selection model. However, this proves not to be the case. If the data are separated into subsets according to whether their source was a noise diode or a pseudo-random mechanism, we find:

- (1) The diode source data produce odds of 25.6 to one in favor of influence. The slight reduction in strength of evidence is entirely attributable to the reduced amount of data.
- (2) The pseudo-random data produce odds of 6.3 to one in favor of influence. Here the dataset is much smaller, so that strong statistical conclusions are not to be expected; nonetheless the evidence seems to show modest preference for the influence model.

Conclusions

The analysis described above thus allows some general conclusions:

1. Despite its conceptual appeal, the selection model is a considerably poorer predictor of the data structure than the influence model.
2. This remains true for either type of noise source, despite the fact that the selection model might seem more natural for the pseudo-random source than an influence model.
3. Since the selection model addresses phenomenology rather than mechanisms, evidence against it is evidence against any predictive, perceptual,

or data-sorting model of remote human-machine anomalies. It is also evidence against deception or failure of controls in the remote protocol; if the effect were due to accidental or deliberate cuing of operators, or to any other protocol failure that allowed them knowledge of the run results before their intentions were recorded, the output would necessarily show the statistical character of a selection model.

We thus conclude that whatever the operators are doing, they are not simply sorting the undisturbed output of the device into intentional "bins." Nor are they, by accident, chicanery, or anomalous means, finding out the values of existing runs and choosing the intentions to suit. Rather, in so far as the evidence allows us to judge, the mean output level of the device is genuinely, albeit slightly, different from one operator intention to another. Any explanatory model for the apparent anomaly must take this fact into account as its starting point.

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Correspondence should be addressed to York Dobyns, Princeton Engineering Anomalies Research, C-131 Engineering Quadrangle, Princeton University, Princeton, New Jersey 08544-5263.

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Dutch Investigations of the Gauquelin Mars Effect

J. W. NIENHUYS

*Department of Mathematics and Computing Science, Eindhoven University of Technology,
P.O. Box 513, 5600 MB Eindhoven, The Netherlands*

Abstract—A team of Dutch skeptics have investigated a new explanation for the Mars effect with sports champions of Michel Gauquelin. They conjectured that outstanding sports people might have diurnal and seasonal birth rhythms different from average people and that moreover the short time base of Gauquelin's observations might further enhance these effects. Essentially their findings were negative. Simulations were either not possible because of lack of data or they showed that along these lines an explanation is only possible if very implausible additional assumptions are made. However, it is argued that the Gauquelin data suffer from a bias, namely some artifact of the exploratory phase. The eminence effect of Ertel is shown to be too weak to draw firm conclusions about its existence. It seems plausible that the Gauquelins did not realize that said artifact had to be tightly controlled for.

Introduction

The Gauquelin Mars Effect consists of the finding that excellent athletes are born slightly more often than other people around the time that the planet Mars either rises or culminates. After Michel and Françoise Gauquelin had observed, around 1950, a similar effect with eminent French physicians (and other planets), the effect with Mars and sports champions attracted the most attention.

It has been conjectured that the Mars Effect is caused by an interplay between demographic and astronomical factors. When in conjunction, Mars is about five times farther from Earth than in opposition, and hence its apparent angular velocity with respect to the Sun is smaller by the same factor. This means that the planet Mars spends comparatively more time in conjunction, and therefore a Mars rise will also coincide more often with sunrise. As there are also disproportionally more children born in the early morning, one may expect that more children are born in a two-hour period following Mars rise.

The exact effect is not so easy to determine. In the first place, Mars sectors last only two hours on average. In fact, traditional astrology divides the time Mars is above the horizon into six "houses," which are more or less equal parts, and the time Mars dwells below the horizon is divided similarly. There are several house systems (methods to make these divisions), and the Gauquelin sectors are just the houses of the Placidus system.

A given Mars sector (say the first, after Mars rise), can therefore vary just as much in length as the duration of daylight. But short first sectors and long first

sectors do not occur in fixed seasons, because the Mars orbit is not an integral number of years. Likewise, the times of Mars opposition and conjunction to the Sun rotate through the seasons.

Nonetheless, the Gauquelins had taken these facts into account and had computed that it raised the percentage of people born in the characteristic Mars sectors by a half point, namely from 16.7% to 17.2%. The number of top athletes in characteristic sectors was around 22%.

The Mars Effect has been the subject of three tests. In cooperation with the Belgian Comitk Para (1976) a test was undertaken that confirmed the effect. The Comitk Para doubted the assumptions and the computations of the Gauquelins, especially the assumption of the constancy (between 1872 and 1945) of the diurnal birth rhythm. Also they questioned the assumption that all combinations of season and Mars position with respect to the Sun were adequately represented in the period mentioned.

A sequel experiment (the Zelen test) investigated the above-mentioned astro-demographic effect. Perhaps those who proposed the test expected it to yield a rational explanation for the outcome of the Para test, but as it happened, only the correctness of the Gauquelins' half percent estimate for the astro-demographic effect was confirmed.

One American test, in 1978 (Kurtz *et al*, 1979a) gave negative results. The results were disputed, and the dispute was further obscured by a similar (and quite unnecessary) dispute about the interpretation of the Zelen test result.

Recently there have been several developments. Ertel has obtained from the Gauquelins more data, including hitherto unpublished ones. He has argued (Ertel 1988) that even though some selection bias is apparent in these data, the principal claim of the Gauquelins still stands, namely that the Mars effect increases with increasing fame of athletes, the "eminence effect." An investigation by the French skeptic group CFEPF has been done, but the results are not published yet. (Incidentally, the Mars effect claim was part of a more general claim, namely that other groups of outstanding professionals such as artists, politicians, scientists and physicians were likewise associated with other planets.)

Spurious Periodicities?

A number of Dutch investigators have explored another possibility, namely that an astro-demographic effect of a different nature is at work. Their work is reported in the Proceedings of the Third EuroSkeptics Congress (1992), but much of their work was done after the congress, actually.

Their explanation was termed "spurious correlations" by the principal investigator, the astronomer Cornelis de Jager. The idea is that the Gauquelin correction of 16.7% to 17.2% may very well be correct in the long run for ordinary people, but fail in the short run for sportsmen. The sportsmen of the Gauquelin data are all born within one century of each other, and the bulk of them within an even much shorter period. If the births of sportsmen-to-be were to show seasonal or daily rhythms different from "average" people, the

interference of these births rhythms with the periodical apparent orbit of Mars might show up in the data. The idea that the time basis is too short is already part of the objections of the Comité Para, but the idea that athletes might differ from "ordinary" people is new.

Such "spurious periodicities" are not an invention of De Jager; they are a common nuisance in signal analysis in the physical sciences, and in astronomy in particular. On the other hand, only close investigation of the Gauquelin data can show what role they might play in the Mars Effect. To do so, De Jager and others (Rieks Jager, Piet H. Jongbloet, Carl Koppeschaar) addressed three questions: (1) how far, from a physicist's point of view, is the Mars effect removed from "random noise"? (2) what kind of periodicities might be specific to excellent sportsmen? (3) can these give rise to the Mars effect?

The physicist's approach in a problem like this is to isolate the purported effect in its clearest form, and try to account for that. In this case this meant concentrating on the surplus in sector 1, that is, in a two-hour period following the rise of Mars. Ertel investigated the eminence effect not only with the original 2087 sports champions of M. & F. Gauquelin (1970), but also for 2303 champions whose data were published later or not at all. However, in these later data no sector jumps out so convincingly that an explanation is warranted.

Here a remark on the methods in physics is in order. Physicists are used to dealing with data that are obtained under well-controlled circumstances. They often have a clear idea of the type of random or systematic errors their measuring apparatus can introduce. Only when "effects" of at least three (and sometimes five) standard deviations show up in data without any apparent theoretical explanation, are they considered worth the effort of investigation. The rationale for this is not so clear. There need not be any relation between the size of the known random errors and the unknown systematic errors, but only when the "effect" is somewhat larger than the known errors is there any hope of tracking it down. Of course, for an experimentalist, improving measuring technique (by locating a hitherto undetected bias) is almost as interesting as discovering a new and unknown "effect."

Whether one chooses three or five standard deviations as the investigation threshold depends on how perfect one's experimental technique is. If one doesn't know for sure whether some kind of bias is overlooked, it is rational to be careful and take five standard deviations as threshold, but if one considers the experiment to be perfect, it is a logical step to put the threshold at three.

If we apply this philosophy to the Gauquelin data, there is nothing left to investigate. We have to pretend that we have such a clear idea about whatever has produced these data, that three standard deviations already are "significant," that is, warrant further investigation.

It must be emphasized that the above is about "effects" without theoretical explanation. When scientists have a theory that predicts some effect, they are less strict about confirmation.

Ertel has disagreed with De Jager and Jager's localization of the first sector in the original Gauquelin data as the only effect needing explanation. De Jager

and Jager apparently want to start from scratch, with only the data themselves as starting points. Hence they compare the distribution of champion births over sectors with a uniform distribution, whereas Ertel argues that one should compare with the known population distribution. If one does so, both sectors 1 and 4 deviate by about the same amount from expectancy. As the Gauquelin hypothesis also treats sectors 1 and 4 on an equal footing, there is no rationale to select only sector 1, if one takes into account claims and explanations already published.

Is there any reason to suppose that sportsmen show birth rhythms that deviate from average people? Yes, there is. Several investigations, collected by Piet Jongbloet and reported by him in the Proceedings of the Third EuroSkeptics Congress (1992), show that there is an annual and a diurnal rhythm. Physically extremely fit people are born relatively more often in winter and summer and less often in spring and autumn. The relative birthrate can differ by up to 30% from the average population. Furthermore, the birth hours of first-borns differ from later-borns, mostly because labor lasts longer in the case of first-borns.

The effect of these rhythms can be simulated with data resembling the original Gauquelin data. The result was that the seasonal rhythm hardly has influence after averaging over a century. The daily rhythm has some influence, but it cannot explain the great number of athletes born in sector 1 in the original Gauquelin data. To get anywhere near the effect seen in the Gauquelin data, one must assume that the diurnal birth rate of future champions varies between 80% more and 80% less than the average birth rate. In fact the Gauquelin data show only a variation with an amplitude of about 20% around the mean.

So as a matter of fact, the idea of "spurious periodicities" did not work out. Of course, even if it had, it would have faced serious problems. It offers no explanation for similar claims for Jupiter or Saturn. Since Venus is always to be found near the Sun, it would have meant that there would have been some kind of Venus effect much stronger than even a Mars effect. Since the Mars Effect claim rests on the combined excess of births near rise and culmination of Mars, explaining the peak near Mars rise would at best have been only half the work.

De Jager and Jager worked closely together with C. Koppeschaar, who performed many simulations. Koppeschaar was able to reproduce more or less the Gauquelin Mars Effect by simulation. If the daily peak of births closely follows sunrise (late in winter and early in summer), and if births in French maternity homes show a drug-assisted afternoon peak and if this effect is stronger among sports champions than in the general population, then a Mars effect, including an excess near Mars culmination might arise.

That is many "ifs," and Koppeschaar met two problems. First, his initial data base (about 900 champions) was too small to check whether his assumptions on the birth times of champions really did hold. He was not able to obtain the full data of all champions and ordinary people (including exact time and place of birth). Secondly, he and De Jager and Jager got all data that Ertel had ob-

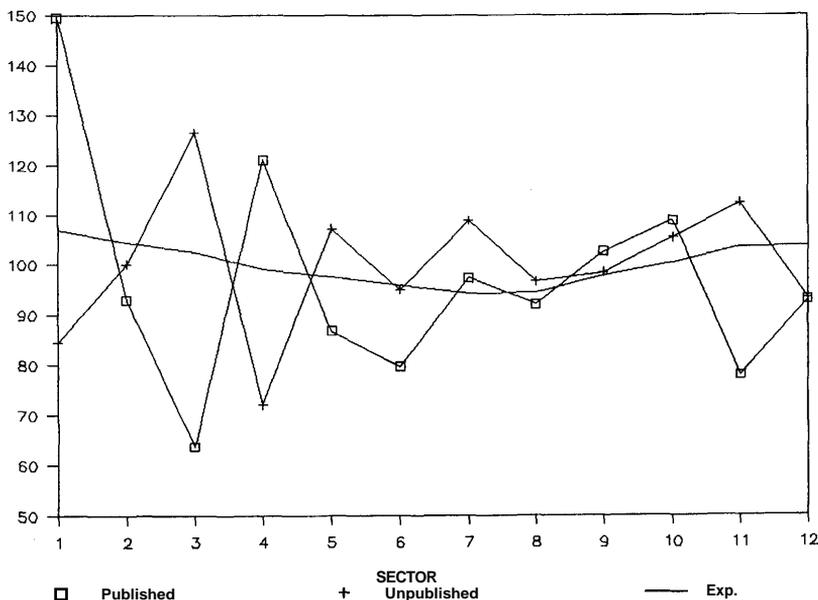


Fig. 1. The distribution of the French champions over the 12 Mars sectors. The unmarked curve shows the theoretical distribution, calculated on the basis of a very large number of "ordinary persons." Marked negative deviations in key sectors 1 and 4 are apparent for the unpublished data.

tained from the Gauquelins, and the closer they inspected these, the more they became convinced that the bias in these data would make all their efforts futile. So in the end Koppeschaar decided not to report on what was necessarily to remain a speculation.

Negative Deviations Among Unpublished Athletes

It is mentioned already that from a physicist's point of view the Gauquelin Mars Effect doesn't amount to much. Close inspection of the data provided by Ertel made the Mars Effect seem even less impressive. The data of sports champions that somehow were deemed not good enough and that were not published, showed deficits at the time of Mars rising or culminating.

This had already been observed by Ertel (1988). Maybe in an effort to remain unprejudiced, the Dutch skeptics examined Ertel's paper closely only in an advanced stage of their research.

Koppeschaar Finds No Eminence Effect

Ertel (1988) argues that nonetheless there is an eminence effect. Koppeschaar says that even this is dubious. If one looks at the French sports champions alone, there simply is no eminence effect, that is, a clear trend for higher citation classes to be born more often in characteristic Mars sectors. Koppeschaar has argued his view by displaying the data graphically, but it can be confirmed with statistical tests.

TABLE 1
Distributions of published French, unpublished French, published foreigners, unpublished foreigners, with corresponding numbers of those born in key sectors.

| Class | PFr | KS | UFr | KS | PFo | KS | UFo | KS |
|-------|------|-----|-----|----|------|-----|-----|-----|
| 1 | 460 | 106 | 176 | 18 | 871 | 175 | 764 | 127 |
| 2 | 398 | 88 | 400 | 55 | 259 | 50 | 43 | 6 |
| 3 | 310 | 71 | 63 | 11 | 152 | 38 | 10 | 0 |
| 4 | 119 | 25 | 40 | 4 | 91 | 21 | 3 | 0 |
| 5 | 34 | 8 | 4 | 1 | 62 | 12 | - | - |
| 6 | 27 | 4 | - | - | 51 | 14 | - | - |
| 7 | 8 | 4 | - | - | 25 | 7 | - | - |
| 8 | 1 | 0 | - | - | 17 | 4 | - | - |
| 9 | - | - | - | - | 3 | 1 | - | - |
| Total | 1357 | 306 | 683 | 89 | 1531 | 322 | 820 | 133 |

Ertel has subdivided the champions into different citation classes, depending on the number of sports directories in which the champions were mentioned. There were nine general directories and nine specialist (cycling, soccer, aviation, tennis, etc.) directories, and no athlete occurred in more than 8 of these. So there were in all nine citation classes: 1 (no citations) to 9 (8 citations).

In the following table the 4391 sports champions are subdivided into four groups: published French (PFr), unpublished French (UFr), published foreigners (PFo) and unpublished foreigners (UFo), each time followed by the total number of that class in the key sectors (KS) 1 and 4. For this computation the original key sectors of the Gauquelins have been used, not the extended ones of their later publications on heredity (M. & F. Gauquelin, 1970/171, 1972). The data are derived from Koppeschaar's paper in the EuroSkeptics III Proceedings (1992).

As one can see, it makes sense to distinguish the French from the foreigners. Among the 2040 French only 636 (31%) did not occur in any of the 18 reference books, whereas among the non-French 70% did not occur in Ertel's books.

Statistical Checks of Koppeschaar's Argument

After reading Koppeschaar's paper, I tried to support his conclusions by a more formal statistical argument. One might make two models for the eminence effect. In the first model the citation class is taken into account, but also the "published" variable (by the Gauquelins). In the second model the variable "published" is omitted. In formula form the models are

$$f/n = \alpha + \beta \text{CITCLASS} + \gamma \text{PUBL} \quad (1)$$

$$f/n = \alpha + \beta \text{CITCLASS} \quad (2)$$

In this model n stands for number of champions and f for the number that were born in one of the Mars key sectors. For example, in case of the French champions, $n = 398$, and $f = 88$ correspond (in the first model) to CITCLASS = 2 and PUBL = 1. The variable "PUBL" has only two values, 1 and 0.

Of course, the different groups of champions are not equally large. It would be not correct to count the fraction $88/398$ with the same weight as the fraction $0/11$ (for citation class 8). So the different data are given weights proportional to n ; in this way each individual champion gets the same weight. Then it is not necessary to fuse citation classes in order to avoid problems with small numbers.

Because such fractions can only have values between 0 and 1, it is better to take the arcsine of their root. If we examine both models for the French and the non-French we find the following.

The coefficient β does not differ significantly from 0 in the case of the French champions [model (1): $p = 0.90$, model (2): $p = 0.58$, two-sided], but γ is very clearly different from 0 in model (1). For the latter conclusion we hardly need a complicated statistical model, as a cursory inspection of Table 1 will show: 13.0% (standard deviation less than 1.5%) of the unpublished French were born in a key sector, compared to 23.5% of the published French.

Just to be on the safe side the "published" and "unpublished" French champions were also separately tested with model (2), the p -values were 0.77 and 0.38, and it should be noted that among the "published" the coefficient β is even negative.

The fact that such low numbers of unpublished French champions are reported to be born in key sectors argues against the hypothesis that something else besides an artifact is involved. These athletes may not represent the very top (even though three quarters of them were quoted at least once in one of these reference works), but they probably outperformed all but one in a thousand ordinary people engaged in their branch of sports.

In case of the non-French, matters are more complicated. In model (1) neither β nor γ differ significantly from 0 (p -values equal 0.33 and 0.055 respectively). Among the non-published non-French athletes (UFO in table 1), the Mars key sector percentage is equal to 17% for class 1, and is 11% for the remainder. If we ignore the difference between published and unpublished, in other words if we look at model (2), then β does differ significantly ($p = 0.0004$) from 0. Each extra citation increases a champion's chance of belonging to a key sector by 1.5%.

If the total collection of 4391 athletes is considered, then again the coefficient β in model (1) is nonsignificant ($p = 0.28$) and in model (2) we have borderline significance ($p = 0.033$). If we apply model (2) to the published and the unpublished athletes separately, β again does not differ significantly from 0 (p equaling 0.08 and 0.21 respectively; in the latter case β is negative again). (The above computations were performed by Dr. J. B. Dijkstra of the Computing Centre of the Eindhoven University of Technology.)

Altogether this presents a very weak support for the eminence effect: the models show it only when the difference between published and unpublished is ignored and when moreover the non-French are included. In other words, only when we assume that no bias is involved in Gauquelin's decision to publish may we say that there is some evidence for the eminence effect. In this connection it must be mentioned that one may not suppose that the citation frequency is entirely unrelated to a possible bias. Three of the reference works used by Ertel were obtained from Gauquelin.

Gauquelin's Procedures

The Gauquelins had from the outset on maintained that these planetary effects were observed with eminent professionals only. It is quite plausible that they tried to find a proficiency level beyond which the effect was clear. Initially they were quite liberal in their judgment about what constitutes eminence. They collected, for instance, 268 Italian aviators. Now given the total number of aviators (not exactly a sport for the millions), one can hardly maintain that they represent the very top of an extremely competitive branch of sports.

Bicycle racing is a much more strenuous and competitive sport, but all the same the 2087 champions of the Gauquelins of their 1970 publication contained 228 Belgian bicycle racers, which seems high for such a small country. Belgian schools are not in the habit of forming racing teams to compete with each other, quite unlike baseball, basketball and football in the U.S.A., which are almost like a national religion, or soccer in The Netherlands (the Dutch national soccer association counts about one in four young males as playing soccer in a recognized soccer club). So we also maintain that in the matter of bicycle racing the standards for what constitutes eminence were rather liberal.

But Gauquelin also went to the trouble of getting birth dates of 599 (or 600) Italian soccer players of the first division of the Italian football league (M. & F. Gauquelin, 1979:180; M. Gauquelin, 1988; Ertel, 1988). These constitute a smaller fraction of the total number of players than the aviators. Moreover, one doesn't get into the highest division without being selected several times for ability. So there was *apriori* good reason to regard these 599 soccer players as a good test of the hypothesis. Gauquelin reported that these 599 did not show the Mars Effect: about the predicted 17.2 percent were born in a key sector. Only when one took out the 98 soccer players that had achieved international status was a Mars effect found.

If one collects data, and if one determines what is a "good" athlete after knowing the data, then one is still in the exploratory phase. One should not be surprised if the aggregate result of this exploration shows deviations from chance expectation. It is a different matter when one puts one's ideas to the test. In that case it won't do to claim "if one takes care to select the very best, they will show an effect." The test should formulate criteria for eminence that are objective, not in the sense that personal judgment should play no role, but

in the sense that the choice of the criteria is demonstrably independent of knowledge of the Mars sectors of the people in the test.

This distinction between exploration and formal testing was not kept clear by the Gauquelins. From Ertel (1988) one can infer that prior to the Belgian test 203 of the 535 champions of that test were already in Gauquelin's files. The eminence criterion for soccer players in that test was twenty international games, not just at least one, as in the Italian case. The files of the Gauquelins even contained birth times of 76 Belgian soccer players that did not reach the twenty-games eminence criterion. Another 165 Belgian soccer players were in the files, but their birth dates had not been obtained. Did knowledge of the Mars sectors of those 203 sportsmen play any role in the choice of the criteria?

After the Belgian test was over, the Gauquelins lumped the results of a formal test together with their exploratory results. Moreover, after the American test they entered a detailed discussion about what constitutes a champion. On the basis of their results with French basketball players (there are only 33 in their 2087 champion list of 1970, of which 3 were born in key Mars sectors) they argued that basketball players should not have been taken from a source listing 1000 U.S. contemporary basketball players. Then they started listing various subsamples that favored their view: 31 "Notable Sports Personalities," 20 Olympic Gold medallists, 73 from two specific directories. They objected to the "first selection" of 128 containing 10 people born after 1950. Altogether they extracted 192 people from the 409 of the American test, and these 192 were found by Ertel in their files.

Now the overall Mars percentage in the American test was 13.5%, but in the material the Gauquelins extracted it was 17.9%, and they made it look as if the Americans deliberately had managed to select 217 anti-champions that collectively showed a Mars Effect of only 8.8%. Later the Gauquelins collected more American champions. It is quite surprising to find in their files birth dates of 19 (published) American champions born after 1950, of which 5 (26%) were born in a Mars key sector. Apparently the restriction that one should not consider champions born after 1950 was only valid some of the time.

If this is what they did after a formal test, then what were they prepared to do with the data they collected themselves in the exploratory phase?

In science the results of exploratory research are often discussed in terms more suitable for formal tests, for example by mentioning "significance." This is not correct. "Significance" only means something when the hypothesis demonstrably is framed independently from the data collected.

In practice independent replication (usually of similar but not exactly the same hypotheses) is warranted after exploratory findings. The planetary effect of the Gauquelins seems to be mainly results of the exploratory research phase and artifacts of the data treatment process. No meaning can be attached to the reported significance levels.

A Simulation

Anyone who doubts the effects of choosing the champion level can program the following simulation. Let "sportsmen," namely pairs of random numbers (a, m) come in batches of 25 each. The number a is a random number between 1 and 100, representing a one-dimensional "ability." The number m is a random number between 1 and 6, representing "Mars sector." Let about a dozen proficiency levels be given between ability 20 and 80. For each batch, choose a separate proficiency level from that dozen, namely the level that maximizes the "Mars effect" for that batch, or rather for the portion of the batch above that proficiency level. Almost half of each batch will be retained, on average, and in the aggregate of many such "champions," the "Mars effect" will be 4% or 5% percent more than the 16.7% expected without any setting of champion levels.

Of course there are differences between a simulation like this and what might have happened with the Gauquelin data. Gauquelin's batches varied in size, and many batches were larger. Moreover, in real life, athletes are not distributed uniformly over ability classes. On the other hand, there are many different ways to judge the abilities of sportsmen, not just a single number. Also, setting champion levels may not be the only exploratory artifact. If one selectively double-checks Mars sector computations of well known champions who are just outside of a key sector (but not those who are just inside), one may introduce a further bias. In this connection it must be remarked that Michel Gauquelin (1984) has reported results of computer recomputations of all his data. The total number of athletes in key sectors was decreased by 17, from 452 to 435. The present discussion relies on data received by Ertel from Michel Gauquelin, and these do not contain the results of this recomputation.

Notes

Mrs. Gauquelin has commented on the findings of the Dutch Skeptics (also in the same Proceedings). She finds many scientific and moral errors with them, but many of her remarks are not clearly related to the text of the papers she comments on. What is needed in the discussion about findings of the Gauquelins is more clarity about the choice of the criteria for championship. Maybe it is difficult to convincingly prove anything about that now. But the fact that one of the main investigators still does not grasp that this is a main issue, or rather does not spend a single word on it in a paper almost twice the length of this article, is telling. It supports the conjecture that the Gauquelins did not know that this mattered at all. How Michel would have reacted to this observation will remain unknown.

De Jager and Jager added a few remarks about Ertel's eminence effect in the proof stage of their contribution to the EuroSkeptics III Proceedings. These remarks confuse the 2088 athletes published in 1970 and 2888 athletes ever published. By an oversight Ertel was not informed about these extra remarks, and

therefore his comment in the same Proceedings that De Jager and Jager did not consider the role of eminence (intended as: "did not think relevant") seems now a mild euphemism for "did not bother to read my 1988 paper carefully."

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Comments on Dutch Investigations of the Gauquelin Mars Effect

SUITBERT ERTEL

Institut für Psychologie, Gosslersstraße 14, 3400 Goettingen, Germany

Abstract— The first of two Dutch skeptics' attempts at disproving the Mars effect failed (Nienhuys 1993a). Contrary to the Nienhuys view, however, the second pass of the Dutch critics at the Gauquelin claim— an attempt at undermining the validity of his data base— is shown to fail as well. The critics drew apparent support from my previous unearthing of a Gauquelin bias— Gauquelin had exempted cases from publication (Ertel, 1988). Yet they neglected the fact that any such bias had been neutralized as a result of my pooling of published and unpublished data. Specifically, a significant eminence trend was demonstrated in the unmanufactured total sample in my 1988 report. In addition, Dutch endeavours at rendering the eminence relationship insignificant either failed (even a less sensitive scale with 12 instead of 36 sector division yielded significance) or were illegitimate (splitting up of the entire sample for that purpose violates methodological logic). Thus, the present (fourth) attempt in the history of resistance against the Gauquelin challenge by organized skeptics has added two misses to their record.

Introduction

The Gauquelin Mars effect has aroused a good deal of opposition and corresponding attempts at empirical refutation by four skeptic committees. The first was the Belgian attempt, which failed (Comite Para, 1976). Another negative result was obtained in the first of two U.S. American efforts ("Zelen test," Zelen, Kurtz, & Abell, 1977). The results of the second U.S. effort (U.S. athletes test, Kurtz, Zelen, & Abell, 1979— 1980) did not unequivocally contradict Gauquelin's claim either (Ertel, 1992b). A recent French replication, based on new data (Benski, 1991) is now being studied— (Ertel, in preparation). The most recent attempts at refutation have been investigations by Dutch skeptics, published in The Proceedings of the Third Euroskeptics Congress (de Jager & Jager, 1992, Jongbloet, 1992, Koppeschaar, 1992, Nienhuys, 1992; rejoinders by Ertel, 1992a, and Schneider— Gauquelin, 1992), and accounted for by Dr. Nienhuys in the present issue of JSE (Nienhuys, 1993a), henceforth "N93."

Dutch Attempts I and II

The Dutch approach consisted of two attempts, the first of which searched for an alternate explanation of the Mars anomaly, an issue that was expounded

at the Euroskeptics Congress. It did not succeed as shown in my critique (1992a) (1) and as admitted by de Jager & Jager (1992) as well as by Nienhuys ("essentially their findings were negative," N93).

The second attempt consists of questioning the soundness of the Gauquelin data base and includes a public relations campaign (2): The Mars effect was turned into an effect of biased sampling, explicitly by de Jager: "this [our] finding killed the reality of the [Mars] effect" (personal communication, letter Dec 10, 1992); less explicitly by Nienhuys, who merely cast doubt on the data leaving conclusions to the reader: "the Gauquelin data suffer from bias." In what follows I will argue that attempt II of the Dutch refutation of the Mars effect also fails.

Attempt II of the Dutch investigations did not come up with new facts, since support for the negative conclusions was drawn from my JSE 1988 account of Gauquelin athletes data—specifically, my comparative data analysis of published vs. unpublished data, which provided evidence for a Gauquelin selection bias.(3). Nienhuys' interpretation of what I found and of additional Dutch observations, however, are not in line with my own, so the divergence demands scrutiny.

Pooling Data Repairs Doubtful Subdivisions

As early as 1955, Michel Gauquelin in his pioneering study on French professionals subdivided three of nine samples using the criterion of "eminence" (Gauquelin, 1955). Eminence in this case was operationally defined by lines devoted to individuals in biographical dictionaries (painters: high-medium-low eminence, p. 233), or by numbers of expert nominations (actors: high-low eminence, p.234; politicians: high-low eminence, p. 235). More pronounced planetary effects were reported for the more eminent as compared to less well-known subsamples. In his classic replication study of 1960 on other European samples (Italian, German, Belgian, Dutch), Gauquelin allegedly found support for his eminence claim for all professional groups ($N = 13$, Gauquelin, 1960, p.160ff). Operational definitions of eminence, however, had not improved—on the contrary, subdivisions of samples had largely been based on sloppy criteria. Among athletes, for example, Gauquelin considered all Italian football players ($N = 599$) and all German athletes ($N = 118$) except 8 (pp. 85, 89) as not reaching the criterion (more instances are given in N93). In one major publication of sports champions data (Gauquelin & Gauquelin, 1970) mediocre figures were not entered. This gave sufficient grounds for my traveling to Paris in 1986 with the intent to search in the Gauquelin archive for missing data. I came up with a great number of unpublished athletes' birth dates ($N = 1,503$), see detailed description in Ertel (1988).

No doubt, Gauquelin's repeated eminence observations had entitled him to select for publication successful samples and to discard mediocre ones. Selections, however, should have been done with utmost objectivity as well as replicability. In this regard Gauquelin, generally a model of painstaking

TABLE 1
Citation percentages for published and unpublished Gauquelin athletes.

| Citation frequencies | Published (N = 2,888) | Unpublished (N = 1,503) | Difference |
|----------------------|--------------------------|----------------------------|------------|
| 0 | 46.1 | 62.5 | 16.4 |
| 1 | -22.8 | 29.4 | 10.5 |
| 2 | 16.0 | 4.9 | -11.1 |
| 3 | 7.3 | 2.9 | -4.4 |
| 4 | 3.3 | 0.2 | -3.1 |
| 5 | 2.7 | 0.0 | -2.7 |
| 6 | 1.1 | 0.0 | -1.1 |
| 7 | 0.6 | 0.0 | -0.6 |
| 8 | 0.1 | 0.0 | -0.14 |
| | 100.0 | 100.0 | |

methodological care, had violated norms. I conclude that this had occurred inadvertently rather than on purpose since Gauquelin facilitated my search for unpublished data without reservation.

My pooling of all athletes data and my subjecting them to citation counts repaired past sampling flaws. Thus, a subsequent reanalysis of the entire data pool showed first that birth dates not published by Gauquelin were in fact predominantly those of least-cited sports people (see Table 1). Secondly, I also found that Gauquelin's discarding of athletes, in a subsample of $N=659$, had been affected by his being aware of Mars sector positions. Any such mismanagement of data, however, including biased selection, was now undone by pooling the data. Pristine conditions, as it were, were thus restored for testing afresh Gauquelin's contention. The eminence trend as shown in Figure 3 of my 1988 paper (p.72), was the result of an unhazarded new approach.

Refutation of Nienhuys' Arguments

The Dutch critique failed because it neglected the improved methodological ground work just described. The first of Nienhuys' arguments is merely a paraphrase of what I had already published and is thus redundant.

Nienhuys' second argument addressing my eminence result ("Koppeschaar finds no eminence effect") deserves three explanatory comments. First, in Figure 9 of his critique Koppeschaar presented Mars key sector proportions of the total of 4,391 athletes, separately for 9 citation ranks (4). His conclusion regarding trend ("no significantly increasing eminence effect is evident"), however, was based on visual inspection only, which is deceptive. That is, Kendall's tau-statistic applied to his numbers reveals a significant eminence trend ($p = .025$). I provided Koppeschaar the result of my calculation and offered my trend program for him to check this, but he did not make use of this offer.

Second, Koppeschaar's calculations utilized Gauquelin's former 12 sector division (two of 12 sectors are defined as key sectors, i.e., sectors 1 and 4 on that scale) instead of the now imperative 36 sector division (8 of 36 sectors are defined as key sectors: nos. 36, 1, 2, 3, 9, 10, 11, 12, on that scale). The 36 sector definition applies to all professions (not only athletes) and to all planets (not only Mars), i.e., it does not just favor the present study. The Dutch skeptics' indifference to scale requirements (Nienhuys excludes this issue as well) despite my pointing the problem out in personal correspondence (Koppeschaar — Ertel), should be kept in mind. The error probability of an eminence trend for athletes' Mars sector proportions, when based on 36 sector precision, is raised from $p = .025$ (12 sectors) to $p < 0.005$ (36 sectors).

Thirdly, Koppeschaar's and Nienhuys' subdividing of the total sample into 4 (or whatever) subsamples and then subjecting them to separate significance tests is not legitimate. This Machiavellian "divide and rule" strategy (i.e., breaking the sample down to get rid of degrees of freedom, thus slipping away from statistical confirmation) had its predecessors in the Zelen test debate and was criticized (Tarkington, 1980, p.19, Curry, 1982, p.37) (6). It is well known that dividing a total sample up into subsamples may be useful for testing the homogeneity of that sample. Thus, the key sector (KS) percentage of, say, a French subsample may be compared, using a Chi-square test, with KS percentage of the rest (non-French) of the total sample. If the French sample's KS percentage level differs significantly from the level of the non-French total a prematurely generalized hypothesis (the Mars effect applies for all nationalities) might possibly be challenged(7). But the Dutch strategy was different. Subsamples were not compared with the total sample, but were instead subjected to separate significance tests (8).

Nienhuys' third point ("Statistical checks of Koppeschaar's argument") was based on a trend test proposed by Nienhuys and tested by Dijkstra. This test served as an alternative to more familiar published procedures. It was applied, as it should be (assuming the details are correct), to the total of $N = 4,391$ athletes. It ought not to have been applied, however, as was shown above, to 12-sector data. Nevertheless, despite the exclusion of 25 percent of the pertinent information, the trend test proved significant ($p = 0.033$) which is close to the $p = 0.025$ level as obtained for 12-sector data by Kendall's tau (see above). But Nienhuys' main argument does not bear on this result at all, dwelling instead on nonsignificant eminence results for separate subsamples, above all for the French.

Nienhuys and Dijkstra's trend test was also applied separately on published and unpublished subsamples, but that approach was unjustified. Gauquelin had separated (and not published), for good reasons, mediocre sports people from more successful ones (see Table 1 above). Separate correlations with eminence for published and unpublished samples, as performed by Nienhuys and Dijkstra are thus bound to decline. As an illustrative analogy consider that the correlation of body size between husbands and wives in a population must decline if done separately for samples of tall and short husbands.

Nienhuys' fourth argument with its minute account of Gauquelin's selecting successful samples does unquestionably point out some flaws, but flaws which I had suspected in 1986 and which had been corrected in 1988 by my pooling of all available data.

Nienhuys' last argument is his simulation. However this caricature of how deliberate and biased selections of good achievers might proceed does not add any new aspect to the question the Dutch desired to settle by negation: "Is a Mars effect present in the Gauquelin athletes' data?" De Jager apparently did not go into the matter deeply enough, even mixing up a crucial sampling division (cf. Nienhuys' endnote), thus missing entirely the logic of my 1988 eminence result. For him as an astronomer the "Mars effect" issue apparently did not deserve serious consideration (9). It is more difficult to excuse Nienhuys' for neglecting the main issue. An extensive correspondence about the Mars effect on two e-mail forums preceded the present publication (10).

The Crucial Issue

Only Koppeschaar, after having been urged in my letters not to neglect in his paper the crucial point of my 1988 article, commented on the essence of what I had conveyed. He quoted from my paper:

"The presence of selection bias, therefore, did not weaken the conclusion that Mars' positions and the athletes' births are statistically related. Correction for selection bias by pooling all records increased empirical support for the stronger version of this claim [= for the eminence claim]: The data have overcome, in spite of disturbing effects of bias, the higher methodological hurdle" (Koppeschaar, p.183).

Koppeschaar, however, took issue with this contention: "I do not agree with this conclusion. How can any trend analysis be based on selection-biased data?" My reply: Selection bias had been removed by pooling published and unpublished data. Any effect of previous subdivisions had thus been rendered ineffective.

Koppeschaar continued: "Especially if one closely inspects the absence of higher ranks of eminence for the unpublished data, it is immediately clear that a trend analysis for the total sample is greatly influenced by the outcome of the already published sample."

My comment on that: Koppeschaar seems to have noticed eventually, first, that Gauquelin's unpublished data are associated with lower eminence ranks as compared to his published data; second, that Mars key sector percentages are lower for unpublished than for published data; and third, that pooling both samples should therefore give rise to an upwards eminence slope just as pooling tall and short husbands should increase body-size correlations with their wives above the level of separate correlations.

Koppeschaar, however, distrusts this compelling logic. In view of the astounding difficulty my Dutch opponents have met with at grasping the core of my message of 1988 it may be helpful to repeat that the eminence slope of ath-

letes ($N = 4,391$) is unrelated to the ways Gauquelin managed or mismanaged previous data subdivisions. That is, the eminence vs. Mars-births relationship would have taken exactly the same shape even if entirely different decisions had been made by Gauquelin regarding which data were or were not to be published (11).

Future Perspectives

Two great advantages are associated with eminence trend assessment in the Gauquelin research: First, such trends are independent of key sector proportions. In other words, significant trends can exist and thus reflect planetary connections even when the average key sector level across eminence ranks is low and thus lacks such a connection. Second, trend tests do not require expectancy calculations whose rationale, as the Comite Para demonstrated, might remain a matter of debate. That is, the trend test approach to the Mars effect hypotheses circumvents one of the greatest past obstacles impeding agreement among researchers. The present debate has cast light upon improved conditions for joint future endeavours in this field.

Postscript

Seven brief remarks on Dr. Nienhuys' comments (Nienhuys, 1993a) on my Munich paper (Ertel, 1993):

- (1) Dr. Nienhuys' reanalysis of my eminence calculations confirmed the trend i.e., Mars key sector births increase with increasing eminence as shown in his Figures 1 and 3. The Mars effect itself is thus confirmed.
- (2) Nienhuys, however, says that when citations from three sources are deleted, this trend disappears, as shown in his Figure 2. The three sources deleted here are important in that they are among the few covering most sports fields; they exceed others in regard to eminence information as becomes evident in Nienhuys' Figure 3.
- (3) The unexplained "modus operandi of the Gauquelin bias" (Nienhuys) seems to imply that the mere fact that these books were borrowed from Gauquelin introduced bias. The critic does not indicate how bias might have worked here and did not provide any reasonable answer to my explicit question in personal correspondence.
- (4) In order to bring the trend curve down in Figure 2 Nienhuys not only deleted citation sources, he also switched to the less sensitive of the two sector definitions (2/12 instead of 8/36 sectors). He knew from Koppeschaar that by switching to the 2/12 key sector definition the eminence trend would decline. Nevertheless he combined, without informing the reader explicitly, both trend-lowering contributions in his Figure 2, whose difference from Figure 1 is therefore ambiguous.
- (5) Nienhuys refers to my discussion of eminence relationships as an "exercise" in arbitrary curve-fitting. No justification is given for this and my requests in subsequent correspondence to point at specific errors or statistically illegitimate decisions were not answered.

- (6) Nienhuys' conjectures in his last paragraph became comprehensible only after having requested and received from him additional explanations. His intent was to explain downward key sector trends by selection bias. On noticing that his bias idea is not supported by the data (and not noticing that his idea is contradicted by the observed upwards trends anyway) he says: "Therefore I think that selection by ... [that fancy bias] cannot be the only source of the Gauquelin bias, other methodological errors may also be responsible" (p. 159). With this twist his whimsical construction is maintained despite counter evidence; the error he himself committed in inventing it is converted into a Gauquelin error, and the explanation for downwards and upwards eminence trends which he failed to provide is expected from more errors of such kind in the future.
- (7) Dr. Nienhuys said at the beginning: "It is not easy to comment on a report in which no data are presented that permit any form of numerical analysis." The reader is not told (respective acknowledgments are missing) that I provided all the data that he used for his Comments and that he did not ask for what he might have wanted in addition.

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Footnotes

1. My critique was based on new analyses of Gauquelin data that put the Dutch claims to the test. Mme. Schneider-Gauquelin (1992) drew attention to already available counter-evidence from past Gauquelin research.
2. "Campaign" alludes here to the Dutch authors' seeking publicity for their first approach by informing the national newspaper "Volkskrant" which headlined a corresponding article "The last citadel of astrologers is crashing" (Schilling, G., 1991). De Jager, Jongbloet, and Koppeschaar proclaimed here in public successful demolition of Gauquelin's basic claim.
3. I drew the skeptics' attention to my 1988 paper. This occurred in my letter to Koppeschaar, Jan. 30, 1992, who apparently had not taken notice of that publication. De Jager was notified correspondingly by members of his group.
4. Koppeschaar refrained from putting citation frequencies 6-9 into one rank =>6 (as I had done in 1988) since rank 8 (N= 18) and rank 9 (N = 3) did not continue the slope displayed by ranks 1 through 7. For him this was an optical advantage.
5. A copy of my trend calculation was posted to Nienhuys as well, but his second argument shows he also preferred to disregard my statistical correction of Koppeschaar's visual analysis.
6. A quote from Tarkington (1981, p.19), on the Zelen test: "Zelen et al. repeatedly broke the total sample down into samples too small to reach statistical significance. No ordinary statistical case, however strong, can survive splitting of the data into small parts. It is a fundamental concept of statistics that a small sample is much more variable than a large one and much less reliable. We can be sure that small samples will show us nothing significant in the way of proof."
7. Challenged by the Dutch skeptics' opposition I checked the internal consistency of the database using χ^2 . None of the six nationalities (French, German, Italian, Belgian, Dutch, US) deviates significantly regarding Mars key sector percentage from the respective total (see Table).

The Gauquelin athletes total sample (N = 4,391) broken down regarding nationality, key sector frequencies (KS) and significance levels for deviation from total based on Chi-square (nKS = non-key sector frequencies).

| Nation | FRA | ITA | BEL | GER | NET | USA |
|----------|------|------|------|------|------|------|
| Total | 2040 | 1328 | 399 | 154 | 60 | 351 |
| KS | 534 | 333 | 99 | 37 | 16 | 91 |
| nKS | 1506 | 995 | 117 | 300 | 44 | 260 |
| KS % | 26.2 | 25.1 | 24.8 | 24.0 | 26.7 | 25.9 |
| χ^2 | .52 | .35 | .17 | .23 | .03 | .01 |
| df | 1 | 1 | 1 | 1 | 1 | 1 |
| P | n.s. | n.s. | n.s. | n.s. | n.s. | n.s. |

8. Koppeschaar correctly noticed a discrepancy regarding citation frequencies (should not be mixed up with key sector percentages) between French and non-French athletes. I was aware of this shortcoming: I had utilized the "Dictionnaire des Sports" as citation source even though this book had also served Gauquelin as retrieval source from which he had drawn a great number of cases. The reason for my decision was that biographical books such as the Dictionnaire covering many sports fields and nationalities are very rare. The gain of information provided by Dictionnaire citations regarding non-French nationalities (non-French in that book are predominantly famous) was associated with a loss of discrimination among French athletes (the French majority in this book is less famous). I might have selected from the Dictionnaire non-French citations only. But fear of crude critique by inconsiderate skeptics for "manipulating" assessment of eminence warned me to avoid more complicated decisions.
9. I quote from de Jager's letter to me, Dec. 10, 1992: "... we wrote our paper and realized this was the moment to stop searching for phantoms and to return to more promising research, which we actually did. Rieks [Jager, coauthor] returned to his X-ray sources and I to my unstable supergiants." In de Jager's view, as I see it, planets in the solar system may not compare regarding size with supergiants in distant galaxies. But if Gauquelin effects are real then planets, regarding human affairs, might prove incomparably more important.
10. The record will be provided on text file upon request. Almost all of it is also retrievable from files SKEPTIC LOG9110A-E LOG9211A-E by listserv at YORKVM1.
11. Prof. de Jager in a letter to me (Dec. 10, 1992) wrote: "I think you made a methodological error by throwing Gauquelin's initial data set (which is data set A in the above terminology) and the 2000 additional ones (B)

into one basket. That spoils both data sets, and does not allow any conclusion." De Jager does not explain why for him throwing samples A (published) and B (unpublished) in one basket would "spoil both data sets." I cannot find any argument supporting this conclusion. For me the opposite conclusion is evident: "Throwing both data sets in one basket" repaired Gauquelin's past faults. In my reply to de Jager's letter I asked for the missing logical premises of his conclusion, but I received no answer to that question. Providing missing premises needed for understanding conclusions (de Jager) may be more difficult than providing missing data (Gauquelin) needed for empirical evidence.

What Are Subtle Energies?

WILLIAM A. TILLER

Department of Materials Science & Eng., Stanford University, Stanford, CA 94305-2205

Abstract—A brief discussion is given of a set of anomalous experimental phenomena that are inexplicable based only on the four accepted forces operating in the physical universe. Possible explanations require defining the existence of subtle energies. Using a quantum mechanical description, the seat of subtle energy functioning is traced to the vacuum state with magnetic vector potential assuming the role of bridge between the subtle energies and physical energies. A brief discussion is given of how we might reliably detect subtle energies and a zeroth order model of the subtle domains as substructure for the vacuum state is given.

Introduction

To date, in conventional science, we have discovered four forces operating in the universe via which we try to explain all the observable phenomena of the universe. These are the strong nuclear force, the weak nuclear force, the electromagnetic force and the gravitational force. Of course, the energy associated with any one of these four forces is just the integral of the particular force with distance. However, for many decades, a growing body of experimental data has appeared that seems inexplicable based upon consideration of only these four forces. Some brief listing of a fragment of such data is given in the next section and these data seem to require the presence and action of what we shall call "subtle" energies for a satisfactory explanation. Subtle energies, then, are all the energies needed to explain this class of phenomena beyond the four fundamental energies that we already know and accept.

In what is to follow, the next section deals with a partial and brief recounting of experimental data whose explanation seems to fall beyond the scope of our accepted "world view." No attempt is made to list all the available data. The following section reviews the origin of matter and antimatter in this world view in order to reveal the required origin of subtle energies without indicating specifics or characteristics of such energies. The next section deals with how we might reliably measure such energies and the last section presents an overly simplistic model for viewing hierarchies in the subtle energy spectrum.

Anomalous Phenomena Needing A Subtle Energy Rationale

Up to this decade, science and medicine have considered living organisms as operating by means of the following sequence of reactions:

$$\text{Function} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Structure} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Chemistry} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Electric \& Magnetic Fields} \quad (1)$$

Whenever an organism wasn't functioning properly, the procedure was to blame structural-type defects in the system arising out of chemical imbalances. Recently, we have found that small electric currents between specific points in the brain give rise to the same behavioral changes that are observed with certain specific brain-stimulating chemicals (Woolridge, 1963). (Becker & Murray, 1970) and (Becker & Selden, 1985) and others have found that small D.C. electric currents can stimulate cells to regenerate, tissue to repair itself and fractures to heal faster.

An obvious defect in Eq. 1 is that it takes no account of mental effects. Under hypnosis, the human body has exhibited truly remarkable feats of strength and endurance attesting to an unconscious mind/structure link. In Aikido, Zen or Yoga disciplines, we see a conscious link between mental discipline and body function. On another front, modern psychotherapy shows us that certain chemical treatments induce mental states and certain mental treatments influence chemical states (Hawkins & Pauling, 1973.) Biofeedback techniques (Brown, 1975; Green, Elmer & Alyce, 1977) show us that directed mind can not only control various autonomic body functions like skin temperature, pain, etc., but also repair the body. Further, in a recent publication (Tiller, 1990), this author described a gas discharge device that responds to an energy emission from the body that is directed by mental intention. Thus, in this case, we see mind indirectly and remotely interacting with electron avalanches in a Townsend regime gas discharge. There seems to be little doubt then that mind, as a subtle energy, must be included in our equation so that it should read

$$\text{Function} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Structure} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Chemistry} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Electric \& Magnetic Fields} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Mind} \quad (2)$$

Experiments on remote viewing (Swann, 1975; Targ & Puthoff, 1977) have shown that people can (a) perceive and accurately describe objects placed several miles away from them, (b) be given the longitude and latitude coordinates of a location on the Earth and accurately describe the terrain of that location even though it is thousands of miles away and (c) tune in to a specific individual and view a remote locality through that individual's eyes. The remote viewer may even perceive the scene before the target individual gets there. Thus, the future time coordinate as well as the remote distance coordinates can be accessed using these techniques, possibilities not allowable via the presently accepted scientific paradigm. It is important to note that extensive, independent replication of remote viewing results has been documented (Targ & Harary, 1984). Here, we see applied mental activity producing results that seri-

ously bring into question our understanding of space-time and thus must involve some subtle energy linkage.

Motoyama placed a person who showed marked psychic ability and a second, ordinary person, in separate and remotely located rooms that were shielded by concrete walls lined with lead. The gifted person concentrated his mind on the other person while Motoyama monitored the subject's bodily functions (Motoyama, 1991). He found remarkable correlated changes in the subject's pulse, blood flow and respiration rate during the concentration period of the psychic individual. Since the two rooms appeared to be well shielded against physical energy passage, the cause/effect relationship required the involvement of one or more subtle energies. Here, we see not only a remote viewing aspect but also a psychokinetic (PK) aspect.

Years ago, Forwald (Forwald, 1969; Forwald, 1977) carried out - 15,000 PK experiments over a 15 to 20 year period on small wooden cubes. He showed a mathematical relationship between his ability to mentally influence the cubes and both the thickness and the chemical (nuclear) nature of the films. The Uri Geller-inspired PK work with children (Taylor, 1975; Hasted, 1981) and all the California "spoon bending" parties attest to remarkable action at a distance type phenomena. The more recent careful studies in the micro-PK area by Jahn and coworkers (Jahn, 1981; Jahn & Dunne, 1987) provides a substantial quantitative base to link the subject's mental intention and distantly applied force. Perhaps an analogous but more therapeutic remote influence falls under the category of healing and healers (Krieger, 1979; St. Clair, 1974, Krippner & Welch, 1992) with the process involving not only the mental domain but the emotional and spiritual domains as well. A recent study by Green, et al. (1991) of sensitives in a specially designed copper wall environment found that, for energy projectors (healers and Chi Gong Masters), anomalously large voltage pulses appeared in the body potential records whereas, for information perceivers, no such pulses were observed. Instead of the usual 10-50 mV baseline with - 1 mV ripple, the body potential often plunged to -30 to -300V and then recovered to baseline in 1 to 30 sec. This is an astoundingly large voltage pulse! In a single 90 min. healing session that took place in this special environment, one particular healer manifested sixteen anomalous bursts with each main burst being composed of 5-6 sub-pulses convolved in one envelope (Tiller, Green and Parks, 1993). It is likely that these voltage pulses are an electromagnetic correlate of the healing process taking place at subtle energy levels.

Karagulla (1967) has used clairvoyants to observe the "auric" fields around patients and thence describe their state of physical and mental health. These auric fields appear to the clairvoyant as patterns of light of different colours extending out from the body. Some clairvoyants can perceive the patterns with their eyes closed or in total darkness. Thus, these patterns are not composed from physical light and thus cannot be photographed with present day techniques. Pierrakos (1990), a psychiatrist who is also a clairvoyant, makes great use of his sensitivity in his practice. The auric field appears to be one or more

near-field radiation patterns from subtle levels of substance associated with the body much like the mathematically imaginary part of the electromagnetic radiation pattern from a physical antenna system. The detection ability, resident in some individuals, probably arises because of the advanced evolutionary state of a subtle sensory system in their body.

As a final example of a subtle energy phenomenon, let us select homeopathic remedies. The medical practice of homeopathy was greatly practiced in the last century while allopathic medicine is predominately practiced in this century. The point of singular interest concerning a homeopathic remedy is that its potency becomes stronger as it is progressively diluted with water and succeeded during the staged dilution (Vithoulkas, 1980). A high potency, on average, has less than one molecule of the physical herb or tincture per cc of homeopathic solution. Such behavior cannot be explained using our conventional scientific paradigm and demands an explanation at the subtle energy level.

As a conclusion to this section, experimental data has been referenced to indicate that Eq. 2 should be replaced by

$$\text{Function} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Structure} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Chemistry} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Electric \& Magnetic Fields} \begin{array}{c} \rightarrow \\ \leftarrow \end{array} \text{Subtle Energies} \quad (3)$$

where subtle refers to spiritual, mental, emotional, etc.

By subtle fields or energies, here, I do not mean weak fields or energies. To paraphrase Einstein, "Subtle is the Lord" does not refer to the Lord being weak. In most present day human experience, the subtle fields are normally only very weakly coupled to our physical fields so we generally see only small effects. However, under a favorable set of circumstances, they can be strongly coupled and then massive effects are possible. I suspect that our future technology in this area will reveal latent energy content and utilization of such subtle fields that are many orders of magnitude larger than that due to any of our presently known physical fields.

The Seat of Such Anomalous Phenomena as Viewed From The Conventional Paradigm

Quantum mechanics, which has only an empirical foundation, deals with the interaction between matter and everything else in the universe. At its simplest level, this means the interaction between matter and the vacuum state. In modern treatments, the vacuum is defined as the lowest energy state of the system whose equations obey wave mechanics and special relativity (it also has zero 4-momentum). The Dirac Equation, although Lorentz-invariant gives not only the energy states of particles but also predicts the existence of particles with negative energy (Aitchison & Hey, 1982). These negative energy states are unphysical and therefore unobservable with present day instrumentation. Nevertheless, by stimulating the negative energy states with sufficient energy, particles may be kicked into positive energy states and become real. The holes

left behind are the antiparticles. Thus, we can imagine that we live in a sea of virtual (unobservable) particles— the Dirac Sea. Since all physical observations represent finite fluctuations in energy and charge with respect to the vacuum state, this leads to an acceptable theory. To date, antiparticles have been found experimentally for all the particles known to physics.

Although hundreds of papers have been published in the past 30-40 years on considerations concerning the zero point vacuum state (Boyer, 1984; Milonni, 1980; Puthoff, 1988; Misner, Thorne & Wheeler, 1970; Wheeler, 1962; Bohm & Hiley, 1975), most physicists, chemists, biologists and engineers have little knowledge of the nature of this ground state for matter. Essentially, today the vacuum is seen as a chaotic sea of boundless energy (energy density equivalent $\approx 10^{94}$ grams/cm³ (Misner, Thorne & Wheeler, 1970) at the quantum relativity level with incredibly large destructive interference of wave functions; i.e., it is the domain where virtual particles and antiparticles are found. An interaction exists between this chaotic virtual particle sea and physical matter. It is this fundamental interaction that determines the ground state energies of all the atoms and thus all the molecules and all the condensed matter present in the universe. For example, if we could somehow alter this fundamental interaction, we should be able to change the ground state electrochemical potentials of reacting molecules in the cells of our bodies thus altering the body's state of function. In such a case, we may be able to observe the resultant physical changes but not the originating changes taking place in the Dirac Sea because our present instrumentation is inadequate for the task.

Lee (1982) points out that, in general, we may expect the vacuum to be as complex as any spin-0 field (boson field) of magnitude $\phi(x)$ at the zero 4-momentum limit. And, like a spin-0 field, it is conceivable that the vacuum state may carry quantum numbers such as isospin I, parity P, strangeness, etc. Lee (1982) shows that there is some justification for the idea that the properties of the vacuum can be altered physically by considering missing symmetry. If one adds up the symmetry quantum numbers of all matter, one finds these numbers to be constantly changing. However, if we also include the vacuum, then perhaps symmetry can be restored; i.e.,

$$\frac{d}{dt} \left\{ \begin{array}{c} \rightarrow \\ S \\ P \\ C \\ CP \\ \vdots \\ \vdots \\ \vdots \end{array} \right\} \neq 0 \qquad \frac{d}{dt} \left\{ \begin{array}{c} \rightarrow \\ S \\ P \\ C \\ CP \\ \vdots \\ \vdots \\ \vdots \end{array} \right\} = 0$$

Matter
Matter + Vacuum

Perhaps in symmetry breaking, the vacuum expectation value is not zero

$$\langle \emptyset | \text{VAC} \rangle \equiv \langle \text{VAC} | \emptyset | \text{VAC} \rangle \neq 0 \quad (5)$$

If Eq. 5 is correct then, under suitable conditions, we must be able to produce excitations, or domain structures, in the vacuum having a volume much larger than microscopic dimensions.

To perform such a feat, it is necessary to calm the chaotic Dirac Sea so that less destructive and more constructive interference exists between the wave functions of this virtual level of reality. Such a task essentially involves controlling the phase of quantum mechanical wave functions. Since the latent energy potential of one cm^3 of vacuum is so huge, it may not be too difficult to "tilt" the situation a little.

To date, the one field present in our armory of fields that has been determined to control the phase of the quantum potential is \bar{A} , the magnetic vector potential. In 1959, Aharonov and Bohm (Aharonov, 1959) (AB) pointed out that it is potentials and not fields that appear in the equations of quantum mechanics. They predicted that the magnetic vector potential \bar{A} , exists as a field with physically measurable attributes even in the absence of other electromagnetic fields. For a long time, we have utilized the mathematical relationships

$$\Delta \bar{E} = -\frac{\partial \bar{A}}{\partial t} ; \quad \Delta \bar{B} = \text{CURL } \bar{A} \quad (6a)$$

where \bar{E} is the electric field, t is time and B is the magnetic field. Of course, people usually like to write Eq. 6a in the form

$$\bar{E} = -\nabla\emptyset - \frac{\partial \bar{A}}{\partial t} ; \quad \bar{B} = \text{CURL } \bar{A} \quad (6b)$$

where \emptyset is the scalar potential resulting from the electric charge distribution. In electrical engineering (Kraus & Carver, 1973), we also define \bar{A} as

$$\bar{A} = \frac{\mu_0}{4\pi} \int \frac{\bar{J}}{r} dv \quad (6c)$$

where the integral is over the volume, v , of magnetic permeability, μ_0 , through which the electric current, \bar{J} , passes. Before AB (Aharonov & Bohm, 1959), people felt that \bar{A} was just a mathematical invention, convenient for solving Maxwell's Equations of Electromagnetism and it had no physical reality.

To prove their prediction, AB proposed that, if a beam of electrons were forced to travel in the neighbourhood of a long solenoidal coil so that \bar{B} exists inside the coil but not outside (\bar{B} falls off as r^{-2} while A falls off as r^{-1} outside

the solenoid) (Kraus & Carver, 1973), the phase of the electron wave function would change because A was not zero but was finite. They predicted the results of a two-slit interference experiment for electrons. Later experiments in Japan by Tonomoura *et al* (1982, 1983, 1986) completely confirmed AB's predictions. Since then, many other experiments of a confirmatory nature have shown that the \bar{A} effect applies to bulk materials as well as to simple quantum systems and that it is a type of "many-body" potential.

An electron wave function, χ , is given by

$$\chi = \chi_0 \exp\left[\frac{ie}{\hbar c} \int \bar{A} d\bar{s}\right] \tag{7}$$

where $d\bar{s}$ is the electron path vector, e is the electron charge, c is the velocity of light and $\hbar = h/2\pi$ where h is Plank's constant. Wu and Yang (Wu & Yang, 1975) observed that, in the AB experiment, it is the phase factor in Eq. 7,

$$\exp\left[\frac{ie}{\hbar c} \int \bar{A} d\bar{s}\right], \text{ that is physically meaningful. Two electrons interfering}$$

result in

$$\chi\chi^* = |\chi_1|^2 + |\chi_2|^2 + 2|\chi_1||\chi_2| \exp\left[\frac{ie}{\hbar c} \left\{ \int \bar{A}_1 d\bar{s}_1, - \int \bar{A}_2 d\bar{s}_2 \right\}\right] \tag{8}$$

Here, the effective phase difference $\left\{ \int \bar{A}_1 d\bar{s}_1, - \int \bar{A}_2 d\bar{s}_2 \right\}$ may be observed directly in an interference experiment.

The logic chain is thus that (1) if we can generate an appropriate A -field, the quantum phase of a set of virtual particles in a certain spatial domain of the vacuum state can be adjusted to enhance the coherence of that domain of space and (2) if a soup of chemicals is placed in that spatial domain, the ground state electrochemical potentials of the molecules in that soup will be appropriately altered such that the normal chemical reactions are changed and new reaction possibilities occur. Such an anomalous result is not inconceivable based on the concepts discussed.

If we go a step further and postulate that the magnetic vector potential, \bar{A} , is the bridging potential between the subtle energy domains and the physical domain, then Eq. 6b may be thought of as only one term in a matrix of terms defining \bar{A} . Now, subtle domain influences are able to modify \bar{A} which then, in turn, can modify \bar{E} , \bar{B} and the quantum phase of real particles or virtual particles in the vacuum state. These changes can generate a variety of phenomena that we would all term "anomalous." As illustrations, (1) to rationalize the anomalous gas discharge effect of the previous section, if human intentionality activates a mental energy (subtle energy) that produces pulses of A emission from the body, then this could temporarily increase the \bar{E} field in the detector

via Eq. 6a so that the micro avalanche size in the detector would increase and counts appear in the recording system as observed; (2) to rationalize the anomalous voltage surges in healers, if the intentionality/subtle energy chain involved in the healing process produces, as a biproduct, pulses of \bar{A} from some body organ or system then the \bar{E} -field produced by Eq. 6a would act on the local electrolyte to produce growing followed by collapsing charge separation and thus generate the type of large voltage pulses observed in the Menninger Clinic experiments and (3) to rationalize remote glowing of fluorescent tubes or small psychokinetic events, once again the intentionality/subtle energy/ \bar{A} pulse chain radiated from the body via some type of natural antenna system could, via Eq. 6a, develop a significant magnitude of high frequency \bar{E} and \bar{B} pulses so as to allow these phenomena to occur via electromagnetic means.

It is well known that, when A is generated via a flux of current, \bar{J} , in some localized domain of physical space (see Eq. 6c), \bar{A} , \bar{E} and \bar{B} fall off strongly with distance. Thus, the suggestion that the anomalous phenomena mentioned here are the result of electromagnetic interactions between humans and their surroundings is not tenable because the available experimental data indicates that the key fields leading to these anomalous phenomena should be distance-independent. What is actually being suggested here is that one or more of the subtle fields operating at a substructural level of the vacuum state creates a 5- or 6-space potential distribution that is impressed globally on our 4-space and it is transduced into an analogous \bar{A} distribution globally in the 4-space. This \bar{A} contribution then generates \bar{E} and \bar{B} contributions that produce various types of action in the 4-space. In this fashion, although there exists some spatial dependence to \bar{E} and \bar{B} , there is no spatial dependence to \bar{A} because, unlike Eq. 6c, the \bar{A} generator is not localized in our 4-space.

How Might We Detect Subtle Energies in a Reliable Way?

From earlier discussion on healers, clairvoyants, etc., it is clear that humans are potential sources and conscious detectors of subtle energies. However, because of the normal temporal variations in human functioning, we don't make very reproducible "electrodes" so statistical data gathering type of research is needed to reveal trends (Jahn, 1981; Jahn & Dunne, 1987). At an unconscious level, humans also respond to subtle and physical energies at a muscular level. Conventional dowsing studies, wherein the dowser holding a wand walks over the ground being scanned, have shown (Harvalik, 1974; Walther, 1981) that the dowsing response is a muscular action connected by a sequence of biological processes to the cause, which may often be some type of magnetic field gradient. The seat of this sensing ability appears to be the adrenal glands (Harvalik, 1974). Osteopathic practitioners have shown that patient muscle tonus can change when specific minerals or chemicals (located in a glass jar) are merely held in the left hand or placed on the stomach. This category of technique falls under the general category of applied kinesiology (Walther, 1981)

with one of the most sensitive techniques being the Omura O-ring Test (Omura, 1985).

A second category of device for detecting subtle energies would be an electrical or mechanical device, connected directly or indirectly to a living system, that responds to this subtle energy via the effect on the living system as an essential transducer. The gas discharge device (Tiller, 1990) discussed earlier falls in this category. The Motoyama AMI device (Tiller, 1988), or other such device that monitors the electrical conductance properties of an array of acupuncture points, plus the Hunt devices (Hunt, 1989), that monitor the electrical properties of muscle groups in the body, are also examples of this category. Once again, because of the temporal variability of the living portion of the detection system, statistical data gathering types of research are needed to confirm trends.

A third category of device would be a "stand alone" type of device based on a unique logic system concerning subtle energy modelling and transductively linked to a physical level read-out system. This category of device would not suffer the temporal variability due to the living system portion of the detectors in the first two categories. However, we don't really have any such devices available for use. If, as proposed in the previous section, the magnetic vector potential, \bar{A} , is the bridging field between subtle energies and physical energies, then the first step would be to construct A-meters based on other components than squid-type of devices. Then, perhaps, \bar{A} probes of \approx mm dimensions could be constructed and used to learn more about the A-field dynamics around humans acting as subtle energy sources. The insights gained from such studies would be invaluable to theoretical modelling of the subtle domains. Until humans develop the conscious capacity to reliably detect and discriminate subtle energies, these energies will not be directly observable and useful devices will need a transducer capability to physically observe energies.

A Zeroth Order Working Hypothesis for Subtle Energy

If one wishes to work with or study subtle energies, it is useful to hold some simple model as a theoretical target against which one can compare actual experimental results. With the passage of time and the accumulation of experimental data, the model becomes refined and the theoretical target shifts in appropriate ways to be consistent with the experimental results. With physical phenomena, we are used to making intensity vs frequency (or some other field characteristic) types of plots to illustrate behavior. Let us presume that the Z-axis represents intensity while the X-axis represents frequency. Likewise, subtle plane phenomena could be looked at in a similar way so that the sum of their intensities versus the metaphorical frequency could be plotted in the Z-Y plane much as if it constituted the mathematically imaginary part of a complex frequency or other field characteristic. Then, the total Z(X,Y) plot would reveal the linear and non-linear interactions between the physical plane and sub-

Spiritual

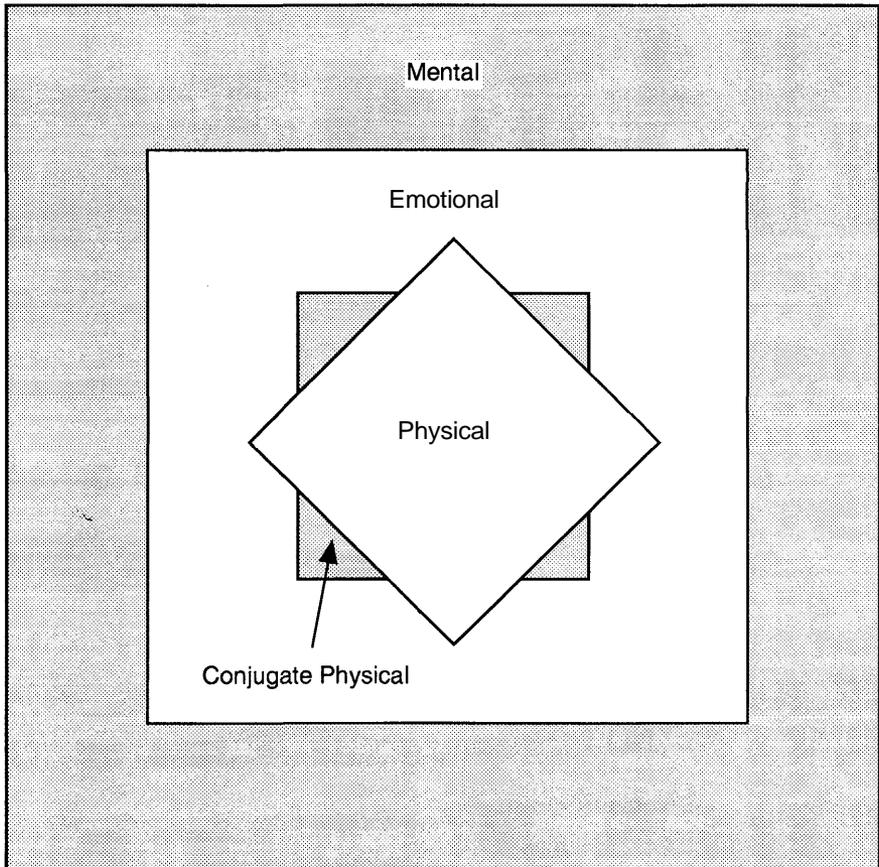


Fig.1.

tle plane phenomena. Of course, until we know more about these subtle energies, such a plot only has conceptual utility with little real accuracy.

To give some substructure to the general subtle energy domain of the vacuum state, all of which lies in the "unobservables" category, let us consider Figure 1 as a hierarchical representation of the relevant territories. Just as the physical territory phenomena are characterized and described in terms of the four accepted forces, each subtle domain is likely to have multiple unique energies involved in the various phenomena appropriate to that particular domain. Most of the domains listed in Figure 1 are familiar to us from our human experience. Only the conjugate physical domain will perhaps be new to some. Discussion of it is beyond the scope of this paper; however, its presence is delineated in Fig. 1 because it is thought, by this author, to be the domain where-

in homeopathy and remote viewing plus other phenomena have their roots. This author's writings and modeling of the past 20 years in this particular area have dealt extensively with the substructure of the vacuum state (without specifically calling it such) and with how the energies thought to function therein might account for these strange phenomena. This type of modeling is currently being revisited and refined and, at some future date, the results will be available for publication.

Conclusion

Subtle energies are real energies that are not directly observable because they function at the level of the "vacuum," the negative energy, chaotic Dirac Sea. They can be converted to an observable in our present human condition only via an intermediate transducer. Today, these transducers are primarily living systems. They account for a large class of experimentally observed phenomena that are quite inexplicable based solely on the accepted scientific paradigm of the four known forces operating in the physical universe. One known potential, the magnetic vector potential, appears to play the role of "bridge" between the subtle, unobservable energies and physically observable energies associated with electric and magnetic fields.

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ANOMALOUS PROPAGATION

by Topher Cooper

Religion ... brings man into closer touch with the inner nature of reality. Statements of fact made in its name are untrue in detail, but often contain some truth at their core. Science ... is concerned with everything but the nature of reality. statements of fact made in its name are generally right in detail, but can only reveal the form and not the real nature of existence. The wise man regulates his conduct by the theories both of religion and science. But he regards these theories not as statements of ultimate fact, but as art forms. — J.B.S. Haldane

Taos? No; But You Hum a Few Bars...

Shhh. Can you hear it? That humming sound, that's what! Well, sitting here at my desk in Massachusetts, I can't hear it either. But a lot of people all over the country, especially around Taos, New Mexico, can hear it, and some find it severely annoying. It seems to have been an "Emperor's New Clothes" type of a thing, with lots of people noticing but nobody willing to be the first to talk about it. Then, a bit over a year ago, Bob and Catanya Saltzman of Taos did speak out, and suddenly there were a lot of people saying "Yeah! What is that hum, and can we stop it? It keeps me up at night!" Apparently only some people can hear it, while others wonder what the fuss is about. It is louder some places than others, but no clear pattern has emerged. The hum has been detected electronically — it is in the 33 to 80 hertz range — but scientists have been unable to triangulate on a source. (My guess is that that means that the source is distributed over a wide area). Proposed explanations include geology, meteorology, civilian and military technology, and, of course, extraterrestrial technology.

SOURCE: John Donnelly; "Listen. It's the Taos Hum"; Knight-Ridder News Service; July 11, 1993. Contributed by Beth Cohen.

Yeti?

He's big. He's anthropoid. Legends are found all over the world about him. And actor Brian Blessed wants the BBC to pay for an expedition to find and film him (or her). The first part of the proposed expedition will be across the Himalayas from the Pamir mountains to the mountains of Bhutan. By using native trackers and night-scopes, they hope to catch sight of the creatures called by some natives the Yeti. From there they will go on to Canada for Sasquatch ("Bigfoot"), and then to Sumatra for the Orangpendeck. Sounds like it would make a great show even if they find nothing.

SOURCE: "BBC to launch expedition to find the Yeti"; UPI; April 14, 1993.

Not Yeti!

It has been fifty years since a previously unknown (by scientists) species of large mammal has been identified. That does not mean, however, that it cannot happen again. In the 1970s the Chacoan peccary was discovered in South America, and it had previously only been known from fossils. This year a wholly unknown large mammal has been found ... sort of. Scientists have still not seen a living representative but they have a number of relatively recent hunters' trophies — skulls and three complete hides — which is sufficient to classify and formally describe it. It is related to cows, goats and antelopes but does not fit within any of those categories. It has two long, gently curving horns, which resembles those of an oryx (though it is no more an oryx than it is a cow). This is the source of its name *Pseudoryx nghetinhensis*. The trophies came from the forests of the Vietnamese-Laotian border, and the Vietnamese scientists who discovered it still hope to find a living specimen. The political and social turmoil of the region does not give one much hope that it can survive in the wild much longer.

SOURCES: Dung, V.V., Giao, P.M., Chinh, N.N., Tuoc, D., Arctander, P. and MacKinnon, J.; "A new species of living bovid from Vietnam"; *Nature*, Vol. 363, #6428 (June 3, 1993), pp 443-445. Gee, H. "Reconstructed from skin and bone"; *Nature*, Vol. 363, #6428 (June 3, 1993), p398

Overheard Dialog

Speaker 2: "Yes, I can."

Speaker 1: "Can you predict the future?"

Speaker 2: "Thank you very much."

Speaker 1: "Say, that's very good!"

Contributed by Earl Wajenberg, who says that as far as he knows it "is so old it has had all of the authorship rubbed off."

Chaotic Newsletter

It's about some of the newest, most exciting areas of science: fractal modeling, chaos science, artificial life and "creative graphics". It is full of stunning, highly varied, excellently reproduced art. The text tells you about some of the newest applications in these areas, clearly and concisely, without swamping you with unnecessary details. Oh yes ... and it's free. Technically, it is advertising for editor/writer Dick Oliver's software company "Cedar Software", but Oliver believes that "you're more likely to enjoy our products if you are enjoying related stuff from other people too." Basically, he tries to get the reader excited and interested in the entire field, and so the "pitch" is extremely enjoyable and educational. The newsletter, entitled *Nonlinear Nonsense*, is

"Published on a nonlinear timeline, more or less sort of quarterly-ish. ... For future issues or back-issues, you must ask nicely".

SOURCE: Cedar Software, RR 1 Box 5140, Morrisville, VT 05661, USA; Phone: 802-888-5275, FAX: 802-888-3009

SETI Hit?

You may have heard some hype in the mainstream press about scientists actually finding radio signals from extraterrestrial intelligences. While the actual news is encouraging, it's far too early to discuss adding telephone direct dialing codes for Beta Reticuli. The background of the story is that one of the three major radio based SETI (Search for Extraterrestrial Intelligence) projects in the U.S. reported some very preliminary results at the annual meeting of the American Astronomical Society. The project, called SERINDIP, is out of the University of California at Berkeley. They simply reported that in their preliminary scan of the heavens they have so far found 164 signals that were unusual enough to be worth a second look. As soon as they can schedule time on the giant radio telescope at Arecibo, Puerto Rico for that purpose, they will do so. Such signals are routinely found. In fact, the one or two hundred most interesting signals are always selected for a second listen and inevitably prove to be spurious; that is, on turning the telescope to the same region of sky at a later date, nothing is there. But the Berkeley astronomers continue. After all, only one signal and the world will never be the same.

SOURCE: David L. Chandler; "The effort to hear ET goes into high gear"; *The Boston Globe* (Health/Science Section); June 14, 1993; pp41, 45

Magnetic Brain Zapping

Two geophysicists, Michael Fuller and Jon Paul Dobson, working with neurosurgeon Hans Wieser, have detected what appears to be an unambiguous neural response to a magnetic field. They were working with three of Dr. Wieser's epilepsy patients. These patients, who had failed to respond to less drastic therapy, were to undergo surgery to remove neural tissue contributing to the seizures. In preparation for this procedure, each had had monitoring electrodes implanted directly into their brain — allowing much greater precision than electrodes outside the skull. The investigators placed the patients' heads within a DC magnetic field much stronger than the Earth's and similar in strength to that produced by many household appliances. In all three cases, epilepsy-like brain-waves appeared — in one case for the first time in two days of monitoring. A difficulty for anyone proposing theories about mechanism is the five second delay between application of the field and the appearance of the response. The degree to which this is relevant to people not suffering from epilepsy, or those suffering from less severe epilepsy — or even to those with equally severe cases but without electrodes buried in their skulls — is open to debate. It should also be kept in mind that the results were presented at the

meeting of the American Geophysical Union, so it has not been fully reviewed by the neurophysiological community.

SOURCE: Richard A. Kerr; "Magnetism Triggers a Brain Response", *Science*, Vol. 260, #5114 (June 11, 1993); p1590

Anomalistic March on Washington

Compared to some of the famous marches of the past in which people demanded their rights from the government, it was small. In fact, only forty-five people took part. They demonstrated in a circle in front of the White House, carrying signs. The president was out of town, and the intense heat assured that there were few casual passers-by to see them. But they were citizens, using their constitutionally guaranteed right to free assembly to attempt to make their voice heard by their government, and they should be respected for that effort every bit as much as members of much larger groups. Specifically, these were members of "Operation Right To Know", who believe that the government is withholding approximately 20,000 pages of documents which prove the existence and explain the nature of UFOs and they want the government to make them public.

SOURCE: Harry F. Rosenthal; "Washington, DC demonstration"; AP; July 6, 1993

Hot UFO Info — For a Price

A new pay telephone service, called UFO Today, is now available in the US. It is targeted at the general public, but those with more knowledge of UFOs may find it interesting as well. It is to include current news stories, interviews, book reviews and primary audio material (such as taped regression sessions with abductees). The cost is US\$2 per minute, with a maximum of 5 minutes of material, changed at least every other week. The producers promise to concentrate on serious, credible material, and say that they will shut the operation down before going to sensationalistic material to make it work financially. We'll see.

SOURCE: "UFO Today"; 900-776-4UFO. All callers must be 18 or over. Customer service is 602-978-0854

Stigmata Captured on Film

Stigmata are wounds which appear on the body without apparent external cause, generally within a religious context. In most cases, they mimic the appearance (or the stigmatists' idea of the appearance) of the wounds that Jesus received during the crucifixion. Photographic evidence of such wounds have been obtained in the past, but generally only after they have appeared. Now a British television camera operator has filmed stigmata as they appeared. Heather Woods, a deacon of a mystic Christian church in Lincoln England, had had a vision foreseeing that she would get the stigmata during Easter Week. As

a result, the camera operator was on hand when the stigmata — a cross on her forehead and bleeding marks on her body, hands and feet — appeared on Easter Friday.

SOURCE: "Woman Filmed as Marks of Christ's Wounds Appear;" Reuters; April 16, 1993

MITI to Study Anomalistics

The Japanese Ministry of International Trade and Industry (MITI) created a study group, whose name means roughly "Sensitivity Business Study Group", to analyze the trend in Japan towards greater interest among consumers for activities and services that promise personal fulfillment of one form or another. They sought to study scientifically the effects of cultural and artistic activities on individuals and society, and that remains their primary focus. A study of the effects on brain-waves of such traditional Japanese tranquillity producing activities as the tea ceremony and flower arranging has lead their interests further afield. The group is considering studies in such areas as ESP and UFOs.

SOURCE: Dennis Normile; "Japanese Pondering Paranormal Projects"; *R&D Magazine*; June 1993, p21

Opinions expressed in this column are those of the author and do not necessarily represent those of JSE. Comments may be directed to the author by electronic mail at "cooper@cadsys.enet.dec.com" or U.S. post at Topher Cooper, Digital Equipment Corporation, 77 Reed Road (HL02-3/G13), Hudson, MA 01749.

THE SKEPTICAL PERSPECTIVE

by Michael Epstein, National Capital Area Skeptics

In this issue I will diverge from the usual survey of local skeptic group activities to concentrate on the general topic of skepticism. I don't have to tell the readers of JSE that the term "skeptical" has become a derogatory label. CSICOP Executive Council member Ray Hyman has been quoted (Hansen, 1992) as saying "As a whole, parapsychologists are nice, honest people, while the critics are cynical, nasty people." That should not be an indictment of skepticism, but rather of some who take extreme skeptical positions.

Skeptics: The Good, The Bad, and the Ugly

The formal history of skepticism goes back to the ancient Greeks, to those like Pyrrho of Elis who founded a school of philosophers who called themselves Skeptics, and taught that nothing whatsoever is certain and that the wise man will suspend judgment on everything. Skepticism has long been labeled as "anti-religion", perhaps because for most of the last two millennia, truths about every facet of life were defined by religious authority. Hansen (1992) has pointed out the large number of CSICOP fellows or consultants who have publicly identified themselves as holding nontheistic or atheistic views. Unfortunately, this derogatory view of skeptics (in a highly theistic society) has been picked up by fundamentalists of all sorts in an attempt to silence critics of their personal belief systems, by labeling them as skeptics. Perhaps this is a good part of the negative connotation of "skeptical" ... but it is an undeserved rap. Religious freedom as we know it today is the direct result of centuries of skeptical questioning of religious truths by those who were, for the most part, believers (Epstein, 1992). Walter Hearn, long-time editor of the newsletter of the American Scientific Affiliation (an organization of scientists of Christian belief) has pointed out that just as atheists sometimes make a pseudoreligion out of scientism, some Christians see scientific skepticism as an enemy of faith. However, gullibility should not be considered a measure of spirituality (Hearn, 1992). Those who are scientifically literate realize that what skeptics have tried to lay to rest is not a biblical but a medieval view of the world — which for many centuries was taken to be the biblical view (Hearn, 1980).

Now that we have hopefully eliminated one stigma associated with skepticism, let's examine scientific skepticism, and in particular, the function of skeptical organizations. Lippard (1993) has written a commentary on the proper role that such organizations should play. In this, he points out that there are two distinct roles: (a) being skeptical and (b) inquiring. The first case, (a) being skeptical, may involve (i) asking for evidence, (ii) offering a refutation

of the evidence, and (iii) offering a reasonable alternative explanation that does not appeal to the paranormal. Where possible, all three modes of response should be invoked to properly address extraordinary claims. Lippard expresses opposition to the belief of CSICOP Fellow C.E.M. Hansel, that simply coming up with a possible non-paranormal explanation (no matter how implausible) is sufficient to reject an extraordinary claim, and feels that Hansel's view is not prevalent in CSICOP. Randi (1981) has stated that Hansel's attitude in this respect (that when analyzing an experiment it is wise initially to adopt the assumption that ESP ... is impossible) is at odds with science and the scientific method.

Lippard rates CSICOP as doing quite well in "being skeptical" of absurd claims, but considers its performance in the area of serious claims to be poorer, because it gives less coverage to such claims. It is in the second distinct role of skeptical organizations, that of "inquiring", that CSICOP and other skeptical organizations have often not met expectations. Lippard suggests raising the publication standards for the *Skeptical Inquirer*, improving the refereeing process, and encouraging more internal criticism of published alternative explanations of paranormal claims. He warns that the failure to draw distinctions between absurd and serious claims in their treatment leads to the unfair equation of parapsychologists and fortune tellers; that skepticism without inquiry leads to an emphasis on debunking; that failure to distinguish nonbelief from disbelief leads to erroneous statements about burden of proof in cases where an alternative explanation is put forth without sufficient evidence; and that skeptics can be misled into the erroneous position that there are no genuine anomalies or that all genuine anomalies can be explained in terms of conventional science. And most important, he points out that no organization or group of individuals can legitimately take the role of the ultimate arbiter of what is true and false in the realm of paranormal and fringe science claims.

As Hyman (1981) pointed out in his critique of Schmidt's PK Experiments, skeptics can either put themselves in an adversarial role, as Hansel (1981) has done, or they can present their arguments in a manner that can be constructive and possibly help researchers to get closer to the truth. This seems to be at the heart of the matter. The contrast between constructive criticism and adversarial polemics can be illustrated by some recent statements by two well-known skeptics. Ray Hyman (1993), in a critique of the recent paper on successful autoganzfeld experiments presented by Daryl Bem at the 1993 meeting of the AAAS, made the following statements. "Taken at face value, the results do seem to show that something beyond chance is occurring ... Bem might be right. Something might be there other than simple artifact. But he is jumping the gun ... any such results would have to be replicated independently ... At this time, I think the most honorable course for skeptics is to be patient."

Contrast that with the hype regarding an upcoming NOVA television program (October 19, 1993) on the paranormal. The message, posted on BITNET skeptic with the source allegedly being James Randi (geller-hotline@ssr.com), describes "some VERY interesting videotape content featured that will not be

at all welcome in certain centers of parapsychological intrigue." It also relates that "New discoveries from several sources will be shown, and there will be wailing and gnashing of teeth in many quarters."

Certainly, skeptics are not always on the side of the scientific majority. Critical attacks have been made by skeptics against the research that resulted in the currently-accepted belief that lead (Pb) intake by children at low levels causes a lowering of IQ ... a belief that will cost the taxpayers millions of dollars for remediation and treatment. Skeptics have attacked the research evidence for the hazards of radon and second-hand smoke, and skeptical creationists are constantly on the lookout for errors in the theory of evolution.

We are all skeptics at one time or another, since we have doubts or suspend judgement about something. The scientific method requires skepticism, whether it involves physics, chemistry, or parapsychology. And certainly, there is also no hard and fast rule that the skeptic must propose an alternative explanation that is in agreement with conventional scientific thought ... if he has sufficient evidence and expertise to back up the proposal. Evaluation of that is what peer review and scientific criticism are all about. In the end, there is nothing bad or ugly about being a skeptic ... but whether believer or skeptic, there is nothing good in being a provocateur.

Skeptics Do Believe in Reincarnation

At least they do when it comes to electronic discussion groups. The SKEPTIC Bitnet group, located at York University (YORKVM1) in Toronto, gave up the ghost in early July. But like the legendary and immortal Phoenix, a new and more vibrant SKEPTIC has risen from the ashes ... thanks to the efforts of Taner Edis (edis@eta.pha.jhu.edu), a graduate student studying condensed matter theory in the Physics and Astronomy Department of Johns Hopkins University in Baltimore. To subscribe to the list, send e-mail with the instruction "subscribe <your name>" to listserv@jhvm.hcf.jhu.edu. Expect up to twenty or more messages in your e-mailbox every day if you subscribe! Recent topics have included the elusive 'red mercury' trigger for nuclear weapons, telepathy, physics and the paranormal, UFOs including the Linda abduction case, and the activities of James Randi and Uri Geller. The list is archived, with each week being stored in a separate file. The usual way to request them is by e-mail, sending a line "get skeptic log ____" to listserv, the ____ standing for the appropriate log name. If you haven't done this before, send a "index skeptic" to listserv to receive a listing of files available through "get", including the archives.

Honorary Skeptic

Enigmatic parapsychologist George Hansen, whose background includes an interest in dowsing and the SORRAT mini-lab experiments (Berger, 1991), is the author of a recent publication dealing with skeptics and skeptical organizations (Hansen, 1992). He has also recently published critiques of remote-

viewing experiments (Hansen, 1992) and the Linda UFO abduction case (Stefula, Butler and Hansen, 1993). The latter earned him stardom in a recent issue of the *International UFO Reporter (IUR)*, in which he was described as having "the soul of a hater", "behaving as if he has declared war on most of life" (Hopkins, 1993), and taking "the path of character assassination and reputation-trashing"⁷ (Clark, 1993). It has been suggested to me that if this continues, he will find himself nominated as a CSICOP consultant.

Last Words

While the following, by Robert T. Weston, may be evidence to some that you can make anything sound "spiritual", it does reflect the philosophy of many skeptics, and is a fitting way to end a discussion of skepticism.

Cherish your doubts, for doubt is the handmaiden of truth.
 Doubt is the key to the door of knowledge; it is the servant of discovery.
 A belief which may not be questioned binds us to error, for
 there is incompleteness and imperfection in every belief.
 Doubt is the touchstone of truth; it is an acid which eats away the false.
 Let no one fear the truth, that doubt may consume it; for doubt is a testing of belief.
 The truth stands boldly and unafraid; it is not shaken by the testing.
 For truth, if it be truth, arises from each testing stronger, more secure.
 They that would silence doubt are filled with fear; the house of
 their spirit is built on shifting sands.
 But they that fear not doubt, and know its use, are founded on a rock.
 They shall walk in the light of growing knowledge; the work of
 their hands shall endure.
 Therefore let us not fear doubt, but let us rejoice in its help:
 It is to the wise as a staff to the blind; doubt is the handmaiden of truth.

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Mike Epstein, electronic mail at CompuServe: 76640,1540 or Internet: mse@enh.nist.gov or by U.S. post at Michael Epstein, B-222 Chemistry, NIST, Gaithersburg, MD 20899.

GUEST COLUMN: THE COMITE PARA — A EUROPEAN SKEPTICS COMMITTEE

by J. Dommaget, president, Comite PARA
Observatoire Royal de Belgique, Brussels, Belgium

Belief in paranormal phenomena has always been part of human behaviour. It has been present in human social life since the beginning of civilization. That this was the situation some centuries ago may be understood in terms of the insufficient development of scientific knowledge at that time. As long as it remains the expression of some folklore, it may be characterized as a human tendency characterizing each epoch of civilization. However when it appears today as a system of rules organizing our lives it is a dangerous handicap to social and intellectual progress. It is regrettable that this belief is so extensive in our cultural background in contradiction with modern scientific and rational knowledge. Scientific progress should permanently reduce belief in the paranormal to an absolute minimum. Unfortunately some irresponsible but important groups of our society, especially the vast majority of the media, do not favor the battle that science conducts against erroneous information.

This situation is not new. And many scientists all over the world have devoted themselves to informing people about the dangerous impact of uncontrolled belief in paranormal phenomena. Some of them have been more active than others. In Belgium, for instance, between the world wars, Prof. J. F. A. Bessemans, doctor of medicine, chirurgica and obstetrics and later rector of Gent University, gave many lectures in our country as well as outside against paranormal belief. Being also an illusionist, he illustrated his talks with impressive demonstrations. In 1932 he even submitted a dowser to a scientifically-controlled experiment. The result was the defeat of the radiesthesist. Other scientists in Belgium were also active in their own fields, especially Dr. A. Hougardy of the Health Department, engineer A. Grosjean, director of the Geological Service, Rev. Father P. Grosjean, Bollandist S. J., specialist in miracle phenomena, and P. M. G. Levy, statistician and adviser at the Repatriation Department. On January 11, 1947, Dr. Hougardy gave a lecture at the Societe de Medecine Legale de Belgique entitled: "Radiesthesie et Sciences Medicales." At the next session of this society an opponent, Prof. Gueben of Liege University, made a presentation. The discussion was intense and Prof. Bessemans proposed organizing experiments subject to scientific controls. Prof. Gueben declined to participate, but proposed M. G. Discry, who accepted, as well as one of his students, Dr. De Nayre. A scientific inspection commission was established that may be considered to be the original core group of the Belgian

Committee of Skeptics. The two sets of experiments (with Discry and De Nayre respectively) led to negative results and were as usual, rejected by believers in the paranormal.

After some meetings and discussions it was felt by the members that this commission (composed of medical doctors, astronomers, physicists, statisticians, engineers, illusionists, members of the judiciary, etc.) should be extended and given a permanent statute. On January 15, 1949 it was decided to create the *Comite Belge pour l'Investigation Scientifique des Phenomenes Reputes Paranormaux* (Comite Para in short), which was officially registered on June 4, 1949 as an "Association sans but lucratif" (non-profit organization).

Since the committee had grown out of scientific experiments on alleged paranormal phenomena, the organization of other similar experiments became a fundamental aim of the committee, governed by a particularly non-dogmatic philosophy expressed in the slogan: *Ne rien nier a priori, ne rien affirmer sans preuve*. It was also decided to publish the results of these scientifically conducted experiments regardless of the outcome. Another reason for the creation of the committee was the perception that too many scientists demonstrated a dogmatic attitude by systematically refusing to accept discussion on any paranormal subject. Nobody may say that there do not exist unknown phenomena worth considering; it appears then too easy for believers in the paranormal to criticize this unscientific attitude. Presently it is up to the believers in the paranormal to prove their assertions, not for the skeptics to find proofs against such phenomena. It would be too easy to proclaim stupid facts or rules without the foundation of basic and time-consuming research, leaving it to others to carry out the huge effort required to demonstrate their inconsistencies. One of the first important research projects carried out by our committee took place in 1952 as a reaction to the unacceptable extension of the activity of the radiesthesists into proposing to families of people who had disappeared during the war to provide information about the situation of parents. A few honest radiesthesists, genuinely believing in their abilities, were unfortunately surrounded by a great majority of "quacks." Legal authorities found themselves involved and concerned: they demonstrated a marked interest in the committee's activity to such an extent that General Deguent, director of the *Ecole de Criminologie du Ministere de la Justice*, became one of its members. The experiments were based on the knowledge acquired by P. M. G. Levy, who since 1945 was personally involved with the alleged ability of radiesthesists to recover missing persons. A wide appeal was launched in our country to recruit volunteers. On the basis of a picture, or even only a hand-written document of inhabitants of Brussels, thirty radiesthesists were asked to locate them on a map and to state whether they were alive or not. The results were the same as would be expected by chance, and were published in 1953 under the title: "Une experience radiesthesique de recherche de disparus." Together with other results obtained at that time, a presentation entitled "Les fausses sciences, leur caractere et leur prophylaxie" was given by A. Bessemans and A. Hougardy (Bruxelles

Medical, 36, 14, 1956, pp. 697-705) in 1955 at a session of UNESCO in Madrid (Conference on the Diffusion of Science).

It was then felt to be necessary to initiate a short bulletin to inform the about one hundred supporting members of the committee concerning the results of the experiments carried out and about news concerning paranormal claims in our country. It was called "Nouvelles Breves (NB)" and appeared at first as a reotyped document of only a few pages. The first issue was published in January 1954, but due to a variable amount of material, it appeared every few months. Today it is a more or less annual publication of some 30 or 40 printed pages. Many other experiments have been conducted by the committee since then, but at the same time public education activities were also carried out. Conferences were held in various places (in both French and Dutch) and 13 lectures were broadcast between June and August, 1953 in French and Dutch. A second pendulum experiment consisting of locating some of the inhabitants of a town on a map was conducted in 1955. There were 28 volunteers. Of 112 trials only one was successful, a result perfectly consistent with chance as had been predicted, sealed and deposited in the hands of a lawyer prior to the experiment (NB, 43, 315-317; also A. de Loz, *Une experience de radiesthesie soumise a un control scientifique*, Le Scapel, 34 et 35, 1962). Another interesting experiment was carried out as proposed by Prof. Y. Rocard in his book, "Le Signal du Sourcier" (Ed. Dunod, Paris, 1962). According to this author, the dowser's "power" could be explained by variations in the earth's local magnetic field due to the underground flow of water. The experiment proposed by Rocard consisted of using a wooden frame of 1 m by 2 m encircled by a wire connected to a 6-volt battery through an adjustable resistor which allows for accurate measurement of the magnetic field. By walking along a path situated about 1 m from the frame, the dowser should be able to detect the magnetic gradient. This experiment was reproduced in five different places: 185 trials have been carried out of which 92 were successful (NB, 42, 297-308), a result perfectly consistent with a purely statistical prediction. Rocard never admitted his defeat! More details about our contacts with Rocard will be given in the next issue of NB.

In 1976 the committee took the opportunity of the General Assembly of the International Astronomical Union (IAU) in Grenoble to invite all participating astronomers to sign a statement contesting the validity of horoscopes. Surprisingly, out of 2000 participants only 383 signed without comment and 50 others asked after the meeting to add their names to the list; 36 signed with favourable comments and some others approved but reluctantly. A great majority showed a categoric unselfishness, and finally many others merely expressed a strong negative attitude for many and various reasons (see NB, 45, 374-377). These were for example: (1) such an action does not help (the majority); (2) we should not impose our acceptance or rejection of beliefs on others (some percent); (3) nobody should claim that astrology does not work (a few cases). A similar statement was proposed the same year to the participants of the General Assembly of the Association Francaise pour l'Avancement des

Sciences in Marseille. The statement was unanimously adopted (see NB, 45, 377).

Simultaneously in 1976 the Comite Para initiated research on the so-called Mars Effect on sportsmen following statistical results claimed by M. Gauquelin. This lasted many years due to the difficulty in understanding the correct statistical mechanism governing the appearance of the birthdates in the twelve classes considered by the author taking into account the motion of Mars, the nycthemeral distribution function and demographic factors. Finally a mathematical model of the phenomenon was established and published (NB, 43, 327-343). Unfortunately it seems that this model has never been clearly understood by Gauquelin and many of those who studied the phenomenon later, as it appears from their comments. The misunderstanding between the committee and Gauquelin led to a great controversy, first in Europe, later in the U.S. (see *The Humanist* from Sept. 1975 to Nov/Dec. 1977; also the *Zetetic Scholar*, 10, 1982). It indirectly became the starting point for the creation of CSICOP, which name is a translation of that of our committee.

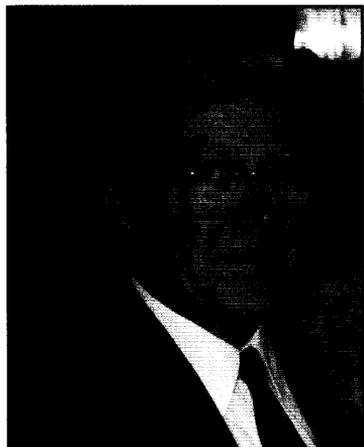
Since then, under the stimulation of CSICOP, one had to observe with great satisfaction the creation of many other national committees all over the world. The Gauquelin problem remained a controversy in which the very clear position of the committee never changed and may be summarized as follows: The committee recognizes that there is no mathematical error in Gauquelin's computation of the astronomical parameters and classes to which the sportsmen belong; there are also no errors in the Gauquelin computation of the observed statistical class distribution; the committee does not accept as correct the formula used by Gauquelin for computing the theoretical class distribution; this formula is identical to that proposed by the committee if one admits the constancy of the secular demographic factors which are difficult to estimate but are certainly not constant; the Mars effect is thus not proven and requires more extensive study.

During these last years public education has been pursued through local conferences and talks on the paranormal, as well as by participation in lessons on ethics given in High Schools (60 in seven years). The committee also participated in a few television performances, especially one with James Randi. Unfortunately this all appears to be disappointing at times, especially when, after a conference given to a group of several hundred people, we have to suffer the destruction of our positive actions by irresponsible radio and television broadcasters as well as by newspaper reports on the most cracked and stupendous paranormal phenomena. At the outset of our fight against paranormal beliefs we all had the feeling we could convince people by logical discussion and correct information. Unfortunately experience has shown that it is very difficult to get people to change their minds in that respect. And if they are persuaded to do so, they generally revert very rapidly to their original thinking, especially under the influence of the media which because of financial competition always arrange their programs to give people what they like and want: the supernatural. This is why we feel it is more useful to teach young people than adults.

We cannot expect to change the world, especially as long as we have to fight against the financial interests of the media which are presently the only effective way to open the eyes of our citizens about their exploitation by the representatives of the paranormal. Media do not correctly inform people in that respect and entertain childish considerations in people's minds. On the other hand, we must also confess that, both in the medical sphere, where too many doctors favor irrational techniques such as radiesthesie, iridology, etc., and in the astronomical community, too many do not find it necessary to fight against paranormal belief. We should here recall the annoying results of our request at the IAU General Assembly in Grenoble in 1976, and should also mention how much, to our great surprise, Prof. L. Goldberg, chairman of the IAU at Grenoble, has been criticized by some of his best friends for having allowed our committee to organize this effort within the official area of the meeting. Hence the question is: Is our action useful and should it be pursued? Is it necessary to repeat countless experiments on paranormal phenomena when they have been shown, time and again, to be "lures"? Even the biggest committee of skeptics in the world will never be able to change the situation unless a minimum of collaboration is obtained from the media. Therefore we feel inadequate to organize a "crusade" against paranormal belief. We are a little committee which lives by the financial support of its members and is unable to counterbalance the immense financial and political power of the paranormal supporters. But even so we consider that is our duty to inform people about what we know on the basis of scientific research. Whether or not people believe in our words should not ultimately be the aim of our action.

Unfortunately the public is primarily composed of people whose minds are made up. As a conclusion we nevertheless consider that we must exist and that as scientists we must teach what we know with the most convincing arguments, bearing in mind that people will nonetheless believe what they want to. But to remain silent is unacceptable to us.

Professor J. Dommanget is the former head of the Department of Astrometry and Celestial Mechanics at the Observatoire Royal de Belgique. He is also a past president of the International Astronomical Union Commission on Double Stars. Since 1981 he has been involved with the Hipparcos satellite project. Asteroid Dommanget 3450 was named in his honor in 1986.



LETTERS TO THE EDITOR

Ukraine Research Institute on Anomalous Phenomena

The Research Institute on Anomalous Phenomena (RIAP) was established in 1992 by the VERTICAL Aerospace Company. It is an independent research body, aimed at scientific studies in the fields of the UFO problem and non-classical SETI (Search for Extraterrestrial Intelligence). The Institute makes its investigations in strict conformity to requirements of the scientific method and in close collaboration with the CIS Academy of Cosmonautics and the Russian Academy of Sciences.

Specific features of the UFO problem are well-known. UFO sightings are unpredictable and therefore difficult to investigate with conventional scientific methods and equipment. Although progress has been made in the last few decades in collecting and analyzing raw data on UFO observations, the nature of genuine UFOs still remains an open question. The UFOlogical community has acquired a huge number of UFO reports (mostly of low quality), together with poorly developed methods of data treatment and hypotheses testing. As a result, mainstream science shies away from this field of research.

To open the way to a solution of the UFO problem, it should be posed as a normal scientific problem, in complete conformity to the cognitive standards of science. Being multidisciplinary, it is best divided into narrow mono-disciplinary sub-problems which could be developed with methodological rigor while providing efficient interdisciplinary coordination. It is essential to organize a systematic gathering and processing of instrumental information on UFO sightings so as to obtain really hard data on the phenomenon. This information will provide a basis for a complete picture and well-founded theoretical models.

The principal research activities of the Institute in the UFO field are: development of methods and strategies of active monitoring of UFOs by means of radar, optical, infra-red and other detection systems; instrumental studies of supposed landing sites, impacts on biological systems and UFO samples; creation of an efficient system of reconstruction of a real anomalous event on the basis of witnesses' testimonies; creation of a unified UFO data base and a computer expert system to identify UFOs; development of physical models of UFOs; psychophysiological investigations of contactees and abductees; studies in the history of the Soviet UFOlogy.

In the SETI field, we are engaged in the program "Search for Alien Artifacts on the Moon" (SAAM). This program includes: search for sunlight reflections from flat (mirror-like) surfaces of hypothetical ET objects (say, reconnaissance devices monitoring the Earth); search for other probable

artificial ET phenomena on the lunar surface; examination of the possibility of interaction between the terrestrial and extraterrestrial civilizations on the Moon; simulation of probable ET strategies for the Moon.

These investigations employ both existing catalogues of Lunar Transient Phenomena (LTP) and the data obtained by a network of LTP observers that has been recently created especially for this purpose and involves at present a group of competent observers in the Ukraine, Russia and Byelorussia.

The Institute staff consists of RIAP Fellows - the scientists and scholars, permanently employed by the Institute, and RIAP Contributing Fellows, who are engaged under contract. They are distinguished specialists in physics, astronomy, history, psychology, and other disciplines.

Formally RIAP is divided into four Departments: 1) UFOlogical Department; 2) SETI Department; 3) Historical Department; 4) Information Department. But the main research unit is a problem-oriented team which is specially created to work on a mono- or multidisciplinary problem. Such a team is headed by a chief researcher, who is wholly responsible for the quality of the work and reports to the Scientific Council and Director of the Institute.

The Scientific Council and Advisory Board of the Institute include such Russian and Ukrainian specialists in the UFO problem and SETI field as A.V.Arhipov (radio astronomer who has discovered supposed ETI radio sources near some distant stars), A.V.Beletsky (historian studying pre-1917 UFO waves in Russia), Dr. E.A.Ermilov (specialist in radio detection of aerial anomalous phenomena), Dr. V.N.Fomenko (investigator of the famous Vashka find, as well as other supposed ET artifacts), Yu.A.Fomin (doen of UFO studies in Russia), Dr. L.M.Gindilis (astronomer and SETI expert), Dr. Yu.V.Platov (Vice-Chairman of the Academic UFO Study Group), Dr. A.V.Zolotov (investigator of the Tunguska explosion), and others. The Advisory Board comprises a group of well-known Western scientists, scholars and engineers - V.J.Ballester Olmos (Spain), Dr. T.E.Bullard (USA), Dr. R.F.Haines (USA), Dr. A.Meessen (Belgium), and others. Dr. V.V.Rubtsov has been appointed the Director of the Institute.

RIAP is starting two special periodicals: the journal "Anomalistics and Astronomy" (in Russian) and the newsletter "RIAP Bulletin" (in English), as well as irregular "RIAP Proceedings".

The budget of the Institute is formed from subsidies of VERTICAL; contracts for research performed by RIAP for other organizations; grants and donations. Thanks to the VERTICAL Aerospace Company, the Institute has sufficient funding in the Ukrainian currency to enable the beginning of its research activities. However, some (limited in the number, but important) equipment, materials and services may be obtained here mainly or even exclusively in dollars or other hard currency. The lack of these means (however small) impedes considerably the work of RIAP.

In this connection, the Scientific Council of the Institute appeals to the international anomalistic community for any possible donations in a freely convertible currency. Any financial help on your part will be most highly appreci-

ated and will serve for the benefit of our common field of study, promoting its further development.

The Scientific Council is also very much interested in establishing contacts and collaboration between RIAP and serious anomalistic bodies, journals, researchers from other parts of the world. For further details please write to: RIAP, P.O.Box 4684, 310022 Kharkov-22, UKRAINE. Fax (057-2) 79-11-11, RIAP. Email: riap%office.kharkov.ua@relay.ussr.eu.net

V. V. Rubtsov

Director

Research Institute on Anomalous Phenomena

P.O. Box 4684

310022 Kharkov-22

Ukraine

Comments on "Guide to UFO Research"

In his "Guide to UFO Research" (JSE, 7, 1, p.78, Spring 1993) Michael Swords lists the "forms of strangeness" that "major UFO researchers" are said to have relegated to an "unadvertised grey basket" which contains such phenomena as alleged Marian apparitions, psychotronics and cattle mutilations. Perhaps for that reason, books by influential authors such as John Keel, Linda Howe or Salvatore Freixedo, or my own *Invisible College*, have been excluded from the list of relevant sources for future research. Among the phenomena for which Swords and his fellow "serious UFOlogists" claim to know the final answer is the Tunguska explosion. He states categorically that "we ... are aware that the Tunguska event was a carbonaceous comet."

Perhaps "serious UFOlogists" know something that the rest of us don't know, but several Russian experts who have done extensive research at the site do not support the comet hypothesis, which was first proposed by Fred Whipple and I. S. Astapovich and was more recently popularized by Carl Sagan. In particular professor Zolotov, with whom I met in 1990, and who had conducted two field expeditions to Tunguska, disagreed with this explanation because it did not account for the total energy of the blast. (*UFO Chronicles of the Soviet Union: A Cosmic Samizdat*. New York: Ballantine 1992).

More recently, in the words of *Sky and Telescope* (March 1993, p.15), "two new studies argue that the 1908 Tunguska explosion over Siberia did not herald the arrival of a comet."

In the first study Christopher F. Chyba and two colleagues from the NASA-Goddard Space Flight Center stated in the January 7th, 1993 issue of *Nature* that "small comets should disrupt too high in the atmosphere to cause much damage on the ground below," a point already made by Zolotov in his book.

In the second study, published in the March 1993 issue of the *Astrophysical Journal*, Jack G. Hills (of Los Alamos National Laboratory) and M. Patrick Goda (of Wabash College) argued that an asteroid fragment at least 80 meters across arriving at 22 km per second could better account for the devastation.

I find myself in agreement with much of Dr. Swords' article. However when it comes to Tunguska, psychotronics, alleged Marian apparitions and cattle mutilations, I would argue that the wisest course to follow is to seek more evidence and to keep an open mind instead of making dogmatic pronouncements that might discourage further research.

Jacques Vallée
1550 California St. #6L
San Francisco, CA 94109

BOOK REVIEWS

Beyond Natural Selection, by Robert Wesson. Cambridge, Mass.: M.I.T. Press, 1991. 353 pp. \$29.95, hardback, ISBN 0-262-23161-1.

This book is another criticism of the "neo-Darwinist synthesis," by which is meant the belief that evolution can be explained without remainder by natural selection working on spontaneous genetic mutations. Wesson mentions almost every weakness of evidence and every explanatory deficiency of neo-Darwinism. The gaps in the fossil record, the improbability of coordinated and simultaneous mutations that would be required to explain the giraffe's neck and the eye, the similarities in the biochemical composition of chimpanzees and humans and their great disparities in form and function, the misleading promissory note assuring us that the delineation of the structure of DNA would itself lead to an understanding of morphology, and the development of many organisms—our enquiring minds, for example—beyond any need for mere physical survival: these are only a few of the many unsolved problems that Wesson examines. Indeed, Wesson hurries his readers from one example to another so that at times I had the impression of reading a *catalogue raisonné* of everything we do not understand about biology, which is, of course, a great deal.

To be a critic of neo-Darwinism you do not have to put forward a better theory, as some neo-Darwinists have whiningly insisted their critics should. Wesson does not offer one. He is not a creationist, and his references to the explanatory value of modern chaos theory seem incomplete and almost half-hearted. He leaves the reader wondering, which is surely the state of mind most likely to lead someone else to think either of a replacement for neo-Darwinism or a major supplement to what it can explain.

Beyond Natural Selection is dense, but well-written and well-referenced. In examining its 30 pages of references I detected only trivial errors in two places.

In one respect, however, I found it disappointing. Although Wesson has familiarized himself with a vast amount of the technical literature of biology, he shows almost no awareness of his numerous predecessors in his criticisms of neo-Darwinism. He mentions Lamarck, but with some brevity, and he does not even give a reference to Lamarck's great work, *Philosophie zoologique*. He tells us that French biologists have been cool toward neo-Darwinism, but he does not guide us toward Grassé or Chauvin. And as for the numerous Anglo-Saxon critics of neo-Darwinism, they receive almost no acknowledgment apart from D'Arcy Thompson and members of the modern group at the Open University in the United Kingdom, such as Mae-Wan Ho. I cannot believe that Wesson—who shows an immense knowledge of biology—could never have

come across the writings of such critics of neo-Darwinism as E. S. Russel, H. Graham Cannon, Alister Hardy, and Michael Denton, to mention a few names only. This conclusion led me to the saddening conjecture that a too great desire to appear original had betrayed an unattractive selfishness.

Even so, I welcome *Beyond Natural Selection* as a useful work of reference to everyone interested in evolution and especially to persons who wish to know just about everything that is wrong with neo-Darwinism when it is proclaimed to provide a complete explanation of the processes of evolution. We cannot be told too often, to use one of Wesson's own phrases that "the construction of organs is a very different problem from the production of proteins, and much more difficult to master" (p. 221).

Ian Stevenson, M.D.

Division of Personality Studies
Box 152 - Health Sciences Center
University of Virginia
Charlottesville, VA 22908

First Review of Forbidden Science, by Jacques Vallée. Berkeley, Calif.: North Atlantic Books, 1992. 466 pp. \$24.95.

Before reviewing this book, I would like to explain its personal significance for me. In 1971, I had a vacancy in my research group at Stanford University for a scientist with a background in astrophysics, statistics, and computers. The position had been advertised for only a very short time when, to my surprise and pleasure, a tall, handsome, soft-spoken Frenchman with impeccable manners walked into my office and offered his services. His name was Jacques Vallée.

The name meant nothing to me, beyond the association occasioned by his visit. However, he had been working with me for only a short time when I learned that he had written three books on UFOs. At that time, I had no interest in the subject. Since Jacques was then a colleague of mine, I felt an obligation to learn something of his interests and work. The book did not persuade me that the Earth is being visited by either little green men or big pink ladies, but it did persuade me that there is more to the subject than meets the casual scientific eye.

I therefore set myself the further assignment of studying the Condon Report that had, as every scientist knew, completely settled any remaining doubts concerning the reality of the UFO phenomenon. The first few pages of the Condon Report certainly gave that impression: Condon stated that the phe-

nomenon posed no threat to national security, and that further study could not be expected to contribute anything worthwhile to scientific knowledge. However, I did not end my study of the Report at that point, but went on to read it from cover to cover. I then found that Condon's conclusions were not supported by evidence compiled by his own staff. Indeed, if one takes the staff work at face value, UFO reports represent a real problem that was then—and remains now—far from understood.

In this way, my encounter with Jacques Vallée led to my interest in the UFO problem. This in turn led to my taking an interest in parapsychology and other anomalies and, in due course, to my joining forces with twelve other scientists to found the Society for Scientific Exploration.

In view of these associations, the book has special significance for me. But now I must step back as a reviewer, and ask what significance it may have for other readers, specifically other members of SSE.

What kind of picture of the author emerges from the pages of this book? I find that the qualities that come through most clearly are Vallée's love of people, his intense curiosity, and his willingness to march to his own drum.

One learns in this book that a defining moment in Vallée's life was the discovery that scientists at the Paris Observatory were destroying scientific data because the inferences from those data were disquieting and might prove embarrassing. Vallée was outraged, as any scientist true to his profession should have been. Eventually, Vallée decided that the United States is more hospitable to divergent opinions than his beloved country of birth and he eventually settled in California, possibly the most tolerant among the fifty states of the Union.

The characters who have entrances and exits in this drama are portrayed realistically but, in general, sympathetically and often affectionately. So this person or that person is not perfect: who is? As one would expect, the most important of Vallée's many colleagues was Professor J. Allen Hynek who for many years served as scientific consultant to the Air Force Project known as *Blue Book*. Subsequently, Hynek took the lead role in arguing for the significance of UFO reports and in calling upon his fellow scientists to share in the study of this enigma. Vallée describes clearly and sympathetically the soul-searching that led Hynek to make this difficult transition.

Through these memoirs, we may share with Hynek and Vallée their initial high hopes for the declared intention of the Air Force (in 1966) to fund a scientific investigation of the phenomenon, their early concerns for the Colorado study, led by Professor Edward U. Condon, and their subsequent disillusionment and dismay at what they considered to be an inept program and unjustified conclusions.

What has been the impact on Vallée? It would appear that his studies have led him to be even more open-minded to strange data and strange ideas than he was when he first faced up to the UFO problem at the Paris Observatory. What his experience has not done is turn him into a groupie or a junkie. Vallée is as much an enigma among his fellow ufologists now as he was among his fellow

astronomers in 1961. In *Passport to Magonia*, Vallke attempted to set the UFO phenomena in the perspective of history and mythology. As far as I can tell, Vallke still prefers to look at the subject in that way and chooses to distance himself from the present-day preoccupation with more dramatic concerns such as MJ12, abductions, crashes and recoveries. Time will tell who among present-day ufologists has the sharpest insight.

Although this book is primarily a biography, it also contains abundant factual content. Most significant, perhaps, is Vallée's disclosure of a document that came to his attention in 1967, and that he then drew to Hynek's attention. It is unfortunate that Vallke regards this document as still subject to a classified status. As a result, we do not read the document but only a brief paraphrase of its contents. According to Vallke, the document provides clear evidence that the Air Force had carried out a more extensive investigation (Project Stork) into the UFO problem than its publicly acknowledged Project Blue Book in which Hynek was involved. In the planning process that led to the CIA-sponsored Robertson Panel, perhaps the most important step was the Air Force decision of what evidence their staff would present to the Panel. The memo cited by Vallke indicates that this decision was made by Stork personnel, not by Blue Book personnel.

The report of a conversation involving Hynek that occurred in June 1966 shows that the Stork revelation would not have taken Hynek completely by surprise. When Don Hanlon remarked "Maybe Blue Book was a sham from the beginning," Hynek replied "I have often thought of that."

It is likely that, within a few decades, science students will wonder either (a) "How could those few scientists ever have thought that there was anything significant behind those obviously bogus UFO reports?" or (b) "How could the vast majority of scientists not have realized that there was something significant behind those obviously real UFO reports?" Whichever question they will be asking, Vallke's book will be a valuable resource in providing first-hand insight into the early development of the UFO controversy.

Peter Sturrock
Stanford University, ERL 306
Stanford, CA 94305

Second Review of Forbidden Science

Jacques Vallée has written several critically acclaimed books about the UFO phenomenon, selling over a million copies in several languages. He is widely known as the real-life model for the French scientist played by Francois Truffaut in *Close Encounters of the Third Kind*. The legacy of his seminal role in the most significant chapters of the modern UFO saga will make for fascinating encyclopedia entries in years to come.

And there hangs the irony of this tale. For despite widespread acknowledgment of his uncommonly accomplished career, a significant number of UFOlogists (self-styled UFO experts) today insist that Jacques Vallke has forfeited his right to be considered a credible figure in the mainstream of UFO studies, coincident with the publication of his latest book, *Forbidden Science*.

Chief among the high crimes of which Vallke stands accused is his chronic nonparticipation in the Church of UFOlogy's most holy of high rituals, its sacramental equivalent of Confession: the mandatory practice of hypnotizing as many people as possible in search of proof that outer space aliens are abducting millions throughout the world and tampering with the genetic legacy of the human species. Vallée's wanton refusal to celebrate this all-important rite precludes him, necessarily, from performing the (equally hallowed and altogether mandatory) ceremony of exhibiting traumatized "pet abductees" to audiences of Oprah, Donahue, Maury, Sally, and Geraldo, whose pioneering programs featuring "Cross-Dressing Homophobic Morticians" and "People Who Talk to Milkshakes" have proved empowering to millions throughout post-literate America. (Are we to assume Vallke hasn't yet grasped how the abject humiliation of naive experimental subjects on national television can enhance public awareness of humanity's collective impotence in the face of overpowering alien might?)

An additional count of apostasy can be leveled against Vallée on grounds that for many years he has conveyed the distinct impression of devoting himself to the careful, sophisticated evaluation of UFO data, tested against original, provocative, and productive hypotheses; the "science thing." Surely Vallée is in a position to appreciate the greater importance of "playing the Catskills"—traveling the annual circuit of UFO purported "research conferences" spread round the hinterlands, setting up card-table-and-folding-chair and selling the essential paraphernalia, the rigging, the icons, the manna-charged talismans of UFOlogy: copies of critically important (and "only now available!") grainy videotapes of airborne ambiguities seen over Nevada; bumper stickers reading "End the CIA-DIA-FBI-NASA UFO Coverup"; and, of course, the ever-popular fourth-generation photocopies of mostly blacked-out "Newly Released Top Secret Government Documents."

It is a matter of public record that not a few of today's self-declared UFO authorities base their expertise on mastery of just such festivities, following the little-known although apparently universal Law of Beckett, first popularized in a now-legendary stage production focusing on the theme of human resourcefulness, *Waiting for Godot*. Because Jacques Vallke has kept a rather decided distance from the concentric institutional paranoia of UFO politics, his gravitation to the periphery of the discipline's field of vision provokes no great amazement.

Even so, this hardly explains why Jacques Vallke—a founding figure in modern UFO research—should today find himself not discernibly more welcome within the ranks of mainstream UFOlogy than a drag queen at a meeting of the Joint Chief of Staffs. To understand this quandary, we must turn to the

author's own confession (a rather damning one, to be sure) from *Forbidden Science* (page 420): "Sometimes I get the awful feeling that I am the only human being who doesn't know what UFOs are."

Thus turns a marvelous plot! By freely confessing that his exhaustive study of the UFO phenomenon has led him to put aside shopworn answers, has impelled him rather to focus on asking subtler and smarter questions, Vallée in a single stroke takes leave not only of mainstream UFOlogy—perennially mired in a self-imposed labyrinth of successively banal variations on the extraterrestrial hypotheses— but also of hardcore debunkers, whose reflexive (and curiously emotional) dismissals of all UFO data bear the stamp of ideological fixation masquerading as "fair-minded skepticism."

A pox on their equally one-dimensional houses, says Vallée, who leaves little doubt that, by and large, UFOlogists and debunkers only appear to be at odds. Both camps actually represent two versions of the same viewpoint, or mindset, best summarized as being "in the know." Most UFOlogists "know" that UFOs are extraterrestrial objects, just as debunkers "know" the field is sheer nonsense and that UFO witnesses are either hoaxers or mediocre observers deceived by hallucinations. Vallée challenges the cultish dogmatism of both camps, in the process making his own position clear:

The UFO Phenomenon exists. It has been with us throughout history. It is physical in nature and it remains unexplained in terms of contemporary science. It represents a level of consciousness that we have not yet recognized, and which is able to manipulate dimensions beyond time and space as we understand them. It affects our own consciousness in ways that we do not grasp fully, and it generally behaves as a control system.

UFOs may be extraterrestrial, but "the form of intelligence the phenomenon represents could coexist with us on earth just as easily as it could originate on another planet in our universe, or in a parallel universe," Vallée insists. "My own speculation is that UFOs operate in a multi-dimensional reality of which spacetime is a subset.. [and that the phenomenon challenges] not only our definitions of physical objects but our concepts of consciousness and reality... [and brings into question] the entire history of human belief, the very genesis of religion, the age-old myth of interaction between humans and self-styled superior beings who claimed they came from the sky, and the boundaries we place on research, science and religion."

No less impressive than Vallée's speculations along these lines is his riveting account of the major personalities and battles of the grand UFO debate, from the extraordinary Socorro landing to the notorious "Marsh Gas case." *Forbidden Science* is comprised almost entirely of Vallée's lucid journal writings detailing out-of-view conversations and heated debates among researchers, from 1957 to 1969, a unique and welcome departure from the norm of UFO literature. "The major revelation of these Diaries may be the demonstration of how the scientific community was misled by the government, how the best data

were kept hidden, and how the public record was shamelessly manipulated," the author summarizes.

Vallée acknowledges that his is simply one observer's perspective on a series of complex events. Yet it must be said that no other account of the UFO saga's climactic 1960s chapter approaches (or even attempts) the attention to detail and insight into human character of *Forbidden Science*. Not only philosophers of science and historians but lovers of detective and high adventure stories will find Vallée's study fascinating—as should students of tragedy. For in the final analysis this is a book about how, in Vallée's poignant phrase, "one of the most profound and puzzling phenomena in the history of man [was] allowed to exist around us without interference, without even a flicker of acknowledgment or an attempt at intelligent response."

Keith Thompson

Author of "Angels and Aliens: UFOs and the Mythic Imagination"

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Roads to Center Place: A Cultural Atlas of Chaco Canyon and the Anasazi, by Kathryn Gabriel. Boulder, CO: Johnson Books, 304pp \$13.95, paperback. (Available from the sourcebook Project, P. O. Box 107, Glen Arm, MD 21057.)

This book is primarily intended for those with a specific interest in the pre-Columbian road-systems of the South-West. There must be quite a lot of such folk about, to judge by the number of individual and group projects, academic or fringe, whose contributions to the subject are acknowledged by the author. They will find this a thoughtful overview of the many problems posed by the 200+ miles of mysterious roadway built by the Anasazi in New Mexico, Arizona, Utah and Colorado, and of the solutions proposed by those who have investigated them. Simply as a work of reference, it will be a valuable resource thanks not only to its summary of previous work, but also its 10-page bibliography, index, and above all the 41+ items listed and described in the "Anasazi Atlas." The book is copiously illustrated with aerial photographs vertical and oblique, with the salient features arrowed; ground-level photographs; maps, plan and diagrams. It is a thorough-going work which speaks of scholarly dedication; moreover it is well-written, clearly structured and decently printed.

If that were all, the book would take an honorable place among the specialist publications available for those who concern themselves with reconstituting the history of America before the Europeans arrived, as a useful contribution to

our knowledge, but not one likely to be of wide interest. However, there *are* wider lessons to be learned from this study.

The first lesson is that in this kind of problem area, the investigator can take nothing for granted. The Anasazi roads are there for all to see, but, like the crop circles of our own day, it is far from clear what their nature may be. The very existence of a problem was disputed by some scholars at one time: the "roads" were dismissed as geological anomalies. Others accepted that they were artifacts, but identified them as canals, or as fence lines, or as irrigation ditches.

Eventually, however, the general consensus agrees that they are roads: but what do we mean by the term? Gabriel gives us a valuable chapter at the beginning of her book in which she asks us to consider what a road is and what its purpose may be: those of us who think of roads simply as means of getting from Dallas to Fort Worth as efficiently as may be, find ourselves having to confront a more complex range of possibilities.

For if one thing is clear about these roads, it is that they are *not* the most efficient way of getting from anywhere to anywhere. They are not the most direct, nor the most convenient, nor the most satisfactorily constructed. They do not lead to the places we think they would lead to; they are often far wider than is conceivably necessary; they dodge engineering problems with dismaying cowardice; sometimes they just stop, like a child who sits down in the path and refuses to go any farther unless she is carried...

However, these criticisms are value-judgments, and imply a set of criteria; and criteria imply that we know what the road-builders were setting out to do in the first place. Suppose it wasn't simply to enable travellers to go from A to B; nor even to enable them to redistribute food, water and other supplies among the population. Once we abandon the rigorously utilitarian motive, the puzzles become less insistently puzzling...

...except that no other frame of reference gives us a better fit. Yes, there do seem to be astro-archaeological matches, so it is reasonable to look for the kind of ceremonial patterns found in other sites around the world: but as elsewhere, so here too we find that no simple as-above-so-below sky-matching will resolve the matter for us. The problems continue to tantalize: the absence of direction, the disproportion between the effort of construction and the conceivable practical benefits, to say nothing of the many minor problems—the seemingly significant orientations, the enigma of the artifacts found associated with the road and the artifacts that might have been expected but were *not* found... and much more. We feel that it was almost in despair that one project gave up on the search for an answer in terms of tangibles, and proposed that the road was "an expression of spiritual values."

It is to Gabriel's credit that she sticks her neck out and offers—albeit with all the diffidence we would expect from so careful a commentator—her own speculative model. It is a very complex model, because the problem it has to resolve is a complex one. We realize that once again, as in so many other fields of anomaly research—the UFO phenomenon is one obvious example, ball

lightning another — the worst mistake an investigator can make is to suppose there is a single all-embracing answer to his problem.

Indeed, a large part of the problem consists in defining it; and this brings me to my own favorite chapter in the book — "Discovering the roads." Few investigators, however uninformed or however much in love with their pet theories, are wholly stupid; if past searchers have variously seen these roads as natural phenomena, canals, fence lines, supply systems, irrigation trenches and much else, it is clear that their nature is far from self-evident. Gabriel describes the various approaches to road-discovery, and gives us a delightfully practical field guide which is rich in helpful tips and clues. Sherlock Holmes could not have done it better.

In short, this is a fascinating study by an author who is clearly in love with her subject as well as deeply versed in its complexities. This reviewer, for one, will never again take roads for granted.

Hilary Evans
Mary Evans Picture Library
Tranquil Vale, London

SSE NEWS ITEMS

Preliminary Announcement of 1994 SSE Meetings

The thirteenth annual meeting of Society for Scientific Exploration will be held June 9 - 11, 1994 in Austin, Texas. The exact location and the program committee will be announced in the next issue of the journal.

As a follow-on to the successful 1992 Euro-SSE meeting at the Technical University of Munich, plans are now underway to hold a second Euro-SSE meeting in August 1994 at a location to be determined. Details will be announced later.

Abstracts of 1993 SSE Meeting

Abstracts of the 1993 SSE Meeting held in Santa Fe, New Mexico, June 24-26, 1993 will appear in the next issue of JSE.

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Center for Space Science and Astrophysics,
ERL 306
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Stanford, CA 94305-4055

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Santa Fe, NM 87501

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Austin, TX 78759

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and Psychiatry
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San Francisco, CA 94109

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Davis, CA 95616

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Space Station Division
McDonnell Douglas Space Systems Co.
5301 Bolsa Ave.
Huntington Beach, CA 92647-2048

Progress in Explaining the Mysterious Sounds Produced by Very Large Meteor Fireballs

COLIN S. L. KEAY

Department of Physics, University of Newcastle, N.S.W., Australia

Abstract — Strange sounds, heard simultaneously with the sighting of brilliant meteor fireballs many tens of kilometers distant, have been an enigma for more than two centuries. The term "electro-phonetic sounds" is now widely used to describe them and distinguish them from the normal sonic effects heard after the fireball has passed by. A physically viable explanation for meteor fireball electro-phonetic sounds has been developed and verified by observation and experiment. The history of this neglected branch of meteor science is presented in some detail, drawing attention to the difficulties which stood in the way of a solution until fairly recently.

Introduction

The entry into the atmosphere of a large meteor fireball is one of the most awesome natural phenomena that a human being can witness without being greatly endangered. The largest and most spectacular meteor fireballs are very rare events, and few people ever see one during their lifetime. For about ten percent (Lamar and Romig, 1964) of those who do witness a very luminous meteor fireball, the mental impression is heightened by strange swishing, hissing and popping noises coincident with its passage across the sky. Such sounds are quite anomalous in that they imply acoustic propagation at the speed of light. This anomaly was first recognized more than two centuries ago, and has defied explanation until quite recently. It is the purpose of this essay to relate the long history of observation of anomalous sounds from bright meteor fireballs, and to recount the course of events which led to a viable physical solution of the mystery.

But first, terminology. For reasons that will emerge, the anomalous sounds heard to accompany the flight of a meteor fireball will from here onwards be called electro-phonetic sounds to differentiate them from the acoustically propagated booms and rumbles which are heard from seconds to minutes after the light of the fireball has extinguished. Electro-phonetic sounds should not be confused with the electro-phonetic effect, otherwise known as electro-phonetic hearing, which relates to the sensation of hearing arising from the passage of an electric current of suitable magnitude and frequency through the body (Adrian, 1977; Walker, 1988). Also, in the interests of brevity, the word bolide will be used in lieu of "large, bright meteor fireball", since that is its accepted meaning.

History

The first lucid account of electrophonic sounds related to the flight of a bolide originated from China in 817 A. D. At the same time as it was seen, the bolide made "a noise like a flock of cranes in flight" (Astapovich, 1951; LaPaz, 1958). It is very probable that electrophonic sounds were heard in more ancient times. Some of the celestial noises mentioned in the writings of early authors such as Hesiod and in the Christian Bible (for example Acts 2:2) may well have been electrophonic of bolide origin.

There is no doubt about the electrophonic effects of a large bolide seen over England on the 19th of March, 1719. Edmund Halley (1719) reported some eye-witnesses "hearing it hiss as it went along, as if it had been very near at hand," but he dismissed such claims as "the effect of pure fantasy." This rejection is related to Halley's realization, by careful triangulation from many observations, that "they abundantly evince the height thereof to have exceeded 60 English miles", which is far too distant for sound waves to arrive instantly. Halley was one of the first to show that meteors occur at a great height compared to most other atmospheric phenomena and that their velocity was "incredible", being "above 300 such miles in a minute."

During the next half century there were two further accounts of electrophonic bolides in the Philosophical Transactions of the Royal Society (Short, 1740 and Pringle, 1759) and another drawing attention to what now would seem to be electrophonic sounds emitted by an intense auroral display (Derham, 1727).

In the year 1783 a spectacular bolide passed over Scotland, eastern England and part of Europe (for a recent evaluation of this event and its importance to meteor astronomy, see Beech, 1989). Many reports of electrophonic sounds were gathered by the Secretary of the Royal Society, Thomas Blagdon (1784). Blagdon, a former army surgeon who was quite familiar with the delay between the flash and the boom of distant artillery, was, like Halley, perplexed by the simultaneous perception of hissing sounds with the visual appearance of a bolide more than 50 miles distant. He was so convinced of the veracity of the witnesses that he did not reject the anomaly and decided that he "would leave it as a point to be cleared up by future observers." Again, following Halley, Blagdon did a disservice to the subject by suggesting that the sound perception may be psychological through "an affrighted imagination." These conclusions, by eminent men, bedeviled studies of electrophonic sounds for two centuries.

It must be realized that the views of Halley and Blagdon were circumscribed by the limits of existing scientific knowledge: by 1784, Coulomb had not yet discovered the fundamental law of electrostatics, and a further century was to elapse before Hertz demonstrated the existence of radio waves.

Over the intervening period there were a number of inconclusive reports of electrophonic sounds from bolides and similar hissing noises from very bright aurorae. The great Leonid meteor shower of November 13th, 1833, gave rise to many reports of sounds accompanying some of the largest meteors. Denison

Olmsted (1834, 1835), Professor of Mathematics and Natural Philosophy at Yale, gathered many reports and wrote, "The sounds supposed to have been heard by a few observers, are ... represented either as a *hissing noise*, like the rushing of a sky rocket, or as slight explosions like the bursting of the same bodies. These comparisons occur too uniformly, and in too many instances, to permit us to suppose that they were either imaginary or derived from extraneous sources."

About a year after Hertz' experiment, a letter to *Nature* from Samuel Sexton (1885) drew attention to the similarity of sizzling, hissing and buzzing sounds to the affliction of *tinnitus aurium*, suggesting this to be the explanation for auroral sounds.

Even when electric fields and radio waves became well understood, the solution to the problem of instantaneous sounds from bolides remained elusive. The only evidence was anecdotal and the incidence of the sounds remained highly capricious, being sometimes heard by only one or two members of a group of eye-witnesses in close proximity to each other. This feature of the electrophonic bolide sounds, together with their simultaneity with a visual event tens to hundreds of kilometers away, undoubtedly led respected meteor observers such as W. F. Denning (1903) to uphold Halley and Blagdon's judgment, despite the existence of a number of reports suggesting otherwise, such as "While walking in my garden my attention was attracted by a distant hissing sound, and on looking up I saw the meteor" (quoted in Denning, 1903).

Although "W. F. Denning was one of those rare amateur astronomers who achieved world-wide respect and fame in several areas of astronomy ... he is probably best remembered today for his work in the field of meteor astronomy" (Beech, 1990). Therefore Denning's conclusion that "hissing and similar noises ... may be dismissed as imaginary," and is an "observational illusion" (1907) carried considerable weight among meteor scientists. He later likened auroral sounds with electrophonic sounds from meteors, stating, "They are either imaginative or due to causes not directly connected with the phenomena observed" (Denning, 1915).

Such was the climate of opinion when a spectacular bolide lit up the night sky of almost the entire State of Texas on the first of October, 1917. Engineering Professor J. A. Udden of the University of Texas gathered more than 60 reports of the event with the intention of locating its impact point somewhere in central Texas. He noted that "Several parties who saw the bright body at a distance of about 200 miles (320 km) or less, report hearing a swishing or buzzing sound, which seems to have been simultaneous with the appearance of the light." After analyzing nine reports of these sounds, he concluded (1917a), "If these observations are not subjective, the cause of the sound may perhaps be sought in either waves that, on meeting the earth, or objects attached to the earth, such as plants or artificial structures, are in part dissipated by being transformed into waves of sound in the air." His very apt conclusion was evidently prompted by one of the observers who "seems to refer this sound to objects attached to the ground" (Udden, 1917b).

Udden's perceptive hypothesis was not readily tested because of the rarity of electrophonic bolides at any given location. Every few years a report of an electrophonic bolide would appear in a scientific journal and, without first-hand experience of the event, all a meteor scientist could do was to gather and collate such reports. One such was Udden's fellow American, C. C. Wylie, Professor of Mathematics and Astronomy at the University of Iowa, who wrote an article "Sounds from Meteors" in *Popular Astronomy* (1932). In it, Wylie asserted "The explanation is without doubt psychological."

In following Denning rather than Udden, Wylie was undoubtedly influenced by his earlier investigation of the large bolide seen over central Illinois in July 1929. He concluded his summary of that event (Wylie, 1929) with the paragraph "Many letters report a swishing or hissing sound. Some report other noises; but we often have from the same community a definite report that no sound was audible to some one sitting on a porch where everything was quiet. Further, there is no mention of an appreciable interval of time between the appearance of the meteor and the hearing of the sound. In all cases the interval should have been minutes. Hence, none of these sounds can be accepted as from the meteor."

Later, Wylie (1939) embellished his case by lumping electrophonic bolides with other, better founded, examples of psychological errors affecting meteor reports from the general public. He also claimed that persons knowing that a meteor must be fifty or more miles away never report hearing such sounds.

The negative opinions of acknowledged meteor experts such as Denning and Wylie led many meteor scientists to shun the subject of electrophonic bolides. Among the exceptions was H. H. Nininger (1939), one of the first prominent meteor scientists to begin "to regard the matter as a problem in physics rather than psychology" and to "finally become convinced of the reality of such sound where the environment of the observer is favorable." Nininger based his view on the "Many cases (that) are on record where the informants insist that the sound attracted them from behind or within buildings, and, in some instances of daylight meteors, the sound was commented upon before any light was seen or known about."

Nininger had earlier (1934) proposed "that there may be, in connection with meteors, ethereal as well as aerial propagation of sound." In his later paper he revealed, "In 1934, Mr. Elmer R. Weaver of the U. S. Bureau of Standards suggested to me in conversation that possibly ether waves are transformed into sound waves upon striking objects in the environment of the observer." Nininger went on to report that it is a matter of common knowledge among radio engineers that many different kinds of object, in the vicinity of powerful radio transmitters, serve as receivers, "sometimes giving out very good reproductions of programs which were being broadcast." In these instances the radio signals were being rectified by the objects in order to demodulate and produce the audible sounds. Similar speculations involving microwave and millimeter-wave energy were presented by Anyzeski (1946).

Weaver's hypothesis came tantalizingly close to success: it was not accepted for want of evidence of radio signals from even the largest bolides. Nor had he been explicit about his suggested transformation process, so the subject remained open to speculation.

Returning now to the problem of nomenclature, Nininger (1939) proposed "that 'ethereal' be used as a designation for sound produced by the natural transformation of ether waves into audible sound" suggesting that he, too, must have been close to realizing how such sounds may be heard. Soon after, in 1940, Professor Peter Dravert, of Omsk University, introduced the term 'electrophonic fireball' (Bronshen, private communication) and this quite rapidly became the accepted practice in describing such events.

Barringer and Hart (1949), in discussing the mechanism of sounds from meteors, were unimpressed by psychological arguments. After summarizing the "mass of data" available, they concluded that "a meteor's audible accompaniment can scarcely be dismissed as a product of the imagination of the visual observers." They presumed that such sounds were carried by radio waves and estimated that a large bolide could easily dissipate energy at the rate of several gigawatts. Of this high level of energy, the thermal radio wave component was far too weak leading them to consider that the light of the bolide might be modulated at audio frequencies. This, of course, led to severe problems of generation and detection mechanisms quite unknown to science. They retreated to suggesting that the ionized wake of the bolide "may reasonably be expected to give off radiation of the intragalactic type," probably alluding to the early discoveries in radio astronomy of hissing signals of extra-terrestrial origin.

Barringer and Hart's suggestion was taken up in a serious way by Hawkins (1958a, b), who conducted a search for radio emissions from meteors at several frequencies, namely 475, 218 and 30 MHz and also at 1 Hz using a magnetometer. The meteors Hawkins observed had visual magnitudes between -1 and +5. He concluded "Thus it is probably true to state that meteors do not emit radio noise within the frequency range 1 Hz - 500 MHz above the limits of sensitivity of these measurements. Meteors therefore show a surprisingly low efficiency in converting kinetic to radio energy."

In the meantime, the problem of electrophonic bolides was under scrutiny in the Soviet Union. The most notable work was undertaken by Professor I. S. Astapovich, who compiled an extensive catalog of electrophonic bolides and drew several important conclusions from his detailed investigations (Astapovich, 1958): only bolides brighter than -9 absolute visual magnitude produce sustained electrophonic sounds; the majority of reports noted that the bolide trajectories had very small inclinations to the horizontal; and, since all of the bolides were observed at mid-latitudes, their low inclinations meant that they were moving at a large angle to the earth's magnetic field lines. Other Soviet scientists noted that the sharp crack or "peal" sometimes heard is always associated with the disintegration and detonation of the bolide. They considered these sounds to be a purely psychological effect, but accepted the physical

reality of other electrophonic sounds from bolides (see Romig and Lamar, 1963, p. 53). The controversy over the nature of electrophonic sounds was contested as strongly among Soviet meteor scientists as it was in Western countries: Academician B. Yu. Levin supporting the psychological explanation, while Astapovich, an ardent defender of the reality of electrophonic sounds, argued in favour of a physical explanation (Bronshen, personal communication).

In a review article, the experienced meteoriticist L. LaPaz (1958) observed that opinion had turned strongly towards accepting with Udden the objective reality of anomalous meteoritic sounds. He attributed this as a possible result of the "ever-increasing amount of prompt, first-hand interrogation of numerous witnesses of large fireball falls" and noted that "several attempts have been made to give rational explanation of their cause."

Such was the situation when the United States Department of Defense decided that the matter should be examined, not least because it was well known from the literature on the subject that Soviet scientists were actively investigating naturally occurring electrophonic phenomena. A contract was awarded to the Rand Corporation, which assigned Mary Romig and Donald Lamar to the work. Their "study was motivated by the possibility that a better understanding of these phenomena will lead to new techniques for determining the size, nature and path of any large body entering the earth's atmosphere" (Romig and Lamar, 1963).

Romig and Lamar's 65-page unclassified report presented 88 references, a catalog of 41 anomalous-sound observations with seven detailed maps (their Appendix A), and a further catalog of 63 Russian observations (Appendix B). Their detailed study of the evidence available was, and still is, essential reading for any student of the subject. However they reached no firm conclusion on the physical mechanism for producing the sounds except to attribute them to an "electromagnetic disturbance," and recommended that "the properties of the plasma sheath and ionized wake should be the subject of further research." Romig and Lamar gave no indication of the generation process other than to state (without a reference) that "the presence of turbulence can greatly enhance normal plasma radiations." As Romig and Lamar's report was inconclusive, many meteor scientists continued to invoke the time-honored psychological explanation for electrophonic sounds from bolides.

At this point it is worth summarizing the difficulties which faced any investigator studying electrophonic sounds, specifically those from bolides:

1. They are rare. Few people have ever heard them, either from bolides, aurorae or lightning. Nor has anyone ever had the good fortune to have had a tape recorder in readiness to record them.
2. They are evidently capricious. Not all witnesses in a group may hear them.
3. Their propagation is instantaneous, implying transmission at the velocity of light, but no electromagnetic disturbance had been known to pro-

duce sound except for electrostatic brush discharges. Such discharges do not propagate over distances of up to 300 km.

4. No electromagnetic disturbance of sufficient magnitude had ever been detected from large bolides or aurorae.
5. The method of conversion of electromagnetic radiation into sound was quite obscure.
6. No physical mechanism was known for the production of strong electromagnetic radiation from bolides or aurorae.

This is where matters stood prior to the initial resolution of the problem published by the author in 1980 (Keay, 1980b).

The Great New South Wales Bolide of 1978

On the morning of 1978 April 7, the dark moonless night sky above eastern New South Wales became as bright as day when a large bolide arriving from the southwest passed over the city of Sydney and headed seaward past Newcastle (Keay, 1980a). Despite the early hour, ninety minutes before sunrise, hundreds of witnesses deluged the news media with telephoned sightings. The bolide reached a maximum brightness of at least -15 mag (absolute) and many observers were temporarily blinded by it.

As usual with such a bright bolide event, there were a number of reports of strange sounds heard while the bolide was in view. At first I rather fashionably dismissed these as a psychological effect, until persuaded otherwise by some clear examples of sounds being noted prior to any visual acquisition of the bolide or its light.

At Rose Bay, Sydney, 20 km from the ground track of the bolide, S. McGrath "Heard a bang before seeing the light. It was like a person in the next apartment slamming a door like a screen door: rather rattley but not loud." This witness had time to get to a window and watch the bolide recede and disappear.

At Edgecliff, Sydney, 20 km from the ground track, A. Hayes "Heard a noise like an express train or bus travelling at high speed. Next an electrical crackling sound, then our backyard was as light as day."

At Vales Point, 40 km from the ground track, J. Ireland "Heard a sound like an approaching vehicle and saw a flash of light (from behind his right shoulder) as everything was lit up like daylight."

At Kotara, Newcastle, 40 km from the ground track, N. Jones heard a noise like a "phut" when the bolide flared, but "It was not loud enough to wake anyone." However a friend standing by the door on the other side of their car heard nothing.

Other impressions of the sound simultaneous with the sighting were "a loud swishing noise"; "a humming sound like a transformer or distant siren"; "like steam hissing out of a railway engine for a count of about ten"; "a swishing sound like the onset of an unexpected high wind"; and "a low moaning,

whooshing transcribable on a tape recorder." It is most unfortunate that a tape-recorder was not immediately available to the latter witness.

Publicity surrounding the 1978 bolide elicited recollections from witnesses of earlier bolides, who provided descriptions of simultaneous sounds quite similar to the above examples. It became clear to me that the psychological explanation was not realistic and a physical explanation had to be sought.

The Search for an Explanation of Electrophonic Sounds

Clearly, the transmission of energy from a bolide to the vicinity of an observer of electrophonic sounds must be by electromagnetic means. High electrostatic fields causing audible brush discharges may not be ruled out, but it is difficult for these alone to explain electrophonic sounds heard well over 100 km from the ground track of a bolide travelling at an altitude of only 30 or 40 km. Electrostatic fields produced by meteoroids entering the atmosphere vertically have been studied by Ivanov and Medvedev (1965) who showed that the induced potentials over distances of the order of the scale height may be several hundred volts for large meteoroids, hardly enough to cause electrophonic effects.

Sustained electrophonic sounds accompany bolides in trajectories having very small inclinations (Romig and Lamar, 1963) rather than very steep or vertical paths. In the latter case, sounds of an electrophonic nature are generally of very brief duration.

The work of Hawkins, already referred to, was widely considered to rule out the generation of electromagnetic radiation by meteors, at least at the frequencies examined. However it seemed to me entirely plausible that very large meteors which penetrate low into the atmosphere — bolides — could excite plasma oscillations not possible with the smaller bodies at higher altitude which were observed by Hawkins.

A literature search disclosed instances where bolides produced no electromagnetic radiation at frequencies in the broadcast band and above, up to at least the microwave region of the spectrum (Keay, 1980b). On the other hand, the acoustic effects suggest that the electromagnetic energy may lie within the audible range from 100 to 10,000 Hz, in which case no rectification is needed to detect the signals: simple transduction suffices. There exists no observational evidence ruling out em radiation at frequencies in the ELF/VLF range, so energy transfer in this region of the spectrum was accepted as a working hypothesis, with transduction to acoustic energy taking place close to or within the hearing organs of some observers (Keay, 1979). The response to this idea was mixed: a prominent meteor scientist dismissed it with the words, "Or is this a more fruitful field for psychologists rather than physicists?" (anon., 1979).

At this point, it should be noted that Romig and Lamar (1963) did suggest "that the sound is electrically transmitted and transduced near the observer." Equality of frequency was not implied because they elsewhere employed the

word transduction for examples where the em frequency did not lie in the audio frequency range. In a later paper, Romig and Lamar (1964) mentioned the possibility that "perhaps the electromagnetic waves act directly on the brain." But Lamar and Romig (1964) claimed (without references) that "There are also many individuals who report that the sounds seemed to have originated from surrounding objects rather than the fireball." Ingalls (1967) quotes a Cornell Radiophysicist, Dr. B. W. Hapke, who, with his wife, witnessed an electrophonic bolide. He stated, "The hissing and crackling noises were definitely associated with the meteor, although we cannot be sure whether or not they appeared to be coming from the meteor or from all around us."

Evidence for direct transduction of ELF/VLF em radiation into sound has been available from at least two sources. Lightning strokes emit em energy over a very broad spectrum and for many years instances of "vits," "clicks" (McAdie, 1928) "tearing noises" and "swishes" (Cave, 1926) preceding thunder have been reported. The latter are probably due to a rapid increase in the geoelectric field just prior to the discharge (Schonland, 1964), but the sharp sounds are usually coincident with the flash. Similar clicks are said to be heard at the instant of atmospheric nuclear weapon detonations from which the strong em pulse is well studied and known to peak at around 12 kHz (Johler and Morganstern, 1965).

The Electromagnetic Energy Generation Process

A large bolide sheds its kinetic energy at rates upwards of tens of gigawatts. Its luminous efficiency, a function of velocity and composition, is of the order of a few percent. Ionization is of the same order, while the remaining energy is mainly liberated as heat. The extremely high energy density residing in the plasma trail should excite all oscillatory modes possible, including those at frequencies in the audio range (ELF/VLF radiation). The problem is to discover a realistic generation mechanism. One possibility appeared to be through excitation of a hybrid-mode magnetohydrodynamic wave within the plasma of the bolide trail. For a typical ion density of 10^{23} m^{-3} at an altitude of 30 km, the plasma frequency is of the order of 10^{11} Hz, in the microwave region of the spectrum. Ion cyclotron oscillations in the Earth's magnetic field have a very low frequency, about 100 Hz, but they are prevented by the high collision frequency of at least 10^8 Hz.

Turning to the possibility of bulk oscillations in the trail plasma generating Alfvén waves, the collision frequency at the above altitude in a fully ionized plasma at a temperature of 5500° K is found to be $1.5 \times 10^{12} \text{ s}^{-1}$. This yields a conductivity

$$\sigma = \frac{e^2 n}{m_e v_c} = 7.5 \times 10^3 \text{ } \Omega \text{ m}^{-1} \quad (1)$$

Assuming an effective oscillating column length across the trail of 200 m, and taking, from the chosen altitude of 30 km, a typical value for the trail den-

sity of $2 \times 10^{-2} \text{ kg m}^{-3}$, the Lundquist number, which determines the likelihood of magnetohydrodynamic wave generation (Alfven and Falthammar, 1963), is

$$Lu = \frac{BL\sigma}{2\pi^2} \sqrt{\frac{\mu_o}{\rho_p}} \approx 0.25 \quad (2)$$

This is quite insufficient, as it needs to be much greater than unity.

In the search for other possible generation mechanisms the production of the pulse of electromagnetic radiation from a nuclear explosion was investigated. Of the principal mechanisms discussed by Price (1974), those involving X- and gamma-radiation may be ruled out for bolides. But the third mechanism, involving the expulsion of the geomagnetic field from the ionized region surrounding the bolide, bears examination.

The ratio of thermal to magnetic energy per unit volume in the plasma sheath of the bolide is given by

$$\frac{3\mu_o \rho_p RT}{B^2 M} \approx 1.3 \times 10^8 \quad (3)$$

where the molecular weight M is taken as the standard value of 29 and the strength of the geomagnetic field B is taken as 0.3 gamma ($3 \times 10^{-6} \text{ G}$). This indicates that the energy density in the sheath is 8 orders of magnitude greater than the geomagnetic field energy density and therefore the geomagnetic field is easily pushed aside by the bolide.

The geomagnetic field expelled from the plasma sheath surrounding a bolide leaks back into the trail plasma at a rate which may be estimated from skin depth considerations. It can be shown that the expelled field will penetrate the bolide trail plasma in a time no greater than

$$t_p = \frac{\mu_o \pi r_o^2}{2} \approx 5 \times 10^{-3} \text{ s} \quad (4)$$

where r_o is the initial trail radius which is taken as 1 meter for a magnitude -16 bolide and total ionization is assumed as an upper limit. This result indicates that the geomagnetic field can be expelled only from the first few tens of meters of the bolide trail. The power radiated amounts to no more than $u_m A v$, where A is the cross-sectional area of the plasma and v is the bolide velocity. This yields less than 100 watts for a bolide of magnitude -16, a consequence of the trail expansion expending most of its energy doing work against the ambient air pressure rather than against the geomagnetic field. Clearly, the mechanisms which operate to produce VLF radiation from a nuclear fireball are unimportant for a meteor fireball unless it is of comparable size and energy.

As indicated, the initial expulsion of the geomagnetic field is very temporary and the field leaks back into the plasma only a short distance behind the bolide body during the brief interval before the onset of wake turbulence. The re-established field is then controlled by plasma motion provided the magnetic Reynolds number Re , is well above unity.

Assuming the initial scale size of the turbulence L_p is of the order of the size of the bolide itself, around 1 meter in diameter, the velocity of the turbulent motion v , is one tenth of the velocity of the bolide, and the conductivity has the value given above, the magnetic Reynolds number is

$$R_m = \mu_o L_p v_p \approx 20 \quad (5)$$

This value is adequate for the transfer of the abundant turbulent wake energy into magnetic field energy for as long as the electrical conductivity remains high.

The turbulent motions in the wake have characteristic frequencies upwards of $v_p/2\pi L_p$, around 500 Hz, as energy is transferred to smaller eddies. The turbulence excites vibrations of the geomagnetic field giving rise to the emission of electromagnetic radiation in the ELF/VLF region of the spectrum. A major release of stored magnetic energy occurs when the conductivity falls, due to recombination or electron attachment as the plasma cools and the magnetic Reynolds number falls to less than unity. The twisted and tangled "magnetic spaghetti" then relaxes, releasing its strain energy as vibrations of the geomagnetic field within the earth-ionosphere cavity. These field vibrations have wavelengths of the order of 100 km, corresponding to an electromagnetic wave frequency of 3 kHz.

The above mechanism for the generation of electromagnetic radiation from large bolides is in accord with the observational finding that only very large bolides give rise to reports of electrophonic sounds. Astopovich (1958) claimed that only those bolides having an absolute visual magnitude brighter than -9 produce sustained electrophonic sounds. This empirical criterion has since been upheld by model calculations (Keay, 1992a) based on the need for the bolide to penetrate the atmosphere deeply enough to produce a turbulent wake (see, for example, ReVelle, 1979) in order for geomagnetic field trapping and scrambling to occur.

Soon after the development of the above bolide radiation mechanism by the author (Keay, 1980b) it was confirmed by Bronshten (1983a and b), who showed that a typical electrophonic bolide of magnitude -13 could generate well over a megawatt of radio power in the ELF/VLF region of the spectrum.

Electrophonic sounds have been reported from bolides fainter than magnitude -9. The sounds are usually of brief duration coincident with flaring or an explosion. Under these circumstances the expansion of the plasma fireball, still travelling forward at high velocity, would create the turbulent conditions

necessary for the generation of radio waves. Japanese observers (Watanabe, Okada and Suzuki, 1988) have succeeded in photographing a large Perseid meteor (a borderline bolide) which exploded and produced an electrophonic "phut" sound, while simultaneously from two other locations radio records were obtained (see Keay, 1992c). This was a remarkable feat given the rarity and random incidence of electrophonic bolides which makes it very difficult to record their radio emissions.

Turning now to other electrical and electromagnetic phenomena associated with meteor flight, Bronshten (1991) has conducted an exhaustive investigation of such effects, concluding that the problem is far from a solution. Many attempts have been made to investigate electric fields and currents in meteors and their trails without conclusive results. Claims that meteors can produce transient pulses in the geoelectric field (Hopwood, 1989) have not been independently verified.

The Electrophonic Sound Transduction Process

It is common knowledge that high electrostatic fields make dry hair stand on end. Human electrophonic hearing (the direct perception of electrostatic fields varying at audio frequencies) has been reported (Sommer and von Gierke, 1964) but the field strengths required are large: several kilovolts per meter. Tests undertaken in an anechoic chamber with 44 volunteers to check their response at frequencies of 1, 2, 4 and 8 kHz showed quite wide variability between subjects (Keay, 1980c). The findings of Sommer and von Gierke were confirmed for the least sensitive subjects. At the higher frequencies, 4 and 8 kHz, the greatest sensitivity was shown by three subjects (2 female) whose common characteristic was very loose or "frizzy" head hair. Their threshold peak-to-peak electric field strength was 160 V/m. Another subject (male) was found to be 3 to 4 db more sensitive at 2 and 4 kHz when wearing glasses.

These findings point towards external transduction near to the ears as being more effective than internal electrophonic hearing processes. This is borne out by Sommer and von Gierke's observation that it proved extremely difficult to eliminate direct acoustic radiation from the electrodes employed. This was also true in the above tests, which required acoustic insulation to suppress electrode sounds. Later it became clear that such electrically excited sounds were really those being sought! Interestingly, Ingalls (1967) alludes to the same problem in his similar tests, which "failed to produce effects which which could be attributed to other than normal aural paths from 20 to 20,000 Hz."

In the same anechoic chamber, tests were also conducted to check the ability of the volunteers to hear magnetic fields varying at audio frequencies. Up to peak magnetic fields approaching 0.1 mT (the maximum attainable with the equipment available) there was no significant response from any of the subjects.

Later, in another anechoic chamber, tests were conducted to test a number of mundane objects, including vegetation, for their ability to act as transducers

(Keay and Ostwald, 1991). Under electric fields of 400 kV m^{-1} peak-to-peak varying at 0.5, 1, 2 and 4 kHz, samples including aluminium cooking foil and typing paper produced sound levels in the 40 to 60 dB (SPL ref. 20 micronewton m^{-2}) range, while sprigs of casuarina pine and coastal myrtle produced from 10 to 25 dB (SPL). These represent minimal responses because the samples were not shaped or mounted in any special way to enhance their transduction ability. Of course, larger or more extensive amounts of the sample materials could be expected to produce similar sound levels at lower levels of electrical excitation. Furthermore, objects having resonant frequencies of vibration in the audio range would exhibit an enhanced response and color the sounds emitted.

From the above tests it is clear that for mundane objects, which may be close to observers of electrophonic fireballs, their transduction efficiencies may vary by ratios of more than 70 dB (power) accounting to some extent for the seemingly capricious incidence of reports of electrophonic sounds. This is borne out by the loudness range of witness reports, which span acoustic power levels (assuming the transduction occurs close to the observer) from as low as 20 dB (10^{-10} watt), "barely audible hissing" or "like a very faint sighing," to at least 80 or even 90 dB (10^{-3} watt). For example, from quite different locations, three independent witnesses of the 1986 fireball over the south-west region of Western Australia reported "a violent explosion," "very loud sounds" and "a roar" during the passage and about 90 seconds later all heard the sonic boom effects. A collection of over 100 electrophonic sound observations indicates that reports of faint sounds are far more common than loud sounds, suggesting that ambient noise levels mask electrophonic sounds in many instances.

Possibly Related Phenomena

Apart from the instantaneous sound occasionally heard to accompany a lightning stroke, as mentioned earlier, the phenomenon most obviously related to electrophonic sounds from bolides is the existence of many disjuncts of sounds from very bright aurorae. Although their existence is hotly disputed by many auroral scientists, accounts of such sounds date back at least one thousand years (Dall'Olmo, 1980). The whole subject of auroral sounds has been exhaustively investigated by Silverman and Tuan (1973) who conclude "that the observational evidence supports the reality of auroral sounds and that the most likely source of these seems to be brush discharges, and that these are generated by aurorally associated electric fields." This was also the conclusion of a life-long student of such sounds, the late Professor C.A. Chant of Toronto (Keay, 1990). Very large electric fields of the order of 10 kV per meter from intense aurorae have been measured (Olson, 1971) but the equipment he used may well have been unable to rule out a large audio-frequency component of the field. Sixty years ago, Burton and Boardman (1933) reported bursts of VLF emission accompanying flashes of auroral light and there have been many similar observations since then proving that the polar regions of the

Earth are at times a very strong radio source with power levels in the gigawatt region (Gurnett, 1974). Four possible generation mechanisms have been proposed (Gurnett, 1978) and the subject is far from resolved.

An intriguing phenomenon, which may also result in the direct transduction of ELF/VLF electromagnetic energy into sound, is the correlation often reported between strong radio emissions and subsequent earthquakes (Corliss, 1983). A number of reports mentioning a "rushing" sound preceding earthquake shocks were gathered by Milne (1841). More recent accounts of such sounds may be lacking because of greater incidence of similar man-made artifacts reducing public alertness to sounds of seismic origin. However, audio frequency electromagnetic disturbances associated with earthquakes have been discussed in this journal (Parrot, 1990), while laboratory studies mentioned by Johnston (1987), and others, show that rock fractures generate electromagnetic signals. Cress and his coworkers (1987) recorded signals peaking in the range from 900 Hz to 5 kHz. Field studies conducted by O'Keefe and Thiel (1991) during large rock-blasting operations revealed a series of electrical pulses with a repetition frequency as high as 5 kHz and an amplitude of several volts. The substantially greater energy release in an earthquake could be expected to generate signals many orders of magnitude higher in amplitude. The connection has yet to be confirmed between these experimental observations and the alarm frequently exhibited by animals immediately prior to an earthquake and, of course, the sparse reports of earthquake sounds by human observers.

Lastly, a phenomenon which is almost certainly identical to the subject of this essay, was first reported when NASA space shuttles began landing in Florida (Keay, 1985). J. Oberg and D. Potter of the Johnson Space Center began receiving reports of people hearing a "swishing" sound as the shuttles re-entered the atmosphere over northern Texas and Oklahoma. Several attempts to record the sounds and the radio signals were thwarted by mission changes and the Challenger disaster. This quest has not yet succeeded, though the relative predictability of space shuttle re-entries makes the deployment of recording equipment more likely to be rewarded than for random bolide events. The destructive re-entry of large rocket stages also may produce electrophonic sounds: such a report was forthcoming from one of the witnesses (D. Deane of Townsville, QLD) to such an event over north Queensland on the early morning of 31 July 1992, when the Cosmos 2204 rocket reentered the atmosphere.

Conclusions

Physically acceptable explanations have now been found for each stage of the transfer of energy at luminal speed from a bolide to the ears of a witness. The explanations are, as yet, far from exhaustive, and there is ample scope for

further investigations. In particular, there is need for a comprehensive treatment of the "magnetic spaghetti" radio generation mechanism taking turbulent plasma processes fully into account, and a need for thorough analysis of the acoustic response of mundane physical objects to impressed electrical stress.

The collection of high-quality observational data is an important priority. The difficulties of capturing records of bolide events are quite formidable given their rarity. Also important for correlation with synoptic ELF/VLF records are catalogs of electrophonic observations for which the times should be as accurate as possible. This has long been recognized by Russian meteor scientists who have now published five catalogs containing over 600 observations of electrophonic bolides (Bronshten, 1991). The only such catalog of Western origin is contained in Romig and Lamar's (1963) comprehensive study, although a new catalog containing more than one hundred entries has been assembled and is shortly to be published by the author in the WGN Report series of the International Meteor organization (Heerbaan 74, B-2530 Boechout, Belgium).

A limited search, with negative results, was conducted by Wang, Tuan and Silverman (1984) using data collected from a VLF monitoring station at Thule, Greenland. The distances from the bolide events were large and the frequencies examined were probably too high. There is a pressing need for better radio observations of bolides known to have been associated with confirmed reports of electrophonic sounds.

Lastly, it is essential for geo-scientists to take reports of audible phenomena more seriously in order that some progress can be made in identifying the physical mechanisms involved. The subject of electrophonic sounds from bolides is now considered respectable within the meteor science community (Keay, 1992b), and a similar shift in acceptance is now overdue within the communities of auroral and seismic scientists.

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Neural Network Analyses of Consciousness-Related Patterns in Random Sequences

DEAN I. RADIN

Department of Psychology, University of Edinburgh, Scotland

Abstract — Researchers investigating the effects of mental intention on the output of random number generators have observed person-unique patterns or "signatures" impressed into the data. A previously reported study used an artificial neural network to analyze the data produced in these experiments and found evidence supporting the signatures hypothesis. The present study again used a neural network to search for patterns, this time using new data and new network configurations. Results of eight analyses confirmed the presence of person-specific signatures. Suggestions for creating practical applications from this phenomenon are outlined.

Introduction

For nearly four decades, more than seventy researchers have investigated hypothesized correlations between mental intention and the statistical output of electronic random number generators (RNG) (Radin & Nelson, 1989). Some of these researchers have claimed that the data reveals person-specific patterns, occasionally referred to as "signatures" (Berger, 1988; McConnell, 1989; Nelson, Dunne & Jahn, 1986).

These signatures may be described as unique graphical shapes that appear in summaries of individual datasets. For example, Figure 1 illustrates signatures for three hypothetical people.

In an earlier paper (Radin, 1989), I described the use of an artificial neural network to examine the signatures hypothesis using data collected in RNG experiments at the Princeton Engineering Anomalies Research (PEAR) Laboratory at Princeton University (e.g., Dunne & Jahn, 1992). Results of that study suggested that an artificial neural network could be trained to associate patterns in RNG data with the identity of individuals who produced the data. It was also shown that a network trained on repeated independent examples of the same individuals' efforts improved the network's ability to correctly identify the patterns.

In this paper, I describe the results of new neural network analyses designed to replicate and elaborate upon the previous findings, using additional data provided by the PEAR laboratory.' In particular, I attempted to

- (a) confirm the previous finding that person-specific signatures can be identified by a neural network;

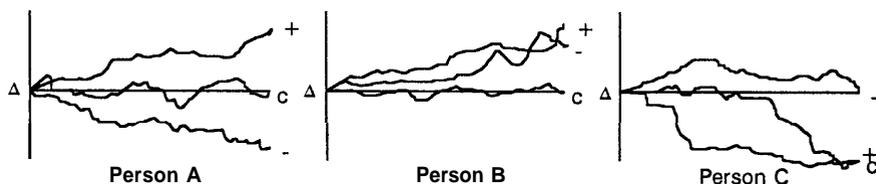


Fig. 1. Illustration of the signatures idea for three hypothetical people. In a typical experiment involving RNGs, individuals are asked to mentally "cause" the output of an RNG to randomly walk *above* chance expectation (indicated by the "+"), to randomly walk *below* chance expectation ("-"), or to randomly walk around chance as a control ("c"). These graphs plot cumulative deviations (hence the Δ symbol) from chance expectation for each of three participants. The signatures claim is based upon the observation that individuals' cumulative data seem to show different characteristic shapes and that these shapes apparently persist over repeated experimental trials and different RNG devices.

- (b) explore whether a network could learn to associate RNG data with individual identities, the type of mental intention the individuals employed, the type of RNG used in the experiment, and then successfully transfer the learned associations to new data; and
- (c) explore whether a network could learn the associations described above, but under an experimental protocol where individuals generated the data with a single button press.

Neural Networks

This section provides a quick overview of artificial neural networks for readers not familiar with this technology. Refer to Radin (1989) for more details on neural network computational techniques as used in the present analysis.

Artificial neural networks encompass a class of relatively new computational techniques and models, variously called neural networks, parallel distributed processing, and connectionism. The technique has attracted broad interest within artificial intelligence, the cognitive sciences, and the neurosciences because neural networks (a) are advancing the theory and development of self-organizing, adaptive systems, (b) are providing solutions to previously difficult problems in pattern recognition and image analysis, and (c) are providing effective, computational ways of studying biological neural networks (Shriver, 1988).

Neural networks are a form of parallel processing roughly based upon research about how the brain encodes and processes information. The power of these networks rests upon the finding that when numerous elementary processing units are richly interconnected, they can automatically learn to associate arbitrarily complex inputs with arbitrarily complex outputs. Properly configured, these networks can also implement desirable features such as self-organizing associative memories, automatically derived statistical descriptions of

spatial and spatio-temporal data, and autonomously acquired knowledge by observation.

Information processing in a neural network involves interactions among large numbers of artificial "neurons." These neurons, also called nodes or units, have four main components, namely: input connections, through which the unit receives activation from other units, a summation function that combines various input activations into a single activation, an output function that converts summation of input activation into output activation, and output connections by which a unit's output activation arrives as input activation at other units in the system (Jones & Hoskins, 1987).

Neural networks typically consist of several layers of nodes. All networks used in the present analysis had three layers: input, middle, and output. The middle layer is sometimes called the hidden layer because it is not "seen" directly by the external world. That is, the input and output nodes form the explicit association problem to be solved, whereas the hidden nodes and the rich set of interconnections among the nodes are used to increase the computational space of the network and to allow complex, nonlinear, nonparametric relationships to be learned (Ripley, 1992).

Method

The Data

Data from four individuals were used in the present analysis (Nelson & Dobyns, 1991), provided in the form of trial scores (the number of times a random bit matched an alternating target bit in a sequence of 200 random bits; mean expectation = 100). One experiment was called a series, consisting of 1000 trials in each of three mental "aim" conditions: aim for high trial scores (i.e., numbers greater than 100), for low scores (numbers less than 100), and a "no aim" baseline condition.

In the PEAR Laboratory protocol, the "manual mode" of operation generates one trial at each press of a button. All of the present series were generated using various types of "automatic mode," in which runs of 50 trials, 100 trials, or 1000 trials were automatically generated with a single button press. Thus, each series of 1000 trials in the present database was produced with 10, 20, or a single button press per aim condition (depending on which run-length was selected). The database consisted of a grand total of 65 series generated with 50 or 100 trials per aim condition, and 82 series generated with 1000 trials per aim condition.

Besides the raw trial scores, the database also included information about the identities of the persons who produced the data (identity arbitrarily coded as 1, 2, 3 and 4), intentional aim (high, low, baseline), type of RNG (pseudo-random algorithm or truly random), mode of instruction ("volitional," in which participants chose the direction of intentional aim themselves, or "instructed," in which participants were randomly assigned the direction of aim), and several other parameters which were not used in the present analysis.

Data Preparation

The previous neural network analysis (Radin, 1989) employed a simple way of summarizing an individual's performance over one series: a measure of the overall shift of the mean from chance expectation per aim condition, calculated by a *t* score, and a test comparing the observed standard deviation per aim condition with the chance expected standard deviation, calculated by a chi-squared statistic and then transformed into a normal *Z* score. Thus, the raw data in each of three aim conditions was transformed into two normalized statistics, for a total of six summary statistics per series, per person. Note that the precise forms of the summary statistics do not matter provided that the method of determining them is consistently applied to all data.

To evaluate whether an artificial neural network has learned to identify a person based upon his or her data, two datasets are needed for each individual: one is used to *train* the neural network (called the training dataset) and the other is used to examine the extent to which the trained network has *transferred* (called the transfer dataset) its learned knowledge to new data. In the previous study (Radin, 1989), each series was split in half due to limited data, using the first half of the data as the training set and the second half as the transfer set.

In the present analyses, because the database provided sufficient numbers of repeated series per person, the training and transfer datasets consisted of summary statistics from completely independent series. This has the disadvantage of losing potential psychological, physical, and environmental similarities in a given individual's performance over a short period of time, but it also provides an advantage: If the signatures hypothesis is genuine, and appears *across* datasets, it would help support the notion that signatures are more than short-term drifts in the RNG equipment, or some other short-lived methodological artifacts that are somehow related to specific individuals.

Control Datasets

The previous study indicated that because of the high noise inherent in data produced by random number generators, to adequately judge the extent of knowledge transferred we need to compare transfer test results using actual series data against some form of control dataset. In the first analysis described below, two control datasets were employed: (a) a *random* dataset generated by computer-simulating the PEAR experiment without any human intervention using a pseudorandom number generator (PRNG),² and (b) a *scrambled* dataset generated by using the original PEAR data, but where the subject identity codes have been scrambled. In the remaining analyses reported here, only random datasets were used for controls (for reasons that are described later).

Implementation Software and Hardware

A three-layer neural network using the conventional backpropagation learning scheme was used in these analyses (Jones & Hoskins, 1987; Rumel-

hart, Hinton & Williams, 1986). The program was written by the author in the C language and a Silicon Graphics IRIS 4DTM workstation was used to run the parallel processing simulations.³

Results of Analyses 1 - 4

Analysis 1

The primary question of interest was whether a network could learn to associate data produced by an RNG with the person who was attempting to influence that RNG, then recognize that person on the basis of new data the network had not "seen" before.

This network consisted of six input nodes, four output nodes, and twelve hidden nodes.⁴ The network "learning rate" was set to 0.2,⁵ and the network was trained for 750 passes.⁶ To avoid potentially biasing the network during the training process, the *order of the rows* in the training set was effectively randomized by numerically sorting the dataset according to the first data item in each row.⁷

The neural network data format was as follows:

| | | | | | | |
|---------------------------------------|--------|-------------|--------|---------------|--------|---------|
| ----- INPUT ----- ----- OUTPUT ----- | | | | | | |
| <i>t</i> hi | Z hi | <i>t</i> lo | Z lo | <i>t</i> base | Z base | ID |
| -1.994 | -0.803 | 0.884 | -0.848 | 0.563 | -1.116 | 0 1 0 0 |

where *t* and *Z* are series summary statistics as explained above, "hi", "lo", and "base" correspond to the three directions of intentional aim,⁸ and "ID" corresponds to an arbitrary but unique code used to identify the person who produced the data. Person one was simply coded as "1 0 0 0," person two as "0 1 0 0," and so on. The first six data items are inputs to the network and the ID code is the output that is to be learned.

The values at the network's output nodes represent network activation levels, which normally range continuously between 0 and 1. However, because the output ID codes in this context were defined as exactly 0 or exactly 1, a simple transform was used whereby network output activation values ≥ 0.5 were recoded as 1 and activation values < 0.5 were recoded as 0.

To maintain consistency with previous analyses (Radin, 1989), only PEAR series consisting of 50 or 100-trial runs were used in this first analysis (i.e., 1000-trial series were excluded). There were a total of 65 such series available. For simplicity, a training set of 40 series was created, composed of 10 series per person (i.e., 4 people x 10 series each), and random and scrambled datasets consisting of 15 series, composed of 5 series for each of three people. (That is, three people because one person in the database only had data for 10 series, thus no additional series were available to be added to the transfer dataset.)

Note that an unbalanced number of series per person in the transfer or scrambled datasets would not affect the transfer test results because the network had

already learned its associations from a completely balanced training dataset. If the training dataset were biased, such that it represented greater or fewer numbers of series for some people, the network would indeed capture that bias and any transfer test results based on what the network had learned would reflect that bias. But, since the training sets reported here were all symmetrically balanced (according to ID code, aim, and the other parameters), no such bias was permitted or possible in the transfer, random, or scrambled tests."

For the random dataset, new data were generated by simulating an entire PEAR experiment using a well-tested PRNG.¹⁰ After the experimental data was simulated, *t* and *Z* scores were calculated in the usual way, and a new dataset was created (i.e., the random dataset) which was identical to the transfer dataset in form, except that all data was produced pseudorandomly and without any form of human intervention.

For the scrambled dataset, ID codes were shifted right two positions, such that person 1's code was shifted from "1 0 0 0" to "0 0 1 0," person 2 shifted to "0 0 0 1," and so on. In this way, a control dataset was formed using the identical data from the transfer dataset, but the identities of the people who produced the data were scrambled.

At this point I must make a slight diversion into the mechanics of neural networks. When a neural network is trained, it goes through a process roughly analogous to the process of casting a metal object. Metal is heated until it turns into a liquid, it is poured into a mold, and it is allowed to cool. Ultimately, the metal permanently takes on the shape of the mold. Heating is like the initial disorganized or randomized state in a neural network, cooling represents the organizing or training process, and the mold represents the nature of the problem that is cast into the network.

When metal cools into a mold, it does so wholistically, with all molecules of the metal settling into the mold simultaneously. The metal does not settle into its proper place sequentially, one molecule at a time. The process of training a neural network is similarly wholistic and non-algorithmic, and as a result the eventual state that a network "settles into," mathematically-speaking, is a *priori* unknown. This is because a neural network (like a liquid metal) provides a large computational space (many configurations of molecules) in which to solve its association problem (the final cast can be achieved by many different arrangements of molecules), and the final solution to the problem depends on the characteristics of the problem (the cast) as well as upon the initial conditions of the network (the heat of the metal). The initial state of these networks is intentionally randomized (i.e., the initial weights on the interconnecting links between nodes are random numbers), thus although the learning process is fully determined by the back-propagation learning equations, the final learned state for a given input-output problem is undetermined.

The fact that there can be many solutions to the input-output problem suggests that it may be difficult to know when (or indeed, if) a network has achieved an optimal solution to any given problem. If we were to train a network once, then judge the amount of information learned in a transfer test

TABLE 1
Transfer test results using actual data, random data, and scrambled data.

| | Transfer to | mean | sd | N | series | % | t-test (df) |
|------------|----------------|------|------|-----|--------|------|-------------|
| Analysis 1 | Actual data | 3.82 | 1.63 | 100 | 15 | 25.5 | |
| | Random data | 1.63 | 1.29 | 100 | 15 | 10.9 | 10.48 (99) |
| | Scrambled data | 1.13 | 1.05 | 100 | 15 | 7.5 | 13.80 (99) |

Mean and *sd* are the average number and standard deviation of correctly identified outputs out of the total number of *series* in the test dataset, after N repetitions of the training-transfer process. Percent (%) indicates the mean percentage of correctly identified items in the transfer dataset. *T-tests* compare the difference between means of actual vs. random and actual vs. scrambled datasets.

using actual, and then randomized data, we would not know if those results were characteristic of the "true" performance of the network. Thus, to help estimate this generalized performance, the training process was repeated from scratch 100 times in this analysis (and 50 times in succeeding analyses), each time using newly randomized initial conditions. The means of the distributions of transfer test results using actual and control data were then compared to see how well the network had performed.

Results, shown in Table 1, suggest that the network learned to associate performance "signatures" with individual identities, confirming the findings reported in Radin (1989). The table shows that when testing the network with actual data (this was called the "transfer" test), the mean number of correctly identified ID's was 3.82 out of 15 possible, or 25.5% correct on average. The same measure with random data was 10.9%, and with scrambled data, 7.5%. A t-test of the differences among these means shows that the network identified significantly more IDs using actual data than control data. (A more complete discussion is deferred to the Discussion section after Analysis 4.)

Analysis 2

This analysis examined whether a network could learn to associate data from a single intentional aim and the identity of the person who produced the data, with the direction of intentional aim plus the type of RNG that produced that data. The data format was as follows:

| | | | | |
|--------|-------|---------|-------|----------|
| t | Z | ID code | Aim | RNG type |
| -2.372 | 0.006 | 0 0 0 1 | 0 1 0 | 0 |

where t, Z and the ID code are as described above, "Aim" corresponds to the direction of mental aim (high = "1 0 0", low = "0 0 1", and control = "0 1 0"), and "RNG type" corresponds to the type of random number generator used to produced the data (pseudorandom = "0", truly random = "1").

TABLE 2
Transfer test results for Analysis 2 using actual and random data.

| | Transfer to | mean | sd | N | series | % | t-test (df) |
|------------|-------------|------|------|----|--------|------|-------------|
| Analysis 2 | Actual data | 9.44 | 2.74 | 50 | 48 | 16.6 | |
| | Random data | 6.68 | 2.44 | 50 | 48 | 11.7 | 5.27 (49) |

The training dataset consisted of 72 such data items, corresponding to 24 series, six from each of four people (i.e., 24 series at 3 aims per series = 72), and the transfer and random datasets consisted of 48 new data items, corresponding to 16 series of data, four from each of four people. The network learning rate was set at 0.1, with 15 hidden nodes, and training continued for 3500 passes. A scrambled dataset was not employed in this or in succeeding analyses because all previous transfer tests using control data had shown no significant differences between random and scrambled datasets.

Results, shown in Table 2, indicate that the network was able to learn to associate individuals' RNG performance and identity with their intentional aim and the type of RNG they used. (Again, further discussion is postponed until after the description of Analysis 4.)

Analysis 3

The purpose of this analysis was identical to that of Analysis 2, except that *only* data produced in 1000-trial series were employed. A total of 54 such series were used in the training dataset and 48 series used in the transfer and random datasets. The network learning rate was set at 0.2, with 15 hidden nodes, and training continued for 2500 passes. The data format was identical to that used in Analysis 2:

| | | | | |
|-------------------|--------------------|---------|-------|----------|
| ----- INPUT ----- | ----- OUTPUT ----- | | | |
| <i>t</i> | <i>Z</i> | ID code | Aim | RNG type |
| -1.899 | -0.221 | 0 0 1 0 | 0 1 0 | 1 |

Results in Table 3 show that the network was able to learn to associate individuals' RNG performance and identity with their intentional aim and the type of RNG they used.

TABLE 3
Transfer test results for Analysis 3 using actual and random data.

| | Transfer to | mean | sd | N | series | % | t-test (df) |
|------------|-------------|------|------|----|--------|------|-------------|
| Analysis 3 | Actual data | 8.02 | 2.06 | 50 | 48 | 16.7 | |
| | Random data | 6.12 | 2.34 | 50 | 48 | 12.8 | 4.27 (49) |

Analysis 4

This analysis examined the possibility that if a network were trained with additional information beyond input data and the ID code, it may further enhance a network's ability to identify the type of intentional aim used to produce that data. To test this hypothesis, a data format with eight inputs and three outputs was used, as follows:

| INPUT | | | | | OUTPUT |
|----------|----------|---------|----------|-------------|--------|
| <i>t</i> | <i>Z</i> | ID code | RNG type | Instruction | Aim |
| 1.936 | -0.944 | 00 10 | 1 | 0 | 00 1 |

where the additional input items are "RNG type," as described previously, and a new item called "Instruction." Instruction is either volitional (value = 0), which is when the person gets to select if they want to aim high, low, or baseline on a given button press, or instructed (value = 1), which is when an RNG is used to assign the direction of aim for the subject.

A total of 72 series were used in the training set (data excluding 1000-trial series) and 117 series in the transfer and random datasets. The network learning rate was set at 0.2, with 10 hidden nodes, and training continued for 2000 passes. Results in Table 4 indicate that the network was able to learn this association, and the actual vs. random t-score of 5.62 was larger, but not significantly so, than the similar actual vs. random t-score of 4.27 obtained in Analysis 3.

Discussion

The results shown in Tables 1 - 4 indicate that the data in the PEAR RNG experiments show forms of internal structure that are unexpected by chance and are consistent with the hypothesis that person-unique patterns are somehow impressed into the data. The differences between means of correctly identified outputs given actual data vs. control data (both random and scrambled) are statistically unambiguous. Even the absolute magnitude of some of the differences are non-trivial (e.g., 10.9% knowledge transferred with random data vs. 25.5% knowledge transferred with actual data, as seen in Analysis 1). But what does this all mean?

In discussing the results of the above four analyses with colleagues," three questions were repeatedly raised: First, What do the percentage transfer rates in Tables 1 - 4 actually mean? In Analysis 1, for example, shouldn't one expect

TABLE 4
Transfer test results for Analysis 4 using actual and random data.

| | Transfer to | mean | sd | N | series | % | t-test (df) |
|------------|-------------|-------|------|----|--------|------|-------------|
| Analysis 4 | Actual data | 28.16 | 4.89 | 50 | 117 | 24.1 | |
| | Random data | 22.92 | 4.32 | 50 | 117 | 19.6 | 5.62 (49) |

by chance to see a 25% transfer rate since there are only four possible output nodes in that network? Likewise, shouldn't one also expect 25% transfer rates by chance in Analyses 2 and 3, and a 33% transfer rate by chance in Analysis 4? Why do none of the transfer results using random data show these expected percentages? These questions are addressed in Analyses 5 and 6, below.

The second question asked what the network was learning that allowed it to discriminate between actual and random data, and how can we begin to understand what was learned given that the information is captured in a computational space of many more than three dimensions (perhaps ten to fifteen "dimensions," depending on the number of hidden nodes used in the network). Analysis 7 addressed this question.

The third question was, Does the network learn to identify only unusual data items, such as data due to one "outlier" individual, or does it learn something about the entire body of data, somehow capturing the gestalt of the dataset?

Results of Analysis 5 - 8

Analysis 5

As a reminder, the data presented to the neural network in Analysis 1 used the following format:

| INPUT | | | | | OUTPUT | |
|--------|--------|-------|--------|--------|--------|---------|
| thi | Zhi | tlo | Zlo | t base | Zbase | ID |
| -1.994 | -0.803 | 0.884 | -0.848 | 0.563 | -1.116 | 0 1 0 0 |

Thus, if random (i.e., unpatterned) data were input, it seems reasonable that we should expect the observed output to correctly match the desired output only 1 in 4 times or 25%. Yet Table 1 shows that the actual data transferred at 25% and random data transferred at only 11%, implying that either the network did not learn anything and somehow the random dataset gained "negative knowledge."

The answer to this apparent problem is actually quite simple. As mentioned above, the continuous-valued activation levels at the network's output nodes were explicitly transformed into either a 0 or a 1 bit by coding activation output values ≥ 0.5 as 1 and activation output values < 0.5 as 0. This means that each output node was coded as either exactly 0 or exactly 1. Since the network had four output nodes, it produced 16 possible output codes (0000, 0001, 0010, etc.) of which only 4 were valid ID codes (i.e., 0001, 0010, 0100, and 1000). We would therefore expect the chance transfer rate for random data to be about 1 in 16. However, this is not precisely the case because during the training process the network is only presented with 4 valid ID codes as possible outputs, so it becomes biased to produce only these four outputs.

The result is that the true chance transfer rate lies somewhere in the range of 1 in 16 to 1 in 4, or 6.25% to 25%. Unfortunately, the theoretically expected

chance transfer rate is unknown. To further compound the problem, if the network really does learn something that can be transferred to actual data, then the scrambled dataset should result in a transfer rate that is *less* than that observed with random data. This is because the data in the "scrambled" dataset are the same as in the actual dataset, but the ID codes are explicitly mismatched by being shifted. Because we postulate that actual data tend to produce correct ID codes as output, if we attempt to match correct IDs with scrambled IDs, we should explicitly produce too many *mismatches*. Table 1 shows that the transfer rates do indeed fall in the expected rank order in Analysis 1, with transfer percentages in the order: actual > random > scrambled. Unfortunately, interpreting what the magnitudes of these percentages mean is problematic.

To help understand whether these results are in line with what one would expect by chance, I changed the output coding transform to define the one node containing the *maximum* output activation as 1, and the other three nodes as 0.¹² For the network used in Analysis 1, there are only four possible outputs, thus the random expected transfer rate using the maximum coding scheme is exactly 1 in 4, or 25%. The scrambled transfer rate should be less than 25%, and the actual transfer rate should be greater than 25%. The same network parameters were used as previously described in Analysis 1.

Table 5 shows the results, which conform to the above expectations. The 95% confidence interval for the random transfer rate includes the expected 25%, the scrambled transfer rate is well below random, and the actual transfer rate is well above random. (t-tests were deemed unnecessary because the magnitude of the differences are self-evident.)

Analysis 6

To replicate results found in Analysis 5, we combined the datasets used in Analyses 2 and 3, and tested a network with the following input/output configuration:

| ----- INPUT ----- | | | ----- OUTPUT ----- |
|-------------------|----------|---------|--------------------|
| <i>t</i> | <i>Z</i> | ID code | Aim |
| -2.372 | 0.006 | 0 0 0 1 | 0 1 0 |

This is the same input as in Analyses 2 and 3, but the output is just three levels of aim (high, baseline, and low). The training set consisted of 126 items; the transfer and random datasets consisted of 105 items. The random transfer rate using the maximum coding scheme is expected to be exactly 1 in 3 or 33%.

Results shown in Table 6 again conform to expectation. The random transfer rate 95% confidence interval includes 33%, as expected, but the transfer rate using actual data exceeds 33%. (A scrambled dataset was not tested in Analysis 6.)

TABLE 5
Transfer test results for Analysis 5 using the maximum coding scheme.

| | Transfer to | mean | sd | N | series | % | 95% confidence interval |
|------------|----------------|------|------|----|--------|------|-------------------------|
| Analysis 5 | Actual data | 5.72 | 1.39 | 50 | 15 | 38.1 | 35.6 - 40.7 |
| | Random data | 3.66 | 1.64 | 50 | 15 | 24.4 | 21.4 - 27.4 |
| | Scrambled data | 2.20 | 1.41 | 50 | 15 | 14.7 | 12.1 - 17.3 |
| | Chance | 3.00 | | | | 25.0 | 13.80 (99) |

Mean is the number of correctly identified outputs, *sd* is the standard deviation of the mean, *N* is the number times the neural network simulation was repeated, *series* is the number of data items used, *percent* (%) is the percentage of data items correctly identified (i.e., mean / series), and *95% confidence* is the 95% confidence interval for the percentage.

Analysis 7

Now that there is some reason to believe that the network transfers knowledge to actual data but not to random data, the question naturally arises as to what exactly has the network learned? Unfortunately, visualizing this learning is difficult because the network has captured its knowledge in an abstract multidimensional space. In Analysis 5, for example, the network has effectively taken a 6 dimensional input, filtered it through a 10 dimensional (hidden layer) space, and transformed that into a 3 dimensional output.

To help visualize what was learned, I used a trained network from Analysis 6, fixed the ID code for each of the four individuals in turn, applied *t* and *Z* values ranging from -2 to +2 to the input nodes (in increments of 0.2), and then examined the value of the output nodes. Because the maximum coding scheme was used, there were only three possible neural network outputs: high aim, baseline, and low aim. A three-dimensional graph was thus created, where the *x* and *y* dimensions were *t* and *Z*, as described above, and the *z* dimension was the network output coded as 1 (high aim), 0 (baseline), and -1 (low aim). These three-dimensional graphs are displayed as contour maps in Figure 2.

These graphs illustrate how the network partitioned its computational space to create the equivalent of an input-output associative map. The maps also reveal that the input variance measure (the ordinates in Figure 2) helped the network discriminate among the different individuals and different intentional aims. This is interesting because the usual measure of interest in RNG experi-

TABLE 6
Transfer test results for Analysis 6.

| | Transfer to | mean | sd | N | series | % | 95% confidence interval |
|------------|-------------|-------|------|----|--------|------|-------------------------|
| Analysis 6 | Actual data | 38.22 | 3.75 | 50 | 105 | 36.4 | 35.4 - 37.4 |
| | Random data | 35.60 | 5.10 | 50 | 105 | 33.9 | 32.5 - 35.3 |
| | Chance | 35.00 | | | | 33.3 | |

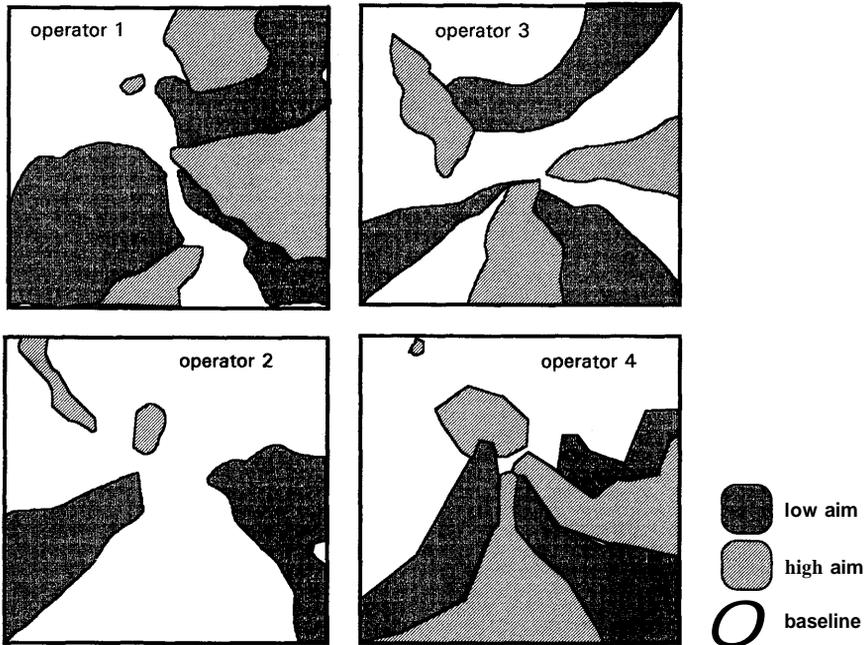


Fig. 2. Visualization showing how the neural network partitioned its computational space to solve the input/output association problem for the four operators. The x and y axes correspond to t and Z scores, as described in the test, and the three different shades correspond to the low aim, high aim, and baseline conditions.

ments is to shift the mean, not the variance. In general then, what the network has learned is represented by the associative maps shown in Figure 2. We see by these graphs that the internal representation generated by the neural network can be quite complex, even for a very simple, six-input system. Networks with many more inputs are correspondingly much more difficult to visualize.

Analysis 8

The remaining question is whether the network in fact learned individual signatures for each person in the dataset, or whether the results were perhaps due to the network focusing in on one individual's deviant data.

This was tested using the same network and parameters as in Analysis 7, except that the training dataset was comprised of 24 series (6 series for each of the four individuals), and 19 series for the transfer datasets. The chi-squared values shown in Table 3 are formed from a contingency table with three columns (high, baseline, and low aim) and four rows (four individuals). The contingency table was used to test the hypothesis that there were no significant differences among the four individuals in terms of how well the network was able to correctly identify the three outputs (i.e., high, baseline, low).

The number of correctly identified outputs in each of the three output conditions is shown in italics in Table 7. For example, in the *Training* table, the total possible number of correctly identified outputs for each aim condition was $N = 300$, calculated as $N = [6 \text{ series per aim per person}] * 50 \text{ training-transfer repetitions}$. The different number of possible outputs per person in the *Transfer* and *Random* datasets shown in Table 7 is due to the different numbers of series available to be tested in those datasets. Recall that an unbalanced number of data items per individual in the *Transfer* (or *Random*) dataset does not bias the outcome because the network is already trained before it is presented with this new data. (Obviously, if a network were changed in any way *after* being presented with new data, it would still be in the training mode.)

The overall percentage transfer rates are shown in bold in Table 7. The network was trained until the output error began to asymptote to a constant value.¹³ This occurred at about 3000 passes. At this point, the *Trained* network correctly identified an overall average of 88.2% of the input-output associations. The non-significant chi-squared value obtained with the *Training* dataset indicates that the network learned to identify aim (high, baseline, low) to approximately the same degree of accuracy (89.3%, 86.3%, 89.1%, respectively) regardless of which individual produced the data. Person 2's data, independent of the aim condition, was correctly identified most often (91.0%).

The slight bias in favor of Person 2 in the *Training* dataset is more evident in the *Transfer* results. The *Transfer* dataset chi-squared value of $\chi^2 = 60.863$ reveals that row (person) and column (aim) figures are not distributed as expected by chance. It seems that this non-chance distribution is largely due to the fact that high aim was correctly identified more often than the other two aims (39.9% vs. 35.4% overall), and Person 2's data was correctly identified more often than the other three people (45.3% vs. 35.4% overall).

The *Random* dataset is also not distributed by chance, but the overall transfer rate for this dataset (33.1%) is almost exactly that as expected by chance (i.e., 33.3%). Note that the aim condition most often identified correctly in the random dataset is the baseline condition. This makes sense because random data *should* look like a baseline aim to the neural network provided it was trained properly *and* that the baseline condition in the actual data resembled "genuinely" random data. The higher success rate in identifying the baseline condition seems to be responsible for the significant chi-square score.

In summary, Analysis 8 indicates that the *Trained* network was not significantly biased in favor of identifying any particular person or aim condition. The *Transfer* test, however, suggested that something about Person 2's performance made that person easier to correctly identify, and it was also easier to identify high and low aims, independent of who produced the data, compared to the baseline aim, as expected if high and low aims actually do introduce consistent patterns into the random data. The *Random* test showed that the baseline data was easiest to identify, as expected if the baseline data in the experiment is genuinely random.

TABLE 7

Contingency tables for correctly identified aims in Training, Transfer, and Random datasets. (See text for explanation.)

| <i>Training</i> | <i>High</i> | <i>Baseline</i> | <i>Low</i> | <i>Sum</i> | <i>Possible</i> | <i>Percent</i> |
|-----------------|-------------|-----------------|-------------|------------|-----------------|----------------|
| Person 1 | 274 | 251 | 254 | 779 | 900 | 86.6 |
| Person 2 | 280 | 271 | 268 | 819 | 900 | 91.0 |
| Person 3 | 262 | 276 | 275 | 813 | 900 | 90.3 |
| Person 4 | 255 | 238 | 272 | 765 | 900 | 85.0 |
| Sum | 1071 | 1036 | 1069 | 3176 | 3600 | 88.2 |
| Possible | 1200 | 1200 | 1200 | | | |
| Percent | 89.3 | 86.3 | 89.1 | | | |
| χ^2 (6 df) | 3.474 | p = .75 | | | | |
| <i>Transfer</i> | <i>High</i> | <i>Baseline</i> | <i>Low</i> | <i>Sum</i> | <i>Possible</i> | <i>Percent</i> |
| Person 1 | 64 | 80 | 65 | 209 | 600 | 34.8 |
| Person 2 | 165 | 124 | 119 | 408 | 900 | 45.3 |
| Person 3 | 29 | 56 | 48 | 133 | 450 | 29.6 |
| Person 4 | 121 | 34 | 103 | 258 | 900 | 28.7 |
| Sum | 379 | 294 | 335 | 1008 | 2850 | 35.4 |
| Possible | 950 | 950 | 950 | | | |
| Percent | 39.9 | 30.9 | 35.3 | | | |
| χ^2 (6 df) | 60.863 | p < .0001 | | | | |
| <i>Random</i> | <i>High</i> | <i>Baseline</i> | <i>Low</i> | <i>Sum</i> | <i>Possible</i> | <i>Percent</i> |
| Person 1 | 35 | 80 | 75 | 190 | 600 | 31.7 |
| Person 2 | 54 | 152 | 89 | 295 | 900 | 32.8 |
| Person 3 | 55 | 68 | 43 | 166 | 450 | 36.9 |
| Person 4 | 111 | 85 | 97 | 293 | 900 | 32.6 |
| Sum | 255 | 385 | 304 | 944 | 2850 | 33.1 |
| Possible | 950 | 950 | 950 | | | |
| Percent | 26.8 | 40.5 | 32.0 | | | |
| χ^2 (6 df) | 52.598 | p < .0001 | | | | |

Discussion

At this point, some readers may wonder why these results are important. It is essential to realize that the effect under consideration here is something that current theory cannot adequately explain: a person-specific correlation between human conscious intention and the output of a machine that is not physically connected to the experimental participants in any (known) way. The present analyses show that the interaction of intention with an RNG produces person-specific patterns in what should be (under the null hypothesis) purely random data.

Summary

The results of the above analyses may be summarized as follows:

- (a) Repeated presentation of RNG data produced by individuals mentally interacting with the RNG apparently improves a network's ability to identify these same individuals when the same trained network is presented with new data. In an earlier paper (Radin, 1989) I reported that presenting a network with a *single* half-series per person produced a 1.1% transfer rate above chance (i.e., 4.6% using actual data minus 3.5% using random data). In that same paper I showed that presenting *three* half-series from each of nine people produced a 4.3% transfer rate. The present study showed that presentation of *ten* full series from each of four individuals produced a 14.6% transfer rate;
- (b) A network can apparently learn to associate individuals' performance with the type of mental aim they employed and the type of RNG used to generate the data;
- (c) A network can learn the association described above, even under an experimental protocol where an entire series is generated with a single button press per aim condition;
- (d) A network's ability to determine intentional aim might be further enhanced by presenting multiple sources of information (i.e., many variables, not just repeated examples of the same variable) about an individual's performance. For example, compare the percentage of transfer obtained when using actual data in Analysis 4 vs. Analyses 2 and 3: 24.1% vs. 16.6% vs. 16.7%, respectively;
- (e) After a successful training process, a network is not necessarily biased to identify one person's data over another, but when a trained network is used to identify patterns in new data, some peoples' data and some mental aim conditions may be easier to identify than others.

Implications

A methodology which can detect person-specific mental intentions in data generated by a remote machine suggests the possibility of developing an array of novel human-machine interaction applications. An example of one applica-

tion might be called a mental lock, or mock. Mock would be an RNG – neural network system, which, upon sensing the presence of a person (using, say, conventional proximity detection circuitry), would initiate the generation of a sequence of random bits, then match the resulting data pattern against previously learned patterns. If a suitable match is found, the mental lock has recognized the identity of the proximal individual on the basis of his or her unique mental patterns which had been "impressed onto" the RNG data stream, and the mental lock would open. It may be that patterns associated with human consciousness are as unique, and therefore as useful for identification or security purposes, as retinal patterns and fingerprints.

Another application might be called a mind-controlled robot, or robomind. This would consist of a two-stage neural network system where the output of the first network was robotic control signals, and the input to that network was the output of a second network which was designed to detect mental influences on RNGs. It might be possible to train a two-stage (or perhaps a multi-stage) network to perform useful work under a combination of conventional and remote intentional control. One obvious use for robomind would be deep-space or deep-ocean exploration, where some degree of control over a remote, autonomous robot may be desired, but where it may be difficult or impossible to systematically or reliably employ normal telemetric control signals because of electromagnetic interference, attenuation of the signal, or possibly even relativistically-prohibitive distances (assuming that the mental intention effect is space-time independent). A less exotic use might be a robomind-wheelchair that a quadriplegic could control by intentional thought.

Conclusion

This study confirms previously reported results suggesting that an artificial neural network can learn to associate machine-generated random data with individuals who somehow "mentally impress" patterns onto that data. The present study also found that properly configured artificial neural networks can learn to associate data with specific mental intentions, demonstrating the feasibility of developing a new form of novel human-machine interaction technologies.

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Footnotes

¹ I thank Robert Jahn, Brenda Dunne, and Roger Nelson for providing a copy of this database.

² A standard UNIX System V™ 48-bit multiplicative congruential PRNG called drand48 was used to provide pseudorandom numbers in this study. This generator has passed extensive first- and higher-order randomness tests (Radin, 1985; Roberts, 1982).

³ I am indebted to Joseph Lubin for his technical assistance with these simulations and for many valuable discussions.

⁴ The number of hidden nodes in this (and succeeding analyses) was empirically determined to provide the best associative fit to the data. In general, the smaller the number of hidden nodes, the better the network is able to generalize what it learns.

⁵ The network learning rate establishes a tradeoff between how quickly the network learns vs. the precision with which it learns. Set too high, the network may initially learn quickly, but then never converge on an acceptable answer to the input/output problem. Set too low, it may take a very long time to converge on an answer. The learning rate typically ranges from 0 to 1; it was determined in this and the succeeding analyses empirically rather than by algorithmic means.

⁶ One "pass" is one training sweep through the network, during which the associative links among the nodes are slightly adjusted to correct for the differences between the obtained and the desired input/output associations. Networks must be trained with hundreds or thousands of such passes before the difference or error between desired and observed associations are acceptably low.

⁷ The original data was provided in an orderly fashion: data from subject 1, series 1, aim high, low, and baseline, then subject 1, series 2, aim high, low, and baseline, etc. However, a too-regular input order can bias a network to

learn "too much too fast" about the first few data items, to the detriment of being able to learn about later data items. To overcome this bias, the subject/data order was scrambled using the sorting method mentioned above. Of course, order of items *within* a row of data was untouched.

⁸ Excepting the baseline condition, of course, which had no explicit direction for intentional aim.

⁹ Actually, even if the training dataset *had* been unsymmetrically biased, useful information could still be gained about whether the network had learned the desired associations by comparing results of the transfer test against those of similarly generated control tests.

¹⁰ One might argue that the random dataset is simply a variation of the single button-press PEAR experiment. The difference is that here an entire *set* of *series* is created with a single button press. Of course, if one accepts the PEAR evidence suggesting that a single button-press experiment can produce significant departures from chance expectation, then our fundamental assumption about the *meaning* of a control dataset — as a condition outside the realm of experimental influence — is immediately suspect.

¹¹ I thank Joseph Lubin, Robert Jahn, Roger Nelson, Brenda Dunne, Alain Kornhauser, Angela Thompson, York Dobyns, Paul Rake and John Bradish for their valuable comments.

¹² Hereafter this is referred to as the "maximum coding scheme."

¹³ Strictly speaking, the error did not asymptote to a constant, but was exponentially approaching zero. The process was stopped after there were no changes in the error term in the third decimal place.

Applied Parapsychology Studies of Psychics and Healers

SYBO A. SCHOUTEN

*Psychology Laboratory, University of Utrecht, Heidelberglaan 2,
3584 CS Utrecht, The Netherlands
and
Dept. of Psychology
University of Edinburgh*

Abstract — Most research in parapsychology is aimed at understanding and controlling paranormal phenomena like telepathy, precognition and psychokinesis. However, in everyday life the practical applications of these phenomena, mainly by psychics and healers, play a more important role. People are generally more interested in the question of how effective these applications are than in scientific explanations of them. They want to know whether they should consult a psychic for a specific problem or what to expect from treatment by a paranormal healer. It is an important aspect of scientific activity to provide society with answers to such questions; answers not based on belief or disbelief in the paranormal but on factual research data.

Research with psychics dates back to the last century. Considering the complexity of the problem it is not surprising that it took many years before proper statistical evaluation methods were developed. Based on studies in which the paranormal impressions of psychics were quantitatively evaluated an assessment is given of what happens when clients consult a psychic and of the merit of the impressions on which the psychics base their advice.

Compared to psychics, there exists a much larger community of active paranormal healers. Despite this large number, the amount of research carried out on paranormal healing is less than the number of studies done with psychics. However, there are many studies available on complementary medicine in general which provide data relevant for the interpretation of the activities of psychic healers. All these data give an indication for the effectiveness of paranormal healing and of the main variables involved.

Two overview studies have been carried out, one on quantitatively evaluated studies with psychics, the other on studies on psychic healing and on complementary medicine. The present paper is a summary and overview of the main results of both studies.

Introduction

Although the origins of parapsychology lie in spontaneous paranormal experiences and in the activities of mediums and healers, parapsychology has developed mainly into an experimental field of research aimed at understanding and controlling the assumed paranormal process. The activities of psychics and healers can be considered as the applications of the paranormal in real-life situations. Psychics offer their help to clients for solving various problems, for instance to trace missing persons. They claim that their advice is based on

paranormal impressions. Such impressions would contain information which at the time is not known to the persons concerned. Psychic healers attempt to cure people from complaints and diseases, also apparently by paranormal means.

Experimental research in parapsychology, like Ganzfeld and RNG studies, appears reasonably successful (at least compared to the general rate of success in Social Sciences) and demonstrates that we might be on the trace of an anomalous phenomenon. For many interested in research in parapsychology the main question is whether such an anomalous phenomenon exists or not. It is usually taken for granted that if the existence of psi can be proven, that then the explanation for the achievements of psychics and healers is also obtained. This might be the reason that little research has been carried out in which the phenomena produced by psychics and healers were studied in order to derive explanatory hypotheses based on the characteristics of the phenomena themselves.

In everyday life the practical applications of the paranormal, the activities of psychics and healers, are of more importance than its possible scientific value. People are generally more interested in the question of how useful these applications can be for them than in their scientific explanation. They want to know whether they should consult a psychic for a specific problem or what they can expect from treatment by a paranormal healer. It is an important aspect of scientific activity to provide society with answers to such questions; answers not based on belief or disbelief in the paranormal but on factual research data.

But such answers are also important for scientists. If, for instance, it would be demonstrated that psychic healing is not effective at all, or only effective for specific classes of diseases, that certainly would have consequences for the applicability of certain explanations. There is also a practical point to consider. Research in parapsychology is hampered by the fact that the phenomena under study are weak and unpredictable. If psychics and healers are able to produce paranormal phenomena at will and on a regular basis then asking these people to take part in experimental research might greatly enhance its effectiveness.

For scientists who are interested in finding explanations for human behavior and achievements it makes sense to make a strict distinction between what people do or experience and the verbal explanations they themselves offer for these activities or experiences. If no additional evidence is given, then that explanation is at best to be considered as an hypothesis, based on the subjective insights of the person. In fact, very often there is no relationship at all between the behavior and the explanation, except that the same person is responsible for both.

Astrology is a case in point. The Gauquelin effect is an interesting phenomenon but can hardly serve as an explanation for what astrologers normally do: reading horoscopes for clients. When astrologers make statements to clients about a variety of topics such statements might well be more often correct than expected by chance. But that does not imply that the hypothesis the astrologer

offers, e.g. that reading a horoscope based on the moment of birth explains the correctness of the statements, has any merits. If computer programs are applied which simply follow astrological rules in a strict sense, then the resulting statements are generally very meager and of little interest. Still most research in astrology, and objections against the practice, focus on the astrological hypothesis instead of on the achievements of astrologers. A study aimed at the question of how the astrologer arrives at his statements might reveal that psychological processes play an important role in generating their contents. Therefore the study of astrology should rather start with a study of how astrologers arrive at their statements and whether these statements are really more often true than chance. Then, assuming there is anything left which needs explanation, the characteristics observed should provide the hypotheses for the most likely explanation. The usual tendency to focus research on the explanations and belief systems, instead of on the deduction of hypotheses from the characteristics of the phenomena itself, is quite remarkable and seems especially prominent in disputable areas. However, it is doubtful whether there is any justification for such an approach. The "pragmatic" approach which I will follow is to study the phenomena themselves and to look for explanations which fit their characteristics.

Past studies on psychics and healers nearly all follow the same tendency: a focus on the assumed paranormal character instead of on studying what psychics and healers do and accomplish. Hence important questions have been somewhat neglected, questions like how effective psychics and healers are, what factors are important in their achievements, and what these data suggest as to the most promising explanations. Therefore I carried out literature studies to collect as much data as possible on such questions. These overviews, one on quantitatively evaluated studies with psychics, the other on studies concerning psychic healing and complementary medicine, are published elsewhere (Schouten, 1993a, 1993b). The present paper, presented at the First European SSE Conference in August 1992, is a summary of the results of these overview studies.

Research with psychics dates back to the last century. Apart from a few recent exceptions, all studies were aimed at establishing the assumed paranormal character of the psychics' impressions. For lack of proper statistical methods this was initially done based on a more or less subjective evaluation of the material. Hundreds of pages in the older parapsychological journals have been devoted to descriptions of sessions with psychics and to discussions about the possible paranormal nature of the statements made. The development of quantitative methods to evaluate verbal statements from psychics only started in the 1930s. Considering the complexity of the problem it is not surprising that it took many years before proper statistical evaluation methods became available.

Compared to psychics, a much larger number of paranormal healers is active. Despite this large number, the amount of research carried out on paranormal healing has been substantially less than the number of studies done with

psychics. Nevertheless the available material provide some indications with regard to the effectiveness of paranormal healing. These data together with the findings from a number of studies on related fields of complementary medicine make it possible to draw conclusions about the value of psychic healing and about the factors which seem to contribute to the observed effects.

Research With Psychics

In the popular literature on the paranormal it is often stated that hundreds of well-documented cases exist which demonstrate that the paranormal impressions of psychics contributed to the solution of at the time unsolvable problems, for instance in cases of missing persons. Unfortunately, this is not true. It is true that occasionally psychics contribute to the solution of such cases, although not as frequently as many people believe, but there hardly exists a case which is well-documented. The reason is that when a psychic becomes involved in a case, for instance of a missing person, then normally there are so many interactions between the psychic and relatives, friends, and police, and in addition many activities of the psychic such as driving around in the environment, consulting maps, etc., which might yield information on the nature of the case, that it is impossible to keep track of all these activities, let alone to document them all. Often only the end result of this process is recorded: the statements which possibly have some relationship with the known outcome of the case.

The main advantage of experimental research is that in principle the activities of the psychic are under control and that all statements and verbal interactions can be recorded. Even then it is often not easy to assess the value of the statements. When a client interacts with a psychic and comments on the psychic's statements these comments can provide additional information which should be taken into account when evaluating subsequent statements. For quantitative analysis this constitutes such a problem that as a rule in experiments such comments, the feedback, are excluded. Therefore these experimental studies are not entirely representative for the conditions under which a psychic normally works. On the other hand, the elimination of information provided by the client does allow for a better test of a possible paranormal nature of the psychic's impressions.

The aim of a quantitative evaluation of the statements of a psychic is to establish whether these statements are more often correct than expected by chance. Although a quantitative evaluation is the best way to obtain an objective assessment of the value of the psychic's impressions, it provides only a limited view on the data. A qualitative analysis, based on a subjective interpretation of the material, might often be more meaningful. Relationships, for instance of an emotional nature, between apparently unrelated statements can provide a very apt characterization of the target person or of the situation, but might not lend themselves well to an objective quantitative analysis. However, a significant outcome of a quantitative analyses can be considered as a require-

ment which has to be fulfilled before a qualitative analyses is allowed. Thus only after it has been demonstrated quantitatively that the statements are especially applicable to the target person does further interpretation in a qualitative and psychological sense seem warranted.

About 40 reports on quantitatively evaluated studies with psychics were found, but the studies involved vary drastically in size. They range from the evaluation of one session to studies involving many years and hundreds of sessions with psychics. To provide an impression of the nature of these studies a short description of some of them follows. The studies described were chosen because they show the various and sometimes ingenious ways in which parapsychologists have tackled the psychic problem. No description is given of all the work and efforts spent to develop the statistical methods for analyzing verbal material which we have today. This can be found in the more extensive overview on which this paper is based (Schouten, 1992a).

J. H. Hyslop (1919) published the first attempt to provide a quantitative assessment of the statements of a psychic, in this case those of the famous Mrs. Piper. Hyslop was irritated by the objection that the medium's statements, although they seemed convincing, could be explained by a mixture of inference, guessing, and a little bit of luck. He intended to demonstrate the foolishness of that objection by running a control study. All statements of a session intended for the target person (Hyslop himself) were rephrased into 105 questions. For instance, if the psychic had stated that the target person had two sons, this statement would then be rephrased into "Do you have two sons?" These questions were sent to 1500 persons (420 returned the questionnaire) with the request to indicate which ones applied to the receiver's situation. This way Hyslop obtained a probability value for the expected correctness of each statement. Thus in the event that (out of 420) 42 persons would answer "yes" to a statement, the probability for the answer 'yes' to that statement would be rated as 1 in 10. Finally he just multiplied all probabilities obtained because, as he stated "I myself can answer all questions in the affirmative." Not surprisingly this way he obtained a very low probability for all statements combined, proving his point. A strong start, but unfortunately also the last experiment in which all statements of a psychic appeared to be correct.

This study illustrates the approach in which by means of control persons estimates are obtained for the probability of correctness of each separate statement. We know now that this approach suffers from many sources of error. An important one concerns the fact that persons who believe that they were the target person about whom the psychic made the statements are inclined to agree with the statements, whereas persons who know that they serve as control person are inclined to deny the correctness of the statements. A method which eliminates this bias in judgement was first applied by J. G. Pratt (1936). He asked the psychic Mrs. Garrett to give her impressions about a target person whom she did not meet and who stayed in an adjacent room. Fifteen sessions were held, each with a different target person. Pratt, who did not know the relationship between sessions and target person, transformed the medium's

verbal impressions of all sessions into separate statements. Then he requested all target persons to evaluate each statement on applicability to their own situation. Thus none of the subjects could know which statements were intended for them and which for the others. After receiving the answers the statements were again allocated to the subjects for which they were intended and for each subject the answers of the other subjects served as control data. The subsequent statistical analysis yielded a significant result. Hence this medium appeared to be able to provide more information than could be expected by chance. However, a replication study by C. V. C. Herbert (1937) with the same psychic and applying the same design failed to confirm this finding.

Pratt's method is a safeguard against the effect that people who believe the psychic's statements were intended for them actively search for events or circumstances in their life to match these statements. This bias can have a strong effect on the proportion of statements judged to be "correct." This was illustrated by D. Parson (1949) in a small study. He took the statements from a session of a psychic which by the target person were largely rated as very fitting to her situation. He found 4 other persons, matching the target person in sex and age, willing to take part in a "psychometric" experiment. A psychometric experiment means that the psychic uses an "inductor," an object belonging to the target person, as a means to obtain psychic impressions about that person. Hence the target person does not have to be present during the session. Parson added the statements of the original session to all four protocols and asked these persons, who did not know that statements from the original session not intended for them had been added, to rate all statements on applicability to their own situation. Considering only the added statements from the original session it appeared that two of the subjects also rated them as very fitting to their situation. For one of them the match appeared even better than the match had been for the real target person.

H. Bender in Germany became interested in the problem of the evaluation of psychics because of the achievements of the Dutch psychic G. Croiset. Croiset attained quite a reputation, among other reasons because of his occasional successes in tracing missing persons. Bender obtained the cooperation of Croiset and started a series of exploratory studies. One of these exploratory studies was the so-called Pirmasens series, in which Croiset made predictions about the person who would occupy a certain chair in a future lecture on parapsychology in Pirmasens. Bender was quite impressed by the subjectively evaluated results. He was amazed, as he expressed it himself, but, as he often stated, what was needed was a statistical assessment (Bender, 1957, 1984; Hoebens, 1984; Timm, 1984). Since these "chair tests" appeared to lend themselves well to quantitative evaluation, a series of experiments was started all involving chair tests. In a chair test the psychic makes statements about an unknown target person who in a future meeting will occupy a certain chair. Croiset made his statements in Holland, the meetings were held in Germany. A random procedure was applied to assign participants in the meeting to chairs.

These series of experiments turned out to be a continuous story of detecting and eliminating sources of error and of improving the statistical tools for analyzing the verbal material. The latter task was handled by the statistician U. Timm. In the period 1955 to 1966, 16 tests were carried out (Timm, 1965, 1966). Six of these tests yielded a positive significant effect; the significance of two tests is disputable (the significance disappeared depending on the type of controls applied) and eight tests yielded a clearly non-significant result. The overall conclusion was that Croiset's achievements were impressive enough to assume a non-chance effect on the data but that his contributions in terms of generating information about the target person were rather weak. In other words, even if one assumes a paranormal element the information he provided about the unknown target person appeared to have little value.

An interesting variation on this theme of the quantitative evaluation of impressions of psychics constitutes a line of research instigated by G. Schmeidler: the quantitative assessment of hauntings (Maher and Schmeidler, 1975). One of the first cases investigated by this method concerned an apartment where first a daughter, and later her mother, occasionally saw an apparition in certain places in the house. The emotional impact of seeing these apparitions was undoubtedly reinforced by the fact that only a few months before the father and spouse had passed away. The investigators reasoned that if these apparitions were really paranormal, then psychics might obtain impressions both regarding how the apparition looked as well as regarding the places it was seen. On the other hand, if the apparition was due to some sensorial illusion then one would expect that non-psychics would obtain similar experiences and hence would do as well as psychics in detecting the haunted spots.

So four psychics and eight skeptical non-psychics individually visited the apartment, accompanied by an uninformed co-experimenter and without the inhabitants present. They marked on a floor plan where they thought the apparition had appeared. The daughter and mother had indicated four haunted spots. In addition, a list was made up containing 37 characteristics of an apparition, of which 8 applied to this specific case. The results were not encouraging. One psychic was marginally-significantly correct in indicating the places, but wide off the mark as regards the characteristics of the apparition. Another psychic achieved the opposite results, a marginally significantly correct description, but none of the places were correctly identified. All other participants failed the test. Although the psychics did slightly better compared to the non-psychics, the difference is not impressive and might be entirely explained by the fact that psychics probably have more experience with cases of hauntings.

An original solution to solve the problem of the unreliable judgement of the correctness of statements by subjects was provided by Douglas Dean (1972). He also used precognitive statements, e.g. statements made by the psychic about future events but, unlike Bender, Dean asked subjects to formulate questions about unknown future events in their life, which were expected to take

place within 12 weeks, in such a way that they could be answered with yes or no. For instance, someone out of work looking for a job might ask: "Will I have a job on the day 12 weeks from now?" The psychic provided answers to all these questions (in total 285 provided by 94 subjects) based on her paranormal impressions. Dean also asked the subjects themselves to guess the answers. Then after the 12 weeks had elapsed subjects again provided the answers, but this time based on the factual situation.

Although the results cannot be evaluated assuming a $p = 0.5$ probability for each answer to be correct, it is possible to compare the psychic's predictions with those made by the subjects themselves. It turned out that the predictions of the psychic did not correlate with those of the subjects. Although one would expect the subjects, who are most familiar with their own situation, to be better able to predict their future that appeared not to be the case. The psychic won that contest and obtained a higher score on number of correct predictions. This in itself does not prove precognition, but it demonstrates why it is not surprising that there are people who become impressed by what psychics achieve.

Many cases are mentioned in popular literature in which a psychic apparently assisted police and relatives in tracing criminals or missing persons. As stated above, most of these accounts suffer from the unavoidable inaccuracy in presenting a complete picture of what actually happened. Thus although cases do exist in which psychics contributed to solving such problems, it is by no means certain that their contribution was the result of paranormal impressions. Experience and detailed knowledge of the circumstances has undoubtedly also played a role. An indication for the latter can be found in the difference in the degree of success between cases of missing persons and criminal cases. Cases of missing persons are rather restricted in possible outcomes and, at least in the Dutch situation, knowing certain details makes it often relatively easy to predict, for instance, that the missing person is probably drowned. On the other hand, crimes are often open-ended cases. If a person is abducted he might be kept or, in the case the victim was killed, be buried anywhere. Now one would not expect the fact that a missing person left voluntarily or that he was abducted to have a drastic effect on the paranormal impressions of psychics. However, it certainly does. Psychics are more successful in cases of non-crime-related missing persons than when crime is involved. The results of the following investigations illustrate this point.

In 1983 F. Heineken, head of the Heineken beer company, together with his chauffeur was abducted and held for ransom. The case was solved a few weeks later and the victims were found alive in an abandoned industrial area near Amsterdam. During these weeks over 100 paranormal impressions were received by the police in connection with this case. Eight of these contained sufficient specific information to take action, but none of these actions contributed to the solution. After the case was solved as a result of applying the usual police techniques, J. Neu (1985) analyzed the value of the supposedly paranormal impressions by comparing their content with the real data. He only found in three of them elements which resembled actual facts. However, when

it would have been possible during the case to select these three out of the other useless ones, the information contained in these impressions was so vague or irrelevant that they still would have been useless for assisting in the investigation.

An even more striking result was obtained in an abduction case which took place in 1989. This time the victim was G. J. Heijn, one of the directors of the biggest chain of food stores in The Netherlands. Again ransom money was demanded for his release. This case lasted much longer and a substantial reward was offered for information leading to the whereabouts of Heijn. No doubt both factors contributed to the much larger number of paranormal impressions received in this case, over 1500. Not only were impressions received, various psychics actively attempted to locate the hiding place of Heijn, using psychometric inductors which were put at their disposal by Heijn's family.

After this case was solved the outcome came as a surprise for everyone, including the police. Although money was handed over Heijn was never released. It turned out to be a single-handed operation in which the abductor had murdered Heijn shortly after the kidnapping. Because of the highly unusual pattern of the case it could have become a showcase for psychics proving everyone wrong. Not so. An ad-hoc evaluation of all these impressions and of the other psychic activities proved that none of them had even come close. In fact, it looks as if most of these impressions simply reflected the state of knowledge and speculation as presented in the media. Here again it was observed that even if one had known which impressions included some correct information, it would not have been of much use to the police (Gerding et al, 1989).

H. G. Boerenkamp, together with the present author, carried out an extensive and systematic study with a group of 15 psychics (Boerenkamp, 1988; Boerenkamp & Schouten, 1983). The study involved over 200 sessions in which psychics provided impressions about target persons unknown to them. Data were collected at the homes of the psychics and apart from constrictions imposed by the experimental conditions the psychics were free to behave as they were used to when interacting with clients. The same applied to the clients or investigators attending the sessions. Hence in a number of conditions feedback was allowed. Aims of the study were to describe what happens when a client consults a psychic; to evaluate the validity of the statements of psychics; and to investigate in a systematic way the effect of various conditions on the psychic's impressions. The evaluations were based on all statements made by the psychic and not only on the more interesting ones. Also for reason of comparison similar tests with matched groups of non-psychics were included.

In order to evaluate the enormous amount of statements obtained, over 10,000, a new method was devised. Normally in evaluations all statements are judged by all target persons. For this study that procedure was out of the question, not only because of the large number of statements but also because feedback was provided during sessions. The evaluation was split in two parts. First

the statements were judged by independent judges on their potential paranormal value. That value was based on the probability, given the information already available from the preceding interactions with the psychic, that the statement would be correct. Statements were also assigned a score for originality. Thus a statement on a topic which was never discussed before would obtain a high score for originality. Both scores were combined into one score for potential paranormal value. Subsequently only statements with sufficient potential paranormal value were retained for the final evaluation. By applying this "window" model, the number of statements was greatly reduced.

From the many interesting results of this study only a few are mentioned here. It turned out that the number of statements, or of paranormal impressions, is strongly correlated with the amount of externally provided information. The more information the psychic receives, for instance through feedback, the more impressions and statements result. The psychics were characterized by a personal style which hardly changed over the years that the experiment lasted. According to the external judges, only about 10% of the statements of the psychics could be considered to be of potential paranormal value. Of these statements on average again only 10% appeared to be correct. Hence of all the statements, roughly 1% is both specific and correct. This figure appeared not to differ from the results obtained by control groups of non-psychics. Thus, although psychics made many more statements compared to non-psychics, they did not do better than non-psychics.

Conclusions on Psychics

Considering the data from all studies in which mediumistic statements were quantitatively evaluated some conclusions can be drawn. The statistical tools at present available for assessing verbal statements seem quite adequate. However, it remains rather difficult to design a test which satisfies the conditions for proper statistical evaluation but at the same time does not restrict the psychic so much that the differences between real-life situations and experiment become too strong. For instance, most studies do not allow feedback from clients, despite the fact that feedback plays an important role in the psychics' daily practice. Also it is not easy to eliminate all the known sources of error. But it is questionable whether experimental designs which better match the normal working conditions of psychics would really yield more impressive results. The Boerenkamp study which did allow for feedback did not yield any evidence for better performance. It should also be considered that most experimental studies were done with the best psychics available at the time. Thus the achievements observed in these studies can be considered to be the best possible.

Most studies did not yield significant positive results. Of those who did a number of them, as for instance the Hyslop study, were not free of various sources of bias. Of course, it is thanks to all these researchers that we now know all the possible errors which can be made in such studies. Of more im-

portance is that even when significant outcomes were observed, the results have not been really impressive. Even with a "star" psychic like Croiset at least half of the studies are a failure and when he succeeded the effects were not really strongly significant. In particular the informational "gain" in the statements of psychics appear rather low. Even if one were able to sort out the correct from the incorrect statements there is little useful information to be found.

This result will be at variance with the picture many people have of psychics. There are a number of reasons for this discrepancy.

First, as discussed in the introduction, nearly all studies focussed on the paranormal aspect. Therefore experimental conditions were created which often are quite different from real-life situations. That Croiset did not do well in all his chair tests does not have to be representative of his real-life achievements as a psychic.

On the other hand, the picture of the real-life achievements of psychics is usually also exaggerated. A few successful cases are widely published and often cited in books on psychics, but the many unsuccessful ones are not reported or quickly forgotten. In experimental tests as a rule all statements are included in the evaluation. In real life the bulk of the psychic's activities and statements are never reported and therefore the successes are usually reduced to a simple story: the psychic "saw" what was unknown at the time, making the case look more impressive than the facts warrant.

A third important reason is that clients of psychics are often impressed by what the psychic seems to know about them. This reinforces their belief that psychics do possess some paranormal faculty and hence they assume that this faculty can also be applied in other situation, for instance in the case of the identification of a criminal. However, if the psychic's successes with clients were based on non-paranormal processes, then of course it becomes likely that different circumstances result in different achievements, depending on whether the circumstances allow the psychic to gain information from these non-paranormal processes. The data suggest that the latter is the case. Psychics as a rule fail in open-ended cases, score occasionally in cases like tracing missing persons, and are at their best in making statements about clients who are present and with whom they can interact.

The data, especially those related to possible sources of bias in experimental studies, make it possible to identify a number of processes which explain the success of psychics in sessions with clients. This interpretation is not only based on the data of the research with psychics but also on my own experience from the many sittings I have had with them.

An Interpretation of the Psychic's Practice

A reading is normally a verbal interaction between a psychic and a client. The client consults the psychic mostly for advice on a specific problem. However, the interaction is mainly about matters familiar to the client, which at first glance seems not very useful and contradictory to the psychic's claims to

be able to see the unknown. But this serves an important function because it allows the client to check the truth of the statements and consequently enables the psychic to establish his authority. There is another reason for spending so much time on matters the client already knows. It serves as an important source for generating feedback, that is, information from the client. This enables the psychic to form a picture of the background of the problem. Therefore most statements by the psychic are, as Boerenkamp called them, of a rhetorical or open-ended nature. They are not just statements but are formulated in such a way that they stimulate feedback.

The role of the client in this interaction is often underestimated. Generally the client is much more active than is assumed. Most clients turn to a psychic for advice about important problems and are strongly motivated to make the session a success. The better the psychic appears, the more faith the client can attach to the psychic's advice. Hence the client will really make an effort in trying to find a confirmation of the verifiable statements by searching for facts or interpretations that make them fit. One might say: A psychic is as able as the client allows! Often the psychic helps the client. If the client can't find a match the psychic might offer a different interpretation for the statement. Then the client starts the search process again.

I am convinced that in most cases neither psychic nor client is aware of their respective contributions and of the psychological processes involved. In addition, a number of other psychological processes contribute to the apparent success of a sitting. These are:

In general the psychic controls the encounter to a much larger extent than the client might be aware of, thus mainly selecting topics he feels comfortable with.

Many topics for readings concern basic needs or fears that almost all people experience. General statements in these areas which create belief or self-assurance are often considered as correct and as very applicable to the client's situation.

The appearance of the client tells the psychic a lot about the person.

Clients might not be aware of their non-verbal reactions to statements on topics which are highly emotional for them. The psychic might not be aware that he or she processes such non-verbal feedback.

There is the previously mentioned tendency of clients to rate statements as correct by actively searching for a match, not only because they want the session to succeed but also because of the common tendency to avoid disagreements and dissonance.

People tend to attach more value and to better remember correct statements than they remember the neutral or incorrect ones. This holds especially in the case where a search process finally yields a satisfactory interpretation. The search process itself and the initial false interpretations are quickly forgotten.

- People are poor at estimating probabilities and might underestimate the probability of the correctness of statements, especially in the case of statements of a general nature.
- A tendency exists to attribute a paranormal character to all statements made in the paranormal phase of the sitting, even simple ones to which in a normal conversation no importance would be attached.
- Most statements have a variety of possible interpretations. This not only strongly increases the probability of being correct but also creates a situation that occasionally the client affirms a statement based on an interpretation which might be different from what the psychic had intended.
- Clients might strongly underestimate the experience psychics have. Clients tend to assume that when a psychic tries to obtain paranormal impressions normal psychological processes, like the effect of experience, are excluded. This is certainly not the case because, among others, one can clearly see the effect of the feedback on subsequent statements which demonstrates that the normal functions of processing information are still in operation.
- To a client his or her own problems and situation are unique. But that does not apply to the psychic. Most questions and problems they are confronted with are rather common.
- An interaction is an active process from which only those elements will be remembered which are of special importance. For the client these are in the first place the impressions of the psychic. Hence clients are inclined to underestimate their own contribution to the interaction, and the comments and feedback they provided.

Most clients and psychics will not be aware of the contribution of the processes described above. Therefore it is not surprising that often both parties are honestly impressed by the results of a session. Psychics might occasionally have impressions which are difficult to explain and which could be considered paranormal. However, from all available data it can be concluded that if so, it plays a very minor role in the regular psychics' activities. A paranormal faculty does not have to be assumed to explain their success in sessions with clients.

It is of interest to compare the readings of psychics with spontaneous paranormal experiences of normal people. From my experience with both types of research I have the impression that spontaneous experiences are often more impressive than readings by psychics. Of course in the case of spontaneous experiences the less impressive and trivial occurrences might be reported less often. Still this seems to me insufficient to explain the difference. Boerenkamp's research suggests that psychics behave rather stereotypically. This was already noted by Saltmarsh (1929). I believe that acting as a psychic easily results in stereotyped behavior and therefore actually has a negative effect on whatever paranormal experiences or faculties they used to have.

From this discussion it should not be concluded that psychics are to be considered as dishonest or even frauds, or that it would be senseless to consult a psychic. The experience psychics might gain in their profession should not be underestimated. Some psychics who specialize in, for instance, tracing missing persons might be more experienced with such cases compared to the average police officer and they usually are able to spend a lot more time on it. Whatever the source of their statements, their experience and expertise in certain areas might make it worthwhile to consult them for advice. But the criteria by which to select a psychic seem to me rather the expertise and experience of the person with the type of problem at issue, rather than an estimate of his or her supposed paranormal sensitivity.

Psychic Healing and Complementary Medicine

Psychic healing is a term used for a very old healing practice which is also called, among others, mesmerism, spiritual healing, ritual healing, paranormal healing, mental healing, psionic medicine, non-contact therapeutic touch, and therapeutic intent. Treatment is based on non-intervention. All the healer does is to concentrate on the patient and to wish the patient to heal. Since the concept of bad health as an imbalance is widely held, healer's often make "passes," movements of the hands along the body, which is supposed to restore the balance and consequently to improve health. Another idea, central to the treatment, is that healers are able to paranormally transfer energy to the patient by which health is restored. Because this transfer is not limited by distance psychic healers also occasionally practice healing-at-a-distance. With distance-healing the patient remains at home and at pre-arranged times the healer concentrates on the patient and attempts to heal.

Psychic healing is a form of alternative or complementary healing, treatments which are not part of conventional scientific medicine as it is taught in the universities. Most people evaluate psychic healing on the merits of the explanation supplied by its practitioners, the assumed paranormal exchange of energies. Here again I prefer to apply the pragmatic approach, that is, to distinguish between what healers do, healing patients, rather than the explanation they offer for what they do, e.g. that the healing effect is due to some particular paranormal process. Thus mainly those aspects which are relevant for the healing process are studied. Most important among these are: does psychic healing really have an effect on patients?; if so, is the effectiveness related to variables such as type of complaint or personality characteristics?; why do people turn to complementary healers?; etc. In order to establish how specific such data are for psychic healing, data on complementary medicine in general have also been included. Since healers apply their abilities nearly exclusively to humans, only studies involving human subjects have been considered. Also all studies not dealing with healing but with ostensible psychokinetic effects on subjects have been left out. A discussion of such studies becomes relevant only if the characteristics of psychic healing clearly point to a paranormal explanation.

Because simple terms like "improvement" and "effect of the method" can be really confusing as to what exactly they mean, it seems useful to first specify them. For a patient, improvement means in general that the feeling of well-being and the ability to function have bettered. This is a subjectively experienced improvement. To conventional medicine improvement usually means a better rating on some objectively measured variable, for instance lower blood pressure readings. This will be denoted as objective improvement. It is generally assumed in conventional medicine that subjectively-experienced improvement is a consequence of objective improvement and that hence the two are highly correlated. That assumption seems to me quite questionable. For some diseases it will certainly hold, but for many others it does not. Hence unless a clear correlation can be proven it seems to me advisable to consider both criteria as independent indicators for the effect of a treatment. For most patients the subjectively-experienced state of health might even be the more important criterion of the two. Also, in the case no objective cause for the complaints can be found, the subjectively experienced state of health is the only criterion left for measuring the effect of a treatment.

For patients the effect of a treatment is mainly associated with the difference in state of health between onset and completion of treatment. This difference will be termed the effect of the treatment. To scientists, though, the effect of the treatment is normally operationalized as a difference between experimental and control or placebo groups. This will be called the effect of the method.

In the following some studies on psychic healing will be described in which both objective and subjectively experienced changes in health were assessed. Also data on the effectiveness of other types of complementary medicine will be presented. Another important source of information are studies on sociological aspects of complementary medicine, studies in which the satisfaction of the patients with different types of complementary treatments were also often measured.

Studies on Psychic Healing

Few experimental studies on the effect of psychic healing, such as those by J. T. M. Attevelt and D. P. Wirth, are available which fulfill basic requirements such as matched groups and a double-blind design.

In earlier survey studies Attevelt (1981, 1982, 1983, 1988) found that 80% of asthma patients treated by psychic healers reported some degree of improvement. In cooperation with a university medical department an experiment was carried out in which objective measures for change in asthmatic condition were also applied: spirometer and peak flow meter readings.

Patients were invited to take part, and based on diagnostic data taken after registration, 32 groups of three patients each were formed, matched by age, gender and medical conditions. Patients of each triplet were then randomly assigned to three conditions. Patients of the "optimal" conditions were treated by psychic healers according to the routines applied in their daily healing prac-

tices. Patients of the "distance" condition were treated by the same healers from behind a one-way screen. The healers participating in this study were all confident that effective psychic healing under such conditions was possible. The "control" condition was in all respects comparable to the "distance" condition, except that no healers were present behind the one-way screens and hence no psychic treatment was actually given. Patients of the "distance" and "control" conditions did not know to which condition they were assigned.

The design allowed one to distinguish between the effects of psychological factors associated with the patients, e.g. the degree of improvement of the "control" condition; a paranormal effect, e.g. the difference between improvement in "distance" and "control" conditions; and of psychological factors associated with the healer, e.g. the difference between "optimal" and "distance" conditions. Treatments lasted for a period of eight weeks, one treatment a week, because from previous surveys it was known that improvements could be expected in eight treatments. Both objective, on a blind basis, and subjective measurements were taken during the period of the study. The medical aspects were supervised by a physician.

Patients of the "optimal" condition improved significantly on peak flow measures. Improvement in subjectively-experienced state of health was reported by half of them. Two patients reported deterioration. Similar results, however, were observed for the other conditions and according to an analysis of variance the groups did not differ in degree of improvements, except for a trend that, subjectively experienced, the patients of the "optimal" group appeared more improved. Thus some positive effects were observed but largely due to psychological factors associated with patients and healers. Correlations between after-treatment measurements of peak flow meter and subjectively experienced health was significantly positive ($r=.32$). However, from the size it is clear that there still remains quite a discrepancy between objective and subjective measures of the medical condition (Attevelt, 1988).

A second study with a similar design involved patients suffering from hypertension (high blood pressure). This time each group involved 40 patients. To avoid a regression to the mean artefact the high blood pressure levels within each group varied from nearly normal to seriously high. The experiment involved 15 treatments, each lasting 20 minutes. This study involved a triple-blind design, because the statistical analysis was also performed without the statistician knowing the relationship between sets of data and conditions.

All groups showed a marked decline in blood pressure over the 15 weeks the experiment lasted. Mean blood pressure values for the "distance" conditions was systematically lower than those of the other conditions and a multivariate analysis showed a weak significant difference ($p < .05$) between groups. However, a direct comparison between "distance" and "control" condition yielded a non-significant result. Subjective improvement was reported by 83% of the subjects in the "optimal" conditions as opposed to about 40% for the other conditions. Hence it seems that the treatment had an effect mainly related to

psychological factors associated with the patients. Some evidence was found for the contribution of a paranormal factor, but the size of this effect was small compared to the contribution of the factors associated with the patients. Again a difference between subjectively experienced improvement between "optimal" and other conditions was observed, indicative of an effect of psychological variables associated with the healers. Interestingly patients who used medication and those who did not improved to the same degree. No significant correlation was found between blood pressure levels and subjectively experienced state of health (Attevelt et al, 1987, see also Beutler et. al, 1987, 1988).

The two Attevelt studies demonstrate that psychic healing can result in objective and subjective improvement in patients, but that these improvements are mainly due to psychological variables associated with the patients and, to a lesser extent, of psychological factors associated with the healers. The effect on the patients seems substantially larger than the effect of the method. The Wirth (1990) study involved not real patients but healthy subjects and the healing was directed at artificially induced dermal wounds. Similar experiments by F. Knowles, who first applied healing to induced wounds, had failed to demonstrate an effect but Wirth was more successful. Dermal wounds were inflicted on the arms of 44 subjects, after which each subject was randomly assigned to treatment and control group. The experiment involved 16 five-minute treatment sessions for the experimental group. Wound sizes were measured on a blind basis on days 8 and 16. On day 16 the proportion of healed versus non-healed subjects was significantly different indicating that Non-Contact Therapeutic Touch, as the authors labeled the treatment, can be effective.

Other studies with Therapeutic Touch, a term coined by D. Krieger, have yielded mixed results. Some found evidence suggestive of an effect of the method, others not. However, the quality of most of these studies does not seem optimal. Few applied a double-blind procedure. Especially the comparability of treatment and control groups with respect to variables which influence the dependent variable is often disputable. Since most studies involved relatively low numbers of subjects, just randomly assigning subjects to treatment and control groups is a risky procedure. Groups should be matched as regards the variables which influence the development of the illness. Assuming that the results have not been influenced by all such possible sources of bias, these studies suggest that occasionally an effect of the method occurs but that, as observed in the Attevelt studies, the strength of such effects is not impressive (see also Benor, 1990).

Some studies on psychic healing are available in which objective measures of health conditions were taken but which do not conform to the strict procedures normally applied in experimental research. Still they have yielded some surprising results. Back in 1949 the Dutch neurologist Musaph studied paralyzed patients treated by the psychic and healer Croiset. The combination of healer and psychic in one person, as with Croiset, is not unusual. Musaph did

observe various interesting reactions like sudden contractions in the paralyzed limbs. Then Musaph carried out several small experiments in which he found that these reactions were not associated with the moments the healer attempted healing but rather with the moments the patient expected the healer to concentrate on him. Musaph himself was able to induce similar reactions by different methods and concluded, that psychic healing was actually a form of psychotherapy (Musaph, 1949).

A similar but even more dramatic result was obtained by Rehder, the head of a German hospital clinic (Rehder, 1955). He had three seriously-ill patients for which conventional medicine apparently had done what it could do. So Rehder took the unusual step of asking a well-known psychic healer to apply healing-at-a-distance on these patients. The patients were not informed about this. No effect at all was observed. Then Rehder, like Musaph, started some experiments of his own. He took much effort to convince these patients of the tremendous healing powers of this healer, gave them books to read about the subject, and did everything to inspire confidence that this healer would cure them. Then he told the patients that healing attempts would start at a certain date and time, knowing that at the stated moments the healer, who this time was not informed, would not be active at all. All patients recovered rapidly and within a few months all were able to leave the hospital. According to Rehder, if nothing works then often belief still does, especially in patients who have suffered a long time and who are really desperate.

One of the most extensive earlier studies on objective and subjective effects of psychic healing has been reported by Inge Strauch, from Bender's institute in Germany (Strauch, 1958, 1963). During seven months in 1955 a well-known healer treated 650 patients in the institute. Data on medical history, psychological variables and subjectively-experienced changes in health were collected. Two physicians medically examined a subgroup of these patients in order to obtain data on objective changes in health due to the treatment. About 75% of the patients had suffered for more than five years from their complaint. Asked how satisfied these patients were with the conventional medical treatment they had had, 54% reported that it had helped them to some degree.

At the end of the study it appeared that 61% felt subjectively improved and 10% felt deteriorated. According to the physicians, objective improvement was observed in 11%, no change in 75% and deterioration in 14% of the patients. However, they had a somewhat unorthodox way to establish these figures. They rated the change in health not as a difference between objective measures before and after treatment, but as a difference between the objective changes observed and the objective changes they themselves expected for those patients. Hence the figures tell us more about how able they considered themselves to predict changes than that they are an indication for the results of the treatment. The correlation between subjective and objective improvements appeared very low.

The majority of the patients reported subjective sensations during treatment. This proved to be a good predictor for likelihood that the treatment would result in the patient feeling better. Personality variables did not distinguish between the "benefit" and "no benefit" patients.

The studies described above all involved some assessment of objective changes in health due to psychic healing. Not all studies are described here. It looks as if psychic healing does have an effect on the health of the patients. The effects seem much stronger for subjectively experienced state of health than for objectively measured health criteria. It appears very important that the patient knows that treatment is applied. The effect due to the method itself is weak or non-existent, whereas psychological variables associated with the patient and with the healer-patient interaction contribute most to the healing effects. In none of the studies were strong negative effects of the treatment observed.

Data on Effect Studies for Other Complementary Treatments

Recently some studies have been reported which present overview data on effect studies for homeopathy and acupuncture. Jacobs et. al (1991) have reviewed the literature on effectiveness of complementary treatments on rheumatism. Most studies involved special diets etc., not relevant for the present topic, but studies have also been included on the effect of acupuncture and of manual therapy on rheumatism. Of the methodologically acceptable studies involving acupuncture, nine did not and two did provide significant evidence for an effect of the method. For manual therapy the score was even. Two resulted in a positive effect, two did not. The authors conclude that most complementary treatments are not more effective than control or placebo treatment.

ter Riet et al (1990) did a similar literature survey on the effect of acupuncture on chronic pain. They found 51 controlled clinical trials. According to methodological criteria, for instance related to the application of double-blind and randomization procedures, studies were classified on a 100-point quality scale. On average the quality of the studies appeared rather mediocre and no study obtained more than 62 points. In over half of the studies no effect of the method was observed, and of the 24 studies in which a significant difference was observed only eight had a methodology score of 40 or higher. Considering the mixed results and especially the low overall quality the authors thought it wise not to draw any conclusions concerning the efficacy concerning acupuncture on chronic pain.

A similar overview study by Kleijnen et al (1991) concerned the effect of acupuncture on asthma. Thirteen studies were found but again the average quality was low. Eight studies reported a positive effect of the method, but only three of them obtained a methodology score of over 50. It appears that the success decreases with the quality of the studies. Hence it is concluded that the

efficacy of the method is not proven. A similar result was observed in an overview study of the effect of acupuncture as an aid to quit smoking.

The efficacy of homeopathy was studied by the same authors (Kleijnen et al., 1991). An exhaustive literature search revealed 107 controlled trials in 96 published reports. Again the above-described classification according to methodological criteria was followed. Although again the low-quality studies dominated, this time 22 studies achieved a score of 55 or higher and 16 studies a score of over 60. According to the authors of the studies, in 81 trials a positive effect of the method was demonstrated and in 24 trials not. However, in 42 trials the reporting was too inadequate to check the authors' claims. If only the 22 best studies are considered 15 trials showed a positive effect. In contrast to the studies described above the proportion of significant outcomes for the better quality studies is not significantly lower than for the lower quality studies. Thus, to their own surprise, the reviewers had to conclude that so far the evidence looks positive. However, the low methodological quality of most studies prevents drawing a definite conclusion.

In some of these overviews it was noted that since most studies concerned the effect of the treatment on subjective symptoms substantial improvement in the control groups could be expected. Hence it seems safe to assume that, as was observed in studies on psychic healing, with these treatments the psychological variables associated with the patients have a much stronger effect than the effect of the method itself. Another similarity between these overviews is that in some studies the effect of the method could be demonstrated, in others not. This indicates that none of these treatments exerts a strong effect due to the method itself. No indications were found for a trend that the experimental group would do worse than the control groups. Hence it appears that these methods do not have strong negative effects on the conditions of the patients.

Further Data on Psychic Healing and Complementary Medicine

A number of studies, mainly of a sociological nature, have provided data on various aspects of psychic healing and of complementary medicine in general. These aspects involve, among others, the number of people who turn to complementary practitioners, socio-economic data on these patients, the nature of the complaints for which treatment is sought, the improvement due to treatment, and possible negative effects. I will not discuss these studies here, since they represent the bulk of the overview presented elsewhere (Schouten, 1992b), but will summarize the results. Most of these studies are from Western Europe.

Number of People Who Consult Complementary Practitioners

Averaging the data from 15 studies it appears that 22% of the population has at least once tried a complementary treatment. Not surprisingly this percentage increases substantially to an average of 65% when only patients are considered. About 80% to 90% of the patients first consult a conventional practition-

er and most patients who turn to complementary medicine keep in touch with their general practitioner or specialist. Hence patients clearly do not view these treatments as an alternative to conventional treatments but as complementary. This finding has been the reason that throughout this paper the term "complementary medicine" has been used. A positive correlation is observed between the use of conventional and complementary medicine. Patients who consult complementary practitioners are above-average users of the entire health system. This seems mainly a consequence of the long illness histories these patients have. From these data it is clear that complementary medicine cannot be considered as a potential competitor for conventional medicine. This implies also, that legalizing complementary medicine will probably not reduce the costs of the health care system, as some of its proponents argue.

Characteristics of Users of Complementary Medicine

Females constitute on average two-thirds of the patients of complementary medicine. Most patients are between 40 and 60 years of age whereas the older age group of over 60 makes relatively less use of complementary treatments. Socio-economic and educational levels are on general higher compared to patients who do not use complementary medicine, although that might not be true for patients of psychic healers. Most patients suffer from chronic complaints. For instance, in the Attevelt survey study it was found that the average duration of the complaint for patients of psychic healers was over seven years.

Nature of the Complaints

Complementary treatment is sought for all sorts of complaints, from simple colds to cancer, but as a rule all patients are still mobile and able to visit the healer. The musculo-skeletal problems like rheumatism dominate with on average 32%, followed by psychic disorders of all kinds. Chronic pain as a symptom is mentioned in about half the cases. In general there appears little relationship between type of complaint and type of complementary medicine.

Motivation to Use Complementary Medicine

Interestingly, none of the characteristics usually associated with patients of healers, such as a preponderance of females or of middle-aged patients, seem typical for users of complementary medicine. This follows from a few studies, especially the one by van Sonsbeek (1983), in which multivariate analysis was applied on background data, collected from a large national sample, of patients of complementary practitioners and patients who did not use complementary treatments. The two groups of patients did not differ at all. Only a tendency was observed that patients of complementary practitioners have a somewhat worse state of health. Few patients appear to turn to complementary medicine for principal reasons, for instance because they believe in the philosophy behind the treatment, or because they were dissatisfied by the communi-

cation and relationship with their conventional practitioners. There is mainly one pragmatic reason to turn to complementary medicine, and that is that the conventional practitioner did not solve the ailment they suffered from.

Improvement Due to Complementary Treatment

From 25 studies figures are available on subjectively experienced degree of improvement. On average 65% (median = 70, range 17% - 91%) of the patients report improvement. Ten studies also provided figures on deterioration. Here the average turned out to be 8% (range 1% - 21%). For serious diseases like cancer or for patients with an extremely long illness history benefit percentages are lowest. The subjectively experienced improvement seems mainly related to either a reduction in seriousness of the symptoms, for instance pain, or to an increased ability to cope with such symptoms.

It is of importance to note that the data suggest that as regards the effectiveness figures not much difference exists between the various types of complaints nor between the various types of complementary medicine. Only the seriousness and duration of the disease seem to have an effect on the benefit rate.

Negative Effects of Complementary Treatment

There is little indication that complementary treatment as such could have a bad effect on patients. In so far as negative effects have been observed, they appear a consequence of the patient either delaying seeing a conventional practitioner or terminating medication without first asking their doctor. Hence potential harmful effects of complementary treatment can be avoided if care is taken that the patient continues to consult conventional practitioners.

Main Conclusions From All Data

Despite the sometimes not optimal quality of the studies I think that the data allow us to draw some tentative conclusions. It is apparently important that the patient knows that healing is attempted. Although this clearly appears the case for psychic healing, I suspect that this also applies to the other types of complementary medicine. In most studies the effects on objectively measured criteria have not been impressive and if objective changes occurred, they often also occurred in the patients of the control group. The changes in experimental and control groups are in general larger than the difference in changes between these groups. In fact, in many studies no effect of the method, a difference between experimental and control groups, was noticeable. Complementary medicine clearly has the strongest effects on subjectively experienced states of health and variables associated with the patients seem to have the strongest influence. From the available data it appears that as regards effectiveness not much difference exists between either the type of complaint treated or between the various complementary methods. This suggests that the effects of all these different methods, despite the different philosophies on which they are based,

are largely due to the same processes. It should be noted though that in only a few studies was the effectiveness of different types of complementary medicine directly compared.

An Interpretation

Because the effect of the method appears small or non-existent in complementary medicine including psychic healing, it follows that the concept of the paranormal cannot really provide an explanation for psychic healing. The data indicate that the healing effects in all forms of complementary medicine are mainly related to variables associated with the patients and, to a lesser extent, psychological variables associated with the healer. No indications were found of a relationship between effectiveness and type of treatment or type of complaint (apart from seriousness of complaint). It is therefore more likely that the same explanation holds for all of these different complementary treatments and that the methods themselves contribute only little to the total effect of each treatment.

The concept of placebo is familiar in the health sciences and, according to Fields and Levine (1981), probably accounts for more subjective improvement than any single category of active drug. Placebo response is inherent to all healing situations and hence also in complementary treatment. Still there are some differences, among others as regards effect size, which suggest that the effects of complementary treatment are not entirely synonymous to placebo effects. Or perhaps the conditions under which complementary treatment is given are extremely favorable to generate placebo responses. However, since the placebo response itself is still not well explained I do not think there is much to be gained by calling the results of complementary treatment just "placebo" or "suggestion." That would merely mean replacing one term with another and would contribute little to an explanation of the phenomenon.

From the material used for this overview on complementary medicine I believe a number of processes can be deduced which contribute to the reported effects of complementary medicine.

Often the treatment explicitly focuses on the ailing part of the body. It has been demonstrated that this occasionally results in physiological reactions such as an increased blood circulation or contractions. This might contribute to an improvement. Also treatment might have other effects like profound relaxation which could have a beneficial effect on the ailment, as for instance in the case of high blood pressure patients. Conventional medicine assumes that the subjectively experienced health is a consequence of the objective health condition. Therefore conventional medicine cannot deal well with situations in which the opposite would hold, e. g. when the illness might be influenced by psychological factors. Part of the effect of complementary treatment might be an increased ability of the patient to cope with the illness and its symptoms which could then result in objective improvement.

Patients probably have a different expectation from complementary treatment than from conventional medicine. Conventional medicine presents a strong image of itself and consequently patients often expect conventional medicine to cure them. Complementary practitioners do not have such an image and thus the expectation patients have from these treatments is on average lower. They just expect, or hope, to improve. Consequently satisfaction will be easier to achieve for complementary practitioners. This holds especially when the complementary practitioner accepts a supportive rather than an authoritarian role. In addition especially chronic patients turn to complementary medicine. These patients will often feel rather desperate and hence are perhaps even less critical and less demanding than other patients when it concerns the expected results of complementary treatments.

Only few patients turn to complementary medicine for ideological reasons, that is, because they accept the belief system on which the treatment is based. Most come mainly for purely pragmatic reasons. That makes it easier for these patients to try different types of complementary treatment. But also within one type of complementary medicine it is easy to change from one practitioner to another. I believe that especially this possibility to search is very important and that it eventually contributes strongly to the effects of complementary medicine.

One advantage of this search process is that it allows the patient each time when needed to take action about his problem and to try something new. In conventional practice it is often difficult to change one's general practitioner or specialist. If no success is achieved the patient soon reaches a dead end. With complementary medicine there is always the possibility to try a new remedy. Even in conventional medicine there are many examples that new remedies can have a much stronger positive effect than, in hindsight, can be explained by the effect of the method.

Another advantage is that the search process allows the patient to search until a healer is found which psychologically is an optimal match for the patient. This might explain why most healers have a circle of patients who are really devoted to them. In such circumstances the interaction between healer and healee can be quite effective.

Thirdly this search process allows the healer to take advantage of the natural ups and downs in the subjectively experienced state of health. Normally an up situation will not get a special meaning if there are no changes in treatment. But when searching, an up situation might coincide with the onset of a new treatment; thus the confidence of the patient in a successful result might be greatly enhanced.

- An essential part of most complementary treatments is the holistic approach and the associated stress on a more healthy life style. In conventional treatments similar advice is given but often as supplementary to, for instance, medication. But in complementary medicine such advice is

often central to the treatment and therefore the patient might make a more serious effort to comply with it.

The methods applied in complementary treatment appear to have little effect themselves and therefore are rather harmless. But as a consequence patients using these methods adopt a more normal lifestyle without medication or other intervening mechanisms. Leaving the body to fight on its own for a while might create better conditions for the start of a self-healing process and for the buildup of improved resistance.

I am afraid that also occasionally a patient is simply fooled by a complementary practitioner in that the healer diagnoses an illness from which in fact the patient does not suffer, or which is much less serious than the patient is made to believe. Such diseases are of course easy to cure. I suspect that diagnoses of that kind are sometimes the explanation of the occasional stories about miraculous healing performances.

The tentative conclusion that the effectiveness of the different complementary treatments is probably due to the same processes rests partly on the finding that the effectiveness of all these treatments seems more or less equal. However, this finding rests on the data of only a few studies. Further data on this issue are needed and might prove that the above mentioned conclusion has to be revised.

It appears to me that complementary medicine deserves a place in the health care system, although a modest one. It can have positive effects especially on the subjectively experienced state of health and therefore seems better able to deal with complaints for which no objective reason can be diagnosed. However, it should be clear that conventional medicine remains superior for all diseases and complaints which can be objectively diagnosed and for which a scientifically proven remedy is developed. Negative effects from complementary treatment can largely be reduced by ensuring that the patients remain under supervision of conventional medicine. Unfortunately the sometimes strong emotional rejection by conventional practitioners might make it difficult for the responsible complementary healer to check whether the patient is really doing so.

The acceptance of complementary medicine, and the incorporation of its positive aspects into conventional medical practice, might become much easier if state of health could be considered as a multi-dimensional concept in which objectively measured state of health and subjectively experienced state of health are seen as two basically independent components. If conventional treatment has a positive effect on both dimensions so much the better. However, from a pragmatic point of view there is nothing amiss with treatments which, when other methods fail or can't be applied, make people feel and function better and which in themselves do not have any negative side-effects.

Applied Parapsychology

From both overview studies it appears that psychics and healers can be successful for clients, although in the case of psychics the informational value of

their impressions is very limited; but the characteristics of the phenomena point to a different explanation for these achievements than the belief system behind these practices suggests. Unexplainable events might occasionally happen suggestive of a paranormal element, but the main achievements seem due to different processes. This explains perhaps why psychics and healers as subjects have hardly contributed to recent successful developments in parapsychology such as Ganzfeld and RNG research.

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Birthmarks and Birth Defects Corresponding to Wounds on Deceased Persons

IAN STEVENSON

*Department of Psychiatric Medicine, University of Virginia, School of Medicine,
Charlottesville, Virginia 22908*

Abstract — Almost nothing is known about why pigmented birthmarks (moles or nevi) occur in particular locations of the skin. The causes of most birth defects are also unknown. About 35% of children who claim to remember previous lives have birthmarks and/or birth defects that they (or adult informants) attribute to wounds on a person whose life the child remembers. The cases of 210 such children have been investigated. The birthmarks were usually areas of hairless, puckered skin; some were areas of little or no pigmentation (hypopigmented macules); others were areas of increased pigmentation (hyperpigmented nevi). The birth defects were nearly always of rare types. In cases in which a deceased person was identified the details of whose life unmistakably matched the child's statements, a close correspondence was nearly always found between the birthmarks and/or birth defects on the child and the wounds on the deceased person. In 43 of 49 cases in which a medical document (usually a postmortem report) was obtained, it confirmed the correspondence between wounds and birthmarks (or birth defects). There is little evidence that parents and other informants imposed a false identity on the child in order to explain the child's birthmark or birth defect. Some paranormal process seems required to account for at least some of the details of these cases, including the birthmarks and birth defects.

Key words: Birthmarks, birth defects, paranormal processes, teratology

Introduction

Although counts of moles (hyperpigmented nevi) have shown that the average adult has between 15 and 18 of them (Pack and Davis, 1956), little is known about their cause — except for those associated with the genetic disease neurofibromatosis — and even less is known about why birthmarks occur in one location of the body instead of in another. In a few instances a genetic factor has been plausibly suggested for the location of nevi (Cockayne, 1933; Denaro, 1944; Maruri, 1961); but the cause of the location of most birthmarks remains unknown. The causes of many, perhaps most, birth defects remain similarly unknown. In large series of birth defects in which investigators have searched for the known causes, such as chemical teratogens (like thalidomide), viral infections, and genetic factors, between 43% (Nelson and

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Holmes, 1989) and 65 — 70% (Wilson, 1973) of cases have finally been assigned to the category of "unknown causes."

Among 895 cases of children who claimed to remember a previous life (or were thought by adults to have had a previous life), birthmarks and/or birth defects attributed to the previous life were reported in 309 (35%) of the subjects. The birthmark or birth defect of the child was said to correspond to a wound (usually fatal) or other mark on the deceased person whose life the child said it remembered. This paper reports an inquiry into the validity of such claims. With my associates I have now carried the investigation of 210 such cases to a stage where I can report their details in a forthcoming book (Stevenson, forthcoming). This article summarizes our findings.

Children who claim to remember previous lives have been found in every part of the world where they have been looked for (Stevenson, 1983; 1987), but they are found most easily in the countries of South Asia. Typically, such a child begins to speak about a previous life almost as soon as it can speak, usually between the ages of two and three; and typically it stops doing so between the ages of five and seven (Cook, Pasricha, Samararatne, Win Maung, and Stevenson, 1983). Although some of the children make only vague statements, others give details of names and events that permit identifying a person whose life and death corresponds to the child's statements. In some instances the person identified is already known to the child's family, but in many cases this is not so. In addition to making verifiable statements about a deceased person, many of the children show behavior (such as a phobia) that is unusual in their family but found to correspond to behavior shown by the deceased person concerned or conjecturable for him (Stevenson, 1987; 1990).

Although some of the birthmarks occurring on these children are "ordinary" hyperpigmented nevi (moles) of which every adult has some (Pack and Davis, 1956), most are not. Instead, they are more likely to be puckered and scarlike, sometimes depressed a little below the surrounding skin, areas of hairlessness, areas of markedly diminished pigmentation (hypopigmented macules), or port-wine stains (*nevi flammei*). When a relevant birthmark is a hyperpigmented nevus, it is nearly always larger in area than the "ordinary" hyperpigmented nevus. Similarly, the birth defects in these cases are of unusual types and rarely correspond to any of the "recognizable patterns of human malformation" (Smith, 1982).

Methods

My investigations of these cases included interviews, often repeated, with the subject and with several or many other informants for both families. With rare exceptions, only firsthand informants were interviewed. All pertinent written records that existed, particularly death certificates and postmortem reports, were sought and examined. In the cases in which the informants said that the two families had no previous acquaintance, I made every effort to exclude all possibility that some information might nevertheless have passed normally to the child, perhaps through a half-forgotten mutual acquaintance of the two



Fig. 1. Hypopigmented macule on chest of an Indian youth who, as a child, said he remembered the life of a man, Maha Ram, who was killed with a shotgun fired at close range.

families. I have published elsewhere full details about methods (Stevenson, 1975; 1987).

I did not accept any indicated mark as a birthmark unless a firsthand witness assured me that it had been noticed immediately after the child's birth or, at most, within a few weeks. I enquired about the occurrence of similar birthmarks in other members of the family; in nearly every instance this was denied, but in seven cases a genetic factor could not be excluded.

Birth defects of the kind in question here would be noticed immediately after the child's birth. Inquiries in these cases excluded (again with rare exceptions) the known causes of birth defects, such as close biological relationship of the parents (consanguinity), viral infections in the subject's mother during her pregnancy, and chemical causes of birth defects like alcohol.

Results

Correspondences between Wounds and Birthmarks

A correspondence between birthmark and wound was judged satisfactory if the birthmark and wound were both within an area of 10 square centimeters at the same anatomical location; in fact, many of the birthmarks and wounds were much closer to the same location than this. A medical document, usually a postmortem report, was obtained in 49 cases. The correspondence between wound and birthmark was judged satisfactory or better by the mentioned criterion in 43 (88%) of these cases and not satisfactory in 6 cases. Several different

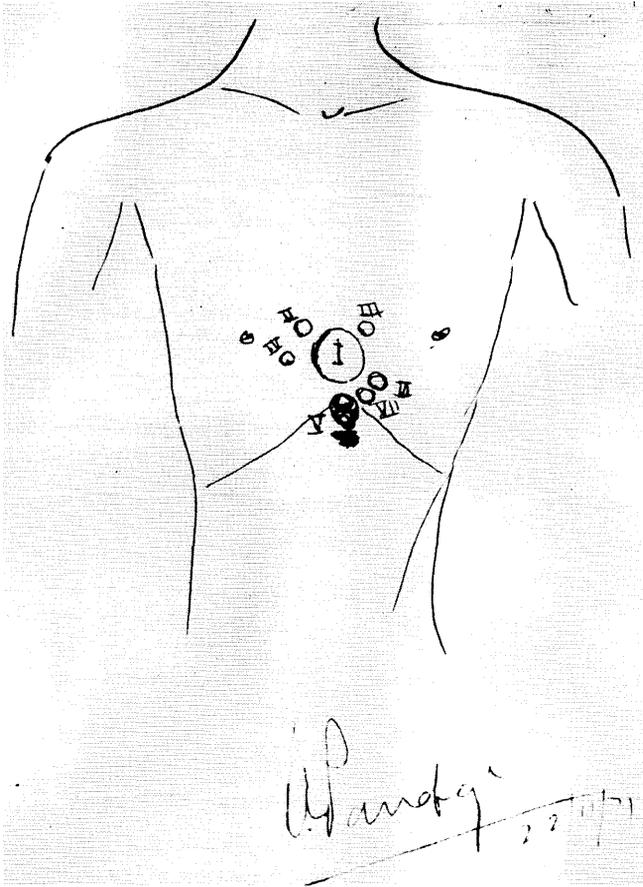


Fig. 2. The circles show the principal shotgun wounds on Maha Ram, for comparison with Figure 1.

explanations seem to be required to account for the discrepant cases, and I discuss these elsewhere (Stevenson, forthcoming). Figure 1 shows a birthmark (an area of hypopigmentation) on an Indian child who said he remembered the life of a man who had been killed with a shotgun fired at close range. Figure 2 shows the location of the wounds recorded by the pathologist. (The circles were drawn by an Indian physician who studied the postmortem report with me.)

The high proportion (88%) of concordance between wounds and birthmarks in the cases for which we obtained postmortem reports (or other confirming documents) increases confidence in the accuracy of informants' memories concerning the wounds on the deceased person in those more numerous cases for which we could obtain no medical document. Not all errors of informants' memories would have resulted in attributing a correspondence between birthmarks and wounds that did not exist; in four cases (possibly five) reliance on



Fig. 3. Large verrucous epidermal nevus on head of a Thai man who as a child said he remembered the life of his paternal uncle, who was killed with a blow on the head from a heavy knife.

an informant's memory would have resulted in missing a correspondence to which a medical document attested.

Cases with Two or More Birthmarks

The argument of chance as accounting for the correspondence between birthmarks and wounds becomes much reduced when the child has two or more birthmarks each corresponding to a wound on the deceased person whose life he claims to remember. Figure 3 shows a major abnormality of the skin (verrucous epidermal nevus) on the back of the head of a Thai man who, as a child, recalled the life of his uncle, who had been struck on the head with a heavy knife and killed almost instantly. The subject also had a deformed toe-



Fig. 4. Congenital malformation of nail on right great toe of the Thai subject shown in Figure 3. This malformation corresponded to a chronic ulcer of the right great toe from which the subject's uncle had suffered.

nail of the right great toe (Figure 4). This corresponded to a chronic infection of the same toe from which the subject's uncle had suffered for some years before he died.

The series includes 18 cases in which two birthmarks on a subject corresponded to gunshot wounds of entry and exit. In 14 of these one birthmark was larger than the other, and in 9 of these 14 the evidence clearly showed that the smaller birthmark (usually round) corresponded to the wound of entry and the larger one (usually irregular in shape) corresponded to the wound of exit. These observations accord with the fact that bullet wounds of exit are nearly always larger than wounds of entry (Fatteh, 1976; Gordon and Shapiro, 1982). Figure 5 shows a small round birthmark on the back of the head of a Thai boy, and Figure 6 shows a larger, irregularly shaped birthmark at the front of his head. The boy said that he remembered the life of a man who was shot in the head from behind. (The mode of death was verified, but no medical document was obtainable.) In addition to the 9 cases I have investigated myself, Mills re-



Fig. 5. Small, round puckered birthmark on a Thai boy that corresponded to the bullet wound of entry in a man whose life he said he remembered and who had been shot with a rifle from behind.

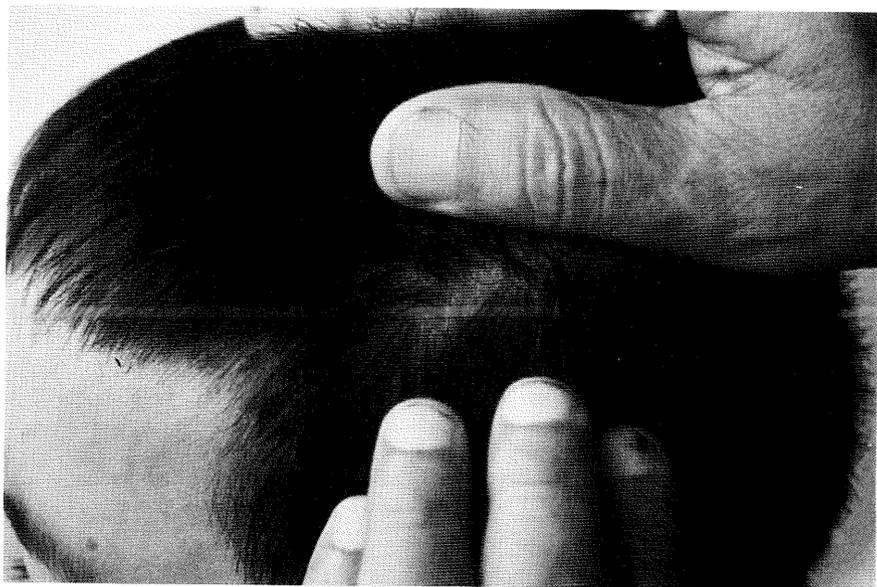


Fig. 6. Larger, irregularly shaped birthmark on the frontal area of the head of the Thai boy shown in Figure 5. This birthmark corresponded to the bullet wound of exit on the Thai man whose life the boy said he remembered.

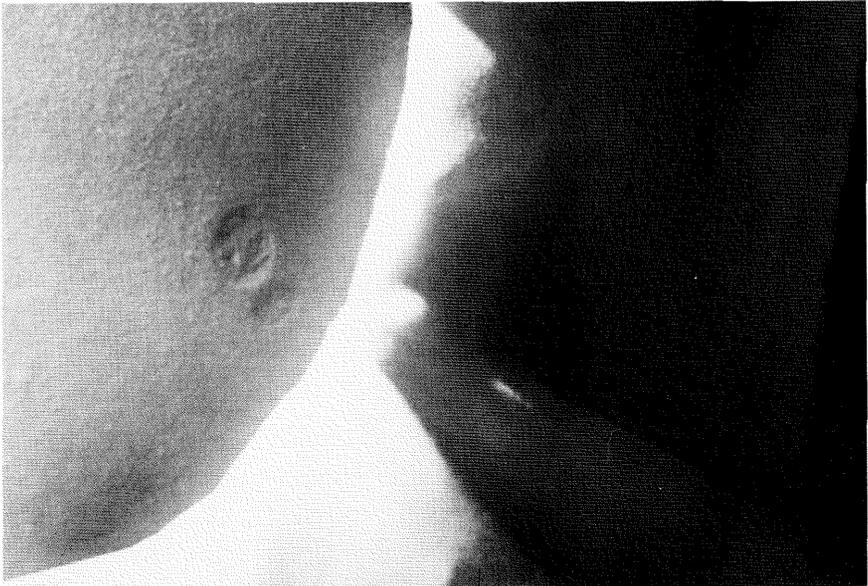


Fig. 7. Two round, puckered, scarlike birthmarks of different sizes on the left breast of a Burmese woman who as a child said she remembered the life of a woman who was fatally wounded by a shotgun that used a cartridge containing *shot of different sizes*.

ported another case having the feature of a small round birthmark (corresponding to the wound of entry) and a larger birthmark corresponding to the wound of exit (both verified by a postmortem report) (Mills, 1989).

I have calculated the odds against chance of two birthmarks correctly corresponding to two wounds. The surface area of the skin of the average adult male is 1.6 meters (Spalteholz, 1943). If we were to imagine this area square and spread on a flat surface, its dimensions would be approximately 127 centimeters by 127 centimeters. Into this area would fit approximately 160 squares of the size 10 centimeters square that I mentioned above. The probability that a single birthmark on a person would correspond in location to a wound within the area of any of the 160 smaller squares is only 1/160. However, the probability of correspondences between two birthmarks and two wounds would be $(1/160)^2$ i.e. 1 in 25,600. (This calculation assumes that birthmarks are uniformly distributed over all regions of the skin. This is incorrect [Pack, Lenson, and Gerber, 1952], but I believe the variation can be ignored for the present purpose.)

Examples of Other Correspondences of Detail between Wounds and Birthmarks

A Thai woman had three separate linear hypopigmented scarlike birthmarks near the midline of her back; as a child she had remembered the life of a woman who was killed when struck three times in the back with an ax. (Informants verified this mode of death, but no medical record was obtainable.) A woman of Burma was born with two perfectly round birthmarks in her left

chest (Figure 7); they slightly overlapped, and one was about half the size of the other. As a child she said that she remembered the life of a woman who was accidentally shot and killed with a shotgun. A responsible informant said the shotgun cartridge had contained shot of two different sizes. (No medical record was obtainable in this case.) Another Burmese child said that she remembered the life of her deceased aunt, who had died during surgery for congenital heart disease. This child had a long, vertical linear hypopigmented birthmark close to the midline of her lower chest and upper abdomen; this birthmark corresponded to the surgical incision for the repair of the aunt's heart. (I obtained a medical record in this case.) In contrast, a child of Turkey had a horizontal linear birthmark across the right upper quadrant of his abdomen. It resembled the scar of a surgeon's transverse abdominal incision. The child said that he remembered the life of his paternal grandfather, who had become jaundiced and was operated on before he died. He may have had a cancer of the head of the pancreas, but I could not learn a precise medical diagnosis. Two Burmese subjects remembered as children the lives of persons who had died after being bitten by venomous snakes, and the birthmarks of each corresponded to therapeutic incisions made at the sites of the snakebites on the persons whose lives they remembered. Another Burmese subject also said as a child that she remembered the life of a child who had been bitten on the foot by a snake and died. In this case, however, the child's uncle had applied a burning cheroot to the site of the bite — a folk remedy for snakebite in parts of Burma; and the subject's birthmark was round and located at the site on the foot where the bitten child's uncle had applied the cheroot.

Three Examples of Birth Defects

Figure 8 shows the right side of the head of a Turkish boy with a diminished and malformed ear (unilateral microtia). He also had underdevelopment of the right side of his face (hemifacial microsomia). He said that he remembered the life of a man who had been shot (with a shotgun) at point-blank range. The wounded man was taken to a hospital where he died 6 days later — of injuries to the brain caused by shot that had penetrated the right side of the skull. (I obtained a copy of the hospital record.)

Figure 9 shows fingers almost absent congenitally on one hand (unilateral brachydactyly) in a child of India who said he remembered the life of another child who had put his right hand into the blades of a fodder-chopping machine and lost his fingers. Most cases of brachydactyly involve only a shortening of the middle phalanges. In the present case there were no phalangeal bones, and the fingers were represented by mere stubs. Unilateral brachydactyly is exceedingly rare, and I have not found a published report of a case, although a colleague (plastic surgeon) has shown me a photograph of one case that came under his care.

Figure 10 shows congenital absence of the lower right leg (unilateral hemimelia) in a Burmese girl. She said that she remembered the life of a girl

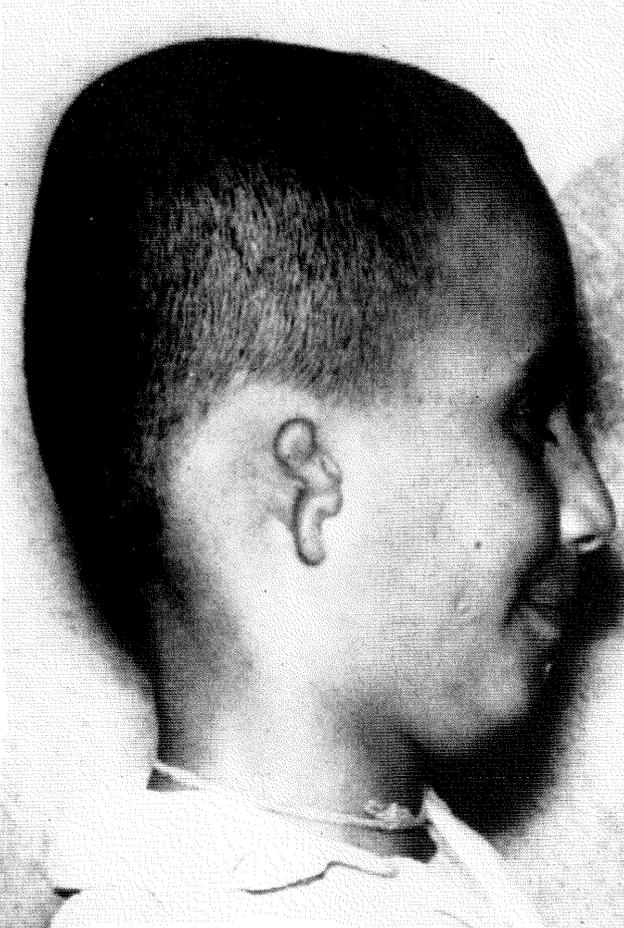


Fig. 8. Severely malformed ear (microtia) in a Turkish boy who said that he remembered the life of a man who was fatally wounded on the right side of the head by a shotgun discharged at close range.

who was run over by a train. Eyewitnesses said that the train severed the girl's right leg first, before running over the trunk. Lower hemimelia is an extremely rare condition, and Frantz and O'Rahilly (1961) found it in only 12 (4.0%) of 300 cases of all congenital skeletal deficiencies that they examined.

Discussion

Because most (but not all) of these cases develop among persons who believe in reincarnation, we should expect that the informants for the cases would interpret them as examples according with their belief; and they usually do. It is necessary, however, for scientists to think of alternative explanations.

The most obvious explanation of these cases attributes the birthmark or birth defect on the child to chance, and the reports of the child's statements



Fig. 9. Almost absent fingers (brachydactyly) of one hand in a boy of India who said he remembered the life of a boy of another village who had put his hand into the blades of a fodder-chopping machine and had its fingers amputated.

and unusual behavior then become a parental fiction intended to account for the birthmark (or birth defect) in terms of the culturally accepted belief in reincarnation. There are, however, important objections to this explanation. First, the parents (and other adults concerned in a case) have no need to invent and narrate details of a previous life in order to explain their child's lesion. Believing in reincarnation, as most of them do, they are nearly always content to attribute the lesion to *some* event of a previous life without searching for a *particular* life with matching details. Second, the lives of the deceased persons figuring in the cases were of uneven quality both as to social status and commendable conduct. A few of them provided models of heroism or some other enviable quality; but many of them lived in poverty or were otherwise unemployable. Few parents would impose an identification with such persons on their children. Third, although in most cases the two families concerned were acquainted (or even related), I am confident that in at least 13 cases (among 210 carefully examined with regard to this matter) the two families concerned had never even heard about each other before the case developed. The subject's family in these cases can have had no information with which to build up an imaginary previous life which, it later turned out, closely matched a real one. In another 12 cases the child's parents had heard about the death of the person concerned, but had no knowledge of the wounds on that person. Limitations of space for this article oblige me to ask readers to accept my appraisal of these 25 cases for this matter; but in my forthcoming work I give a list of the



Fig. 10. Congenital absence of lower leg (unilateral hemimelia) in a girl of Burma who said she remembered the life of a young woman who was accidentally run over by a train, with her right leg being severed first.

cases from which readers can find the detailed reports of the cases and from reading them judge this important question for themselves. Fourth, I think I have shown that chance is an improbable interpretation for the correspondences in location between two or more birthmarks on the subject of a case and wounds on a deceased person.

Persons who reject the explanation of chance combined with a secondarily confected history may consider other interpretations that include paranormal processes, but fall short of proposing a life after death. One of these supposes that the birthmark or birth defect occurs by chance and the subject then by telepathy learns about a deceased person who had a similar lesion and devel-

ops an identification with that person. The children subjects of these cases, however, never show paranormal powers of the magnitude required to explain the apparent memories in contexts outside of their seeming memories.

Another explanation, which would leave less to chance in the production of the child's lesion, attributes it to a maternal impression on the part of the child's mother. According to this idea, a pregnant woman, having a knowledge of the deceased person's wounds, might influence a gestating embryo and fetus so that its form corresponded to the wounds on the deceased person. The idea of maternal impressions, popular in preceding centuries and up to the first decades of this one, has fallen into disrepute. Until my own recent article (Stevenson, 1992) there had been no review of series of cases since 1890 (Dabney, 1890); and cases are rarely published now (Williams and Pembroke, 1988). Nevertheless, some of the published cases — old and new — show a remarkable correspondence between an unusual stimulus in the mind of a pregnant woman and an unusual birthmark or birth defect in her later-born child. Also, in an analysis of 113 published cases I found that the stimulus occurred to the mother in the first trimester in 80 cases (Stevenson, 1992). The first trimester is well known to be the one of greatest sensitivity of the embryo/fetus to recognized teratogens, such as thalidomide (Nowack, 1965) and rubella (Hill, Doll, Galloway, and Hughes, 1958). Applied to the present cases, however, the theory of maternal impression has obstacles as great as the normal explanation appears to have. First, in the 25 cases mentioned above, the subject's mother, although she may have heard of the death of the concerned deceased person, had no knowledge of that person's wounds. Second, this interpretation supposes that the mother not only modified the body of her unborn child with her thoughts, but after the child's birth influenced it to make statements and show behavior that it otherwise would not have done. No motive for such conduct can be discerned in most of the mothers (or fathers) of these subjects.

It is not my purpose to impose any interpretation of these cases on the readers of this article. Nor would I expect any reader to reach even a preliminary conclusion from the short summaries of cases that the brevity of this report entails. Instead, I hope that I have stimulated readers to examine the detailed reports of many cases that I am now in the process of publishing (Stevenson, forthcoming). "Originality and truth are found only in the details" (Stendhal, 1926).

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Correspondence and requests for reprints should be addressed to: Ian Stevenson, M.D., Division of Personality Studies, Box 152, Health Sciences Center, University of Virginia, Charlottesville, VA 22908

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The "Enemies" of Parapsychology

ROBERT McCONNELL

Langley Hall, University of Pittsburgh, Pennsylvania

Abstract — The author regards as "enemies" of parapsychological research (1) those critics who confuse parapsychology with popular superstition, (2) those parapsychologists who know all the pieces of evidence for the reality of psi effects but who lack the capacity to integrate and to evaluate that evidence as a whole, and (3) those professional psychics whose faltering attempts to apply psi for profit give the field a bad name. The author believes that parapsychology's urgent task is to bring mutual understanding between scientists and the public by exploring the obscure but real psi phenomena that give rise to popular superstition. He sees extrasensory perception and psychokinesis as evocable, operationally-defined psi phenomena. However, he rejects as a religious endeavor the search for logical proof of their reality and advocates, instead, a Bayesian summation of countervailing intuitive probabilities. The author rejects blind empiricism as a practical path to the utilization of psi. He offers several speculations regarding future discoveries in parapsychology, three of which are:

(1) Healing by self-hypnosis, as opposed to noncontact therapeutic touch, may be normal in an evolutionary sense.

(2) Psychoneuroimmunology and psi may play complementary roles.

(3) The principal future importance of parapsychology may be to allow scientific understanding of psi processes occurring within the human body.

Definition

To minimize controversy, I shall write plainly, beginning with two definitions, so that you can know whom and what I am discussing.

By the word "parapsychology" I mean the scientific study of psychic or psi phenomena. By "enemies of parapsychology" I mean those sometimes friendly and usually well-meaning persons whose actions discourage the funding of parapsychological research; for I believe that today there are no barriers to progress in understanding psi phenomena except lack of money. In my judgment, we have proved that psi phenomena occur, and in the last two decades we have discovered how to do cumulative basic research.

Satisfying the Professional Critics

The most formidable of the "enemies" of parapsychology are the professional critics who have made a career of misrepresenting and ridiculing the

field instead of acknowledging it as a legitimate area for investigation. The principal method of attack by these critics has been to blur or deny the distinction between the scientific and the popular approaches to psychic phenomena.

Perhaps the most egregious example of dedicated professional criticism of this field will be found in Chapter 9 of a report titled *Enhancing Human Performance* by the National Research Council, the principal operating agency of the U.S. National Academy of Sciences (Druckman & Swets, 1988). The flaws in that report have been discussed by Palmer, Honorton, and Utts (1989) and by McConnell and Clark (1991).

To the list of "enemies" of parapsychology I would have to add a few eminent scientists who, while they do not personally criticize the field, are willing to lend their names as sponsors of organized efforts to belittle this kind of research. Elsewhere, I have analyzed in detail these systematic attempts to denigrate parapsychology as a field of science (McConnell, 1987a).

The best way for parapsychologists to respond to the confusion between scientific parapsychology and popular superstition may be in two steps: (1) present the familiar idea that superstition is the attempt of uneducated people to deal with frightening events that they do not understand, and (2) explain that parapsychologists have found an important reality underlying popular occult beliefs. Thus, it becomes parapsychology's task to bring about a rapprochement between science and the public by exploring at a fundamental level those natural phenomena that give rise to popular occult superstitions.

The Prevalence of Psychic Experience

Ordinary people who repeatedly observe extrasensory perception in themselves or in close friends cannot help but lose confidence in their scientific leaders who tell them that ESP is impossible. Of course, there are many other areas of science and technology that are alienating the nonscientist. These range from psychiatry in the courtroom to the radioactive and chemical contamination of our living space. However, among all such gaps between the pretensions of science and the reality of experience, psychic phenomena occupy a unique place, for they relate to our basic beliefs about ourselves in the universe.

It has been estimated that more than three-fourths of all Americans have occult beliefs of one kind or another. Many of these believers are of high socioeconomic status. One remembers, for example, how the wife of former U.S. President Reagan publicly confirmed that she guided the president's daily schedule by consulting an astrologer and saw nothing wrong with that. With occultism pandemic, it will be difficult to correct the occult beliefs of lay persons by education if our scientific leaders cannot draw a distinction between popular astrology and laboratory extrasensory perception.

If one grants the possibility that psi occurs, one may still remain unconvinced that psi is an important factor in the alienation of the lay public from science. If psi is weak and difficult to observe in the laboratory, how much of it

occurs spontaneously? I would like to propose a startling answer to this question.

On the one hand, it is agreed that scientists rarely knowingly experience spontaneous psi. In my own case, for example, I recall having experienced identifiable spontaneous ESP only once in my lifetime, and that was of a trivial and scarcely noticeable event.

On the other hand, after 40 years of conversations with persons who came to me because they were annoyed or frightened by their own psychic experiences, I am inclined to believe that spontaneous psi is commonplace among less analytic people in our society. Consequently, I suspect that scientists' neglect of psychic phenomena contributes significantly to the ever-growing gap between that small minority who believe that their views of reality are rationally based and the vast majority of our citizens who cannot distinguish between rationality and irrationality and who know only the reality of experience.

I shall have more to say later about the reality of experience. For the moment, however, I merely wish to acknowledge that we have no explanation for this seeming difference between thinkers and feelers as regards the experience of psi. Scientists would like to believe that this difference is simply a matter of who is more likely to deceive himself or herself, a scientist or lay person? However, it may also be true that scientists actually experience many instances of ESP which their training leads them to ignore. Or, what I believe is quite probable, there may be a generic difference in the psi ability of brains that emphasize analytic thinking and brains that emphasize feeling.

If blame must be placed somewhere for the persistence of antagonism between thinkers and feelers concerning the reality of psi phenomena, I believe it belongs on the scientists. Whether or not they privately indulge in a divinely-revealed religious belief, scientists, almost without exception, have declined to apply the methods of science to the relation of consciousness to the physical world.

If one does not believe in the reality of psi, this kind of talk will be regarded as fantasy. But if psi occurs, then urgent possibilities come into view. My point is simply this: Much hangs upon the question, "Do psi phenomena occur?" It is time that scientific orthodoxy should face this question honestly. Scientists who are concerned about popular antagonism toward science might do well to urge support of parapsychological research so that the public can know what to believe and what is false or uncertain, and so that the public's faith in the perspicacity of our scientific leaders will not suffer.

More Friendly Enemies

I perceive as a unique and surprising class of "enemies" those parapsychologists who find themselves unable to make an overall evaluation of the evidence for psi phenomena. They agree that an anticipated but unexplained anomaly occurs repeatedly under specified laboratory conditions. Yet they say

that the evidence is inconclusive. If, after one hundred years of research in this field, prominent parapsychologists, for whatever reasons, take the position that we do not know if the phenomena occur, who can blame uninformed outsiders for relegating parapsychology to a back burner on the stove of scientific research? That, of course, is where many parapsychologists would like to keep it. Later in this paper I shall explain why I believe we can justly claim that there is no longer a reasonable basis for doubting the reality of at least some psi effects.

Still another class of enemies of parapsychology, in my opinion, are those lay persons who apply psi phenomena to make a living. These include those psychic healers, psychic finders, and future-tellers who advertise their services. These enemies seek publicity and compete with parapsychology for financial support even while they draw criticism upon parapsychology from our scientific leaders, who, for lack of interest, are unable to distinguish between scientific investigation and preparadigmatic application.

As adjunct to this class of enemy, I would list those foundation directors and private philanthropists who finance applied psi projects with insufficient understanding of science and no evident feeling for the experimental implications of what we have already learned in parapsychology. I shall speak later about the mutual importance of applied and pure research in parapsychology. First, however, I shall address the question of the adequacy of the proof of psi phenomena.

The Nature of Scientific Proof

Psi is an anomaly because it is unexpected within the currently dominant worldview. But psi is more than an observational anomaly. It is evocable and operationally defined. Psi is the process in which consciousness directly gathers information from, or exerts a force upon, the world outside the human body without use of the body's sensorimotor mechanisms.

One cannot by logic prove the occurrence of an anomalous operationally-defined phenomenon. More specifically, one can never prove by logic that an unrecognized counter explanation based upon familiar principles does not exist for any experiment seeming to show psi. To put it briefly, one cannot logically prove nonexistence. I shall come back later to the question of how psi might be proven to occur.

A perfect experiment would be one that, when reported in the literature, would be accepted as undeniable proof of the occurrence of psi. The only kind of undeniable written proof is logical proof. Since we cannot prove psi's occurrence by logic, a perfect experiment will not be found in the literature, now or at any future time.

Nevertheless, before they will agree to the unqualified statement that psi phenomena occur, some parapsychologists and most critics of parapsychology demand the impossible, perfect experiment. Or, if it is not quite perfect, it must at least be "repeatable upon demand."

"Repeatability upon demand" falls under the idea of "predictability"; for one cannot have the former without the latter. In science, "predictability" usually requires or implies "theory," and "theory" is a form of "understanding."

Thus, we have four slightly different, but in some circumstances more or less equivalent, terms: "repeatability upon demand," "predictability," "theory," and "understanding."

None of these four conditions is required for proof of the occurrence of a phenomenon. Astronomers did not have to understand supernovae to be sure they existed. All that was needed was sufficient observation.

The Observation of ESP

How do we know when there has been sufficient observation to establish the reality of a phenomenon? Even in astronomy this is not a logical question, but a matter for informed judgment.

As to extrasensory perception, there have been spontaneous cases reported throughout history among all races and cultures, and they are still being reported today. These cases involve both reputed psychics and ordinary people. It would be unscientific to ignore this evidence. Moreover, by statistical inference we have reason to believe that for many ordinary people minor instances of ESP occur at least several times a month. These instances can be identified with confidence only in those unusual cases when the facts are so convoluted that coincidence is not a reasonable counter hypothesis and where the surrounding circumstances accidentally conspire to eliminate logical inference as an explanation. These conditions are so rare that the actual number of psychic incidents must be much higher than the number recognizable by a parapsychologist.

Recent and historical records of spontaneous psi make a strong *prima facie* case for its reality. Nevertheless, one might excuse the skeptics if the matter rested there, as it did before the founding in England of the Society for Psychical Research. In the last one hundred years and especially in the last fifty, in the laboratory we have observed the extrasensory perception of symbols on hidden cards, of pictures concealed at a distance, and of thoughts existing only in someone's consciousness. We have detected ESP in persons in a normal wakeful state, or in hypnotic trance, or dissociated in the Ganzfeld condition, or dreaming in sleep. We have found laboratory evidence of ESP regardless of sex, age, intelligence, race, and mental health.

There have been more than a thousand psi experiments reported in peer-reviewed journals — some of them so carefully done as to be ridiculously meticulous (Honorton, 1987; McConnell, 1983b; McConnell and Clark, 1987). It has been shown by meta-analyses in the last five years that the overall success rate in psi experiments cannot be explained by the selection of favorable results for publication and that success is statistically unrelated to quality of experiment.

Taken separately, any one observation of ESP may have little to contribute to evidential proof. Taken together, the totality of observations is, in my judgment, conclusive. I believe that the day will come when, looking backward, historians will ask, "How could Twentieth Century scientists have been so blind as to reject psi?"

Countervailing Probabilities

A crucial question is, of course, how can a multitude of unrelated observations be combined to tell whether psi occurs? Many years ago I outlined a conceptual answer to this question (McConnell, 1977). My ideas were not particularly original, but they were carefully organized, and they have never been disputed. Briefly summarized, my line of argument was this: To decide whether a phenomenon such as ESP is real, one must subjectively formulate, compare, and choose between two opposing independent probabilities, one of which seems to say that ESP occurs, while the other denies it.

We may begin as follows. For each observation of a supposed psychic phenomenon such as ESP we must estimate a subjective probability that the observation was the result of chance and/or observational mistakes. In making this estimate, we must lay aside all belief as to whether ESP does or does not occur. For a laboratory experiment this subjective counterexplanatory probability (mnemonic: SCEP) might be thought of as the customary null-hypothesis chance probability, plus estimated probabilities of fraud and of honest mistakes of all kinds. For example, if we judge an experiment to have been well done by a trustworthy experimenter, we might arrive at a subjective counterexplanatory probability as large as one in ten, even though the calculated chance probability was as small as one in ten thousand.

By itself, a counterexplanatory probability of one in ten for a single experiment is not very interesting. However, when such probabilities for all independent observations are multiplied together, the resulting overall subjective probability can be extremely small. This overall probability might be thought of as the reciprocal of the betting odds favoring ESP based upon all of the direct evidence. This is the first of the two probabilities that must be weighed against each other.

Of course, one must also take into account the indirect evidence. This can be represented by a second probability, the subjective antecedent probability (mnemonic: SAP) that ESP might be a real effect. This second probability is antecedent in the sense that it is derived from generalized experience and belief, including one's exposure to the opinions of others, rather than from ad hoc study of the phenomenon. Among educated people in our culture, the antecedent probability of ESP is usually quite small because it is based largely upon two things: (1) the generally adverse opinions of scientists, most of whom have not studied the evidence (McConnell & Clark, 1991), and (2) assumptions about the nature of ESP that are not supported by the laboratory evidence. If one adopts the prevailing negative opinion among scientists and if one has unreal expectations of how ESP would manifest itself if it were to

occur, one can defend a vanishingly small subjective antecedent probability for the reality of ESP. This might be thought of as the reciprocal of the betting odds *against* ESP based upon the *indirect* evidence.

By comparing the subjective counterexplanatory probability and the subjective antecedent probability, one can make a subjective decision as to the reality of ESP. This is not a logical decision but rather, the kind of practical judgment each of us makes every day in our lives. I want to emphasize that this kind of weighing of countervailing probabilities is something we do every time we choose any course of action and that our fortunes and our very lives often depend upon it. On the other hand, only theologians and mathematicians depend upon logical proofs, and their conclusions are always implied by their starting assumptions.

It is a strange fact that very few competent scientists have used the countervailing probability approach to parapsychology. It is strange because the mere existence of these two tiny contradictory probabilities demands attention. If an examination using the above principles were widely undertaken, I believe that scientists, generally, would find themselves agreeing that both extrasensory perception and psychokinesis have been shown to occur within the epistemological framework of Western science.

I do not expect skeptical scientists to sit down and numerically estimate intuitive probabilities. That is not the way scientists work. What I would expect is that all true scientists would regard parapsychology with an open mind — remembering that we understand nothing about consciousness as a property of physical matter, and that quantum mechanics has taught us that we cannot trust intuition to tell us the limits of reality. Also, I would expect true scientists to suspend judgment on the impressions they may have gained from the news media and from proponents such as me as well as from the professional critics of parapsychology.

That much I would expect of all scientists. I would hope for more from a few scientists who were curious about the role of consciousness in the universe, who were not overwhelmed by the need to publish or perish in their own specialty, and who were competent in elementary physics, psychology, and statistics. I would hope that these few scientists would look first at a careful selection of original experimental papers from the peer-reviewed journals of parapsychology and then — led by their captured imaginations — would look at the entire field of evidence, and would thereafter form a subjective judgment as to the probable reality of psi phenomena.'

This is the method of evaluation that I used some forty years ago when I bet my professional career on the proposition that, beyond all reasonable doubt, psi phenomena do occur.

Pure Versus Applied Research

I promised earlier to discuss the relative need for pure and applied research in parapsychology. Let me say to begin with that I am not opposed to seeking immediate practical applications of psi. The question is one of the relationship

and relative importance of these two kinds of research. To illuminate this statement, may I turn to another field?

Before the science of chemistry began, physicians for several thousand years had practiced medicine with a certain degree of success. Nevertheless, what has happened in the last century in this field has dwarfed in importance all that went before, and has done so because basic research gave us scientific understanding.

A comparable situation exists with regard to psychic phenomena. Throughout the ages, psychic persons have offered inspiration and practical assistance to their fellow humans. Nevertheless, after millenia of applied psi we know next to nothing scientific about these phenomena beyond the fact of their occurrence. Moreover, the ethical teachings of Christ and other celebrated psychics are largely disregarded by the governing elites of the Western world on the ground that revealed religion contains many contradictions and has no basis in science. The need for understanding human nature has never been greater than today, but, if we believe that a direct search for clinically beneficial psi effects will lead to dependable control and to cultural acceptance, I think we are deluding ourselves.

The search for psi applications can, however, be useful if it points the way for basic research. Moreover, if it is carried out as a discipline within the tradition of Western science, it may yield some level of empirical understanding. Even so, brute-force empirical research in science is well known to be uncertain and painfully slow.

Speculations

In closing, let me give my own partial vision of the role of experimental parapsychology as it may affect the future. I shall restrict myself mainly to the topics of healing and health.

From study of the relevant literature it became evident to me about a decade ago that consciousness can affect the body through psychokinesis. To some parapsychologists this has been obvious for a much longer time; while to others it is still not obvious at all. Moreover, not only can each of us affect our own body in this way, but some persons, who have a special gift for healing, can appreciably affect the body of another person directly by prayer, or suggestion, or whatever you may choose to call it.

I shall close by offering six speculations, of whose truth I am more or less convinced, and that may or may not become established after further research.

1. I am convinced by experiments already in the literature that psychokinesis is the essence of hypnosis and, hence by parsimony, that what theologians call "prayer" is actually psychokinesis (McConnell, 1983a, pp. 154-177).
2. I have no doubt that in the future, even as today, a few people using psychokinesis will experience dramatic healings by themselves or with the help of others, but evidence from the study of the placebo effect sug-

gests that these beneficial effects will be only contingently available and will be available only to a minority unless we achieve basic scientific understanding of the underlying mental phenomena.

3. I would guess that healing by self-hypnosis is a normal process but that the healing of another person by hypnosis, as in the noncontact "laying on of hands", is abnormal in an evolutionary sense. In the same vein, I infer that one of the challenges in future parapsychology will be to develop techniques by which each of us can accept such external psychokinetic influences as we desire and can reject or defend ourselves against the psychic evil intentions of others.
4. Since I believe in the reality of psychokinesis, I think it almost certain that there is a psi component to what orthodox scientists gingerly call "behavioral medicine." Psychoneuroimmunology has to do with brain-body mechanisms of healing. The accompanying consciousness-brain relationship is in the province of parapsychology (Braud, 1990; McConnell, 1987b, 1987c).
5. I would urge that we consider the possibility that the technical importance of parapsychology will derive from the fact that it is devoted to the study of psychokinesis and extrasensory perception outside the body, where one can hope to separate variables and to perform simple experiments to discover the scientific nature of psi, but that the ultimate utility of parapsychology will lie in the understanding it will give of the psi processes that occur *within* the human body.
6. And finally, I am not alone when I hope that great social benefit will someday follow from parapsychology's relevance to the question, "What are we?" Do our individual boundaries lie somewhere beyond our skin? How do we relate to our fellow human beings, and what are our natural obligations to one another?

Acknowledgments

Adapted from a lecture to the 33rd annual convention of the Parapsychological Association at Chevy Chase, Maryland, August 1990. Address for correspondence and reprint requests: Professor Robert McConnell, A234 Langley Hall, University of Pittsburgh, Pittsburgh, PA 15260.

Footnote

'Some parapsychological papers published since 1980 that should be of interest to skeptical scientists are: Alcock (1987); Bem and Honorton (in press); Braud (1990); Braud and Schlitz (1990); Dunne, Nelson, and Jahn (1988); Honorton (1985, 1987); Honorton et al. (1990); Honorton and Ferrari (1989); Honorton, Ferrari, and Bem (1992); Hyman and Honorton (1986); Jahn (1982); Jahn and Dunne (1986); May, Humphrey, and Hubbard (1980); McConnell (1983b, 1989); McConnell and Clark, (1987, 1990); Palmer, Honor-

ton, and Utts (1989); Radin (1988, 1990); Radin and Ferrari (1991); Radin and Nelson (1989); Rao and Palmer (1987); Schlitz and Gruber (1980, 1981); Schlitz and Honorton (1992); Schmidt (1981); Schmidt, Morris, and Rudolph (1986); Utts (1991). Included here are papers by Alcock and Hyman, two of the most virulent critics of parapsychology in modern times. Papers listed with an asterisk after the date are meta-analyses especially useful for surveying the field. Regarding meta-analyses in general, see Rosenthal (1986). Another 24 papers of substantial evidential significance published between 1965 and 1979 have been listed by McConnell (1983a, pp. 311-323).

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ANOMALOUS PROPAGATION

by Topher Cooper

I throw a spear into the dark. That is intuition. Then I have to send an expedition into the jungle to find the way of the spear. That is logic.

– Ingmar Bergman

Benveniste's Lab Closed

The laboratory of the controversial French immunopharmacologist, Jacques Benveniste, is to be shut down. INSERM (the National Institute for Medical Research) claims that the action is a routine administrative one, done without reference to the controversial studies (among many very highly regarded mainstream ones) done at the lab. Benveniste claims that the shutdown is the result of a long-term, Machiavellian manipulation intended to allow the closing.

Benveniste stirred up a scientific hornet's nest when he and his collaborators published, in the conservative, prestigious journal *Nature*, claims that they had found evidence that water retained a kind of "memory" of a substance dissolved in it – even when diluted to the point that there is virtually no chance that even a single molecule of the substance remains in a sample being studied. Furthermore, this memory of the substance remains active biologically – able, for example, to trigger allergic responses in cells. Such a phenomenon would provide a possible mechanism for the alternate medical system of homeopathy, in which such ultra-dilute solutions are used as medicines.

After publishing the paper in question, John Maddox, *Nature's* editor, put together a team (himself, magician James Randi, and "fraud-buster" statistician Walter W. Stewart; none of whom had any expertise in the kinds of experimental systems being used in the study) to do a site visit to Dr. Benveniste's laboratory. Two weeks after the original paper the team reported their findings in another paper in *Nature*: Benveniste *et al.* were "deluded."

Many people in the scientific community questioned both the concept of such an "editor assembled hit squad," its constitution and its methods. Some suspected that Maddox had done the whole thing as a publicity stunt. Despite these questions about the "debunking" many French scientists felt that French science had been held up to public ridicule and that Benveniste was at least partly to blame.

A year later (1989) Benveniste's lab was up for review (such reviews occur every four years as a matter of routine). The reviewers gave the lab high praise – except, of course, for his homeopathy experiments. The laboratory was allowed to continue operation – but only if no public moneys were spent on the

homeopathy experiments and if Benveniste promised to not describe his findings in the popular press.

It is now four years later, and Benveniste's laboratory – hugely successful by most scientific criteria – is twelve years old. According to INSERM procedures, after twelve years an INSERM lab must reapply for a charter. Benveniste has failed to do so, and so his lab will cease to exist.

Benveniste claims, however, that he is unable to apply for a new charter under the rules. They require that at least two researchers at a laboratory seeking a charter must be employees of INSERM, but Benveniste is currently the only INSERM employee there. This is because INSERM has been, according to Benveniste, systematically refusing to hire his employees or directing them away from his laboratory upon hiring them.

INSERM denies any systematic policy on the matter.

SOURCE: Tara Patel, "'Molecule memory' man to lose his laboratory"; *New Scientist*, Vol. 140, #1896, (23 Oct., 1993) p. 10.

The Watch had not been Invented in the Days of St. Januarius

The semi-annual liquefaction of the blood of St. Januarius (San Gennaro) occurred slightly ahead of schedule September 19th. Usually it takes about an hour of praying after the vial containing the blood has been removed from the special safe, but this time it had reportedly liquefied by the time it was removed. The time required to liquefy the blood is highly variable – the previous May four days of prayer were needed.

SOURCE: "San Gennaro blood liquefies in record time," UPI, Naples, 19 Sept., 1993; For additional information see: Epstein, M. & Garlaschelli, L.; "Better Blood Through Chemistry;" *Journal of Scientific Exploration*; Vol. 6, pp. 233-246.

The Miracle Here is that it *Doesn't* Liquefy, Burn, or Vaporize

The basement inventor who makes a stunning breakthrough which leaves the experts gaping, is purely a fictional character . . . or is he? Maurice Ward, a British plastics recycler, has been tinkering with plastics for a decade, trying to find one more flame resistant than those presently known. In 1989 he succeeded. He found that his latest batch, containing 21 polymers, ceramics and additives, completely resisted the effects of his welders torch. Not only didn't it smolder or smoke, it was completely undamaged. Even tests with a plasma torch (which can easily cut through metal several centimeters thick) at a major industrial lab was unable to damage a thin piece of the miracle plastic sheet dubbed Starlite by Ward. Tests at the Foulness nuclear lab near London were unable to do more than mar the surface of the plastic with a high-power laser device designed to test the affects of nuclear bomb blasts on materials. Enough energy was pumped through the lasers to raise the surface temperature of the sample to 10,000 C. The stunning insulating properties of the substance were

also demonstrated in this test: the temperature of the opposite side of the sample only increased 25 C.

When will we be seeing this miracle commercially? That's a good question. Ward wants more than the standard few-percent-off-profits royalty. He is holding out for a 51 percent share. In the meantime, he is releasing no information about the formula, and keeping careful control of all samples. It will not even be known whether commercial production will be practical until the formula is known.

SOURCE: "Starlite dazzles scientists, but its inventor won't divulge the recipe," Fred Guteri, *Business Week*; 16 Aug., 1993.

Peter Beckmann: 1924 - 1993

Peter Beckmann did not believe in special relativity. Perhaps more accurately, Peter Beckmann did not believe that there had been sufficient experimental justification for abandoning the absolute time and absolute space of classical mechanics. He expressed his thoughts on the matter in an eloquent and scholarly fashion in his book, *Einstein, Plus Two*. He was editor of the journal *Galilean Electrodynamics* which gave many people with unorthodox views of physics an opportunity to present them in a scholarly fashion. He was also the author of the popular, and quite mainstream book, *A History of Pi*. Whether Peter Beckmann was right or wrong, he made many people examine the basis of their belief very carefully, and that is an important contribution to science. If you enjoy mathematics in almost any form, this book is a "must read."

SOURCE: Robert Firth, "Peter Beckmann"; USENET computer bulletin board, group: alt.sci.physics.new-theories; 10 Aug., 1993

Sleep Aid

You've been having trouble sleeping, so you go to the doctor. She writes you out a prescription, which you go to the drugstore to fill. That night before going to bed, you take the spoon and put it in your mouth and leave it there for about twenty minutes. The spoon contains no medication. It is attached by a wire to a small box of electronics.

The spoon is actually a spoon-shaped antenna. It radiates low power, pulsed microwaves of a precise frequency. Preliminary double-blind studies seem to indicate that regular use aids sufferers from insomnia. It has been developed by a Swiss based corporation called Symtonic, Inc. Clinical trials are now under way by the Mayo Clinic Sleep Disorder center, the Stanford University Sleep Research Center, and (testing the device's efficacy in treating anxiety) The Massachusetts General Hospital Department of Psycho-pharmacology.

SOURCES: Pasche, B., Erman, M., & Mitler M. "Diagnosis and treatment of insomnia" (Correspondence) *New England Journal of Medicine*; Vol. 323, pp. 486-487. Hadjukovic, R., Mitler, M., & Erman, M. "Effect of Low Energy

Emission Therapy (LEET) on sleep structure" in *Electricity and Magnetism in Biology and Medicine*; Martin Blank, Ed., San Francisco Press, CA 1993

Hypnoimmunology

A new study provides evidence that hypnosis can strengthen the action of the immune system. The subjects were divided into three groups. The first group underwent hypnotic induction accompanied by suggestions that they would imagine their white blood cells attacking germs in their bodies. They were also instructed to repeat the exercise twice daily for a week through self-hypnosis. The second group relaxed in a warm flotation bath at the beginning and the end of the week. The third group received no special treatment. Only the hypnosis group showed an increase in the number of two important kinds of white blood cell. Within that group, subjects who were rated as highly hypnotizable showed a stronger average response. The study apparently did not, however, compare their hypnotic condition to one using imagery without hypnotic induction, nor, apparently, did the relaxation condition include twice daily relaxation periods comparable to the suggested self-hypnosis sessions.

SOURCE: "But hypnosis offers immune aid", Bruce Bower; *Science News*; Vol. 144, #10 (4 Sept., 1993); p. 153.

Music for the Smart Set

College students who are given ten minutes of Mozart to listen to before testing score an average of eight to nine IQ points higher in "spatial reasoning" than either those given ten minutes of a relaxation tape or ten minutes of silence. These results, which were quite clear-cut, were reported in a letter to *Nature*. The brevity required of this form of reporting inevitably leaves a lot of questions unanswered about the experiment, and the form of the experiment makes subject expectancy effects difficult to rule out, but it is an exciting beginning.

SOURCE: Rauscher, F. H., Shaw, G. L., & Ky, K. N.; "Music and spatial task performance"; *Nature* (Scientific Correspondence); Vol. 365, 14 Oct., 1993; p. 611

Light Show

"The observers are mistaken, no such phenomenon exists. Perhaps it's caused by afterimages created by ordinary lightning" has been the standard response to the many reports by pilots and others of huge "light shows" occurring above thunderstorms. A few scientists have considered that there might be something to the reports: "Perhaps occasionally there can be circumstances where a lightning bolt can strike vertically upward, rather than downward or horizontally" ignoring the protestations of the observers that it doesn't look anything like a lightning bolt.

Now NASA scientists, flying in a research plane have recorded images of the phenomenon. It is real and it looks nothing like ordinary lightning. Three of the nineteen images were described as something like a spiked crown, a big jellyfish, and a giant carrot. They stretched several thousand feet upwards from the tops of the clouds. The news report said that most of the images only lasted for one-thirtieth of a second, but that is the scan rate for many video instruments developed in the US, so that may simply be an upper limit.

All that is left, now that the phenomenon has been found to be real, is to figure out what it is, and what causes it. A more ambitious project is now being planned to answer those questions.

SOURCE: "NASA scientists witness lightning dance"; UPI; Mountain View, CA; 25 Sept., 1993.

Opinions expressed in this column are those of the author and do not necessarily represent those of JSE. Comments may be directed to the author by electronic mail at "cooper@cadsys.enet.dec.com" or U. S. post at Topher Cooper, Digital Equipment Corporation, 77 Reed Road (HL02-3/G13), Hudson, MA 01749.

THE SKEPTICAL PERSPECTIVE

by Michael Epstein, National Capital Area Skeptics

It is a capital mistake to theorize before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts. – Sherlock Holmes

Alternative Medicine

The newly-created Office of Alternative Medicine at the National Institutes of Health (NIH) has obviously hit a nerve in the Skeptics movement, or perhaps it has just directed a rather credulous news media to the subject. In any event, alternative medicine and the publicity surrounding its use have been the recent focus of much skeptical activity. The National Capital Area Skeptics (NCAS) have protested the slant of media coverage of a "remarkable" demonstration of the Chinese art of Qi Gong at a public lecture held at NIH (Denman, 1993). According to an article in the Washington Post (Brown, 1993), Qi Gong Master Wang-Pong Cheng punctuated his lecture by appearing to "shatter a rock without actually striking it." The NIH Record (Garnett, 1993) reported that he also "cut a chopstick in half with a dollar bill," and although not shown at the lecture, he was also known "for burning a newspaper without lighting it." Cheng also treated members of the audience for various musculoskeletal problems by waving his hands near the patients followed by a light massage. Although there was no official NIH endorsement, the location of the program attracted notice and lent an air of scientific respectability. NCAS will be presenting an opposing viewpoint at a public lecture and demonstration scheduled for mid-January 1994 at NIH, and featuring James Randi and Jim Alcock. The Rocky Mountain Skeptic published an article on the same topic by Dr. Zhang Honglin (1992), head of the Department of Qi Gong Research at the Institute of Acupuncture and Moxibustion, which is part of the China Academy of Traditional Chinese Medicine in Beijing, China. Dr. Zhang decried the "pseudo-Qigongists" and said they "deceive people by putting on a show, not offering scientific proof." His extensive investigations have found "no scientific evidence that proves the existence of supernatural remote Qi Gong."

A number of other skeptic groups have looked into alternative medical practices:

-The Rational Examination Association of Lincoln Land (REALL) commented critically on Chicago area media coverage of treatments such as naprapathy (Bloomberg, 1993).

-The New York Skeptics heard Dr. Robert Baker talk on fringe medicine (Arents, 1993).

-The New Zealand Skeptic published articles on the placebo effect (Morris, 1993), acupuncture (Dutton, 1993), herbal medicine (Walker, 1993), and a wide variety of other alternative medical treatments (Welch, 1993).

-The Rocky Mountain Skeptics are challenging the use of therapeutic touch by Colorado nurses (Rojas, 1993).

-The North Texas Skeptics presented a highly critical article on life extension pseudoscience, particularly the work of Pearson and Shaw, and an expose of diet supplements (Gorski, 1993).

-Recent CSICOP Skeptical Briefs contain critical articles on chiropractic and alternative medicine (Barrett, 1993).

-Every skeptic's favorite whipping boy and faith-healer, W.V. Grant, was highlighted by both the Georgia Skeptics (Hestevold, 1992) and the Bay Area Skeptics (Critelli, 1993).

The outcome of any serious research can only be to make two questions grow where only one grew before.

Worth Repeating - Rules for Skeptics

The REALL News (Hyman, 1993) recently reprinted an article on proper criticism written by Ray Hyman, professor of psychology at the University of Oregon and a Fellow and member of the Executive Council of CSICOP. The article, which first appeared in Skeptical Briefs, May 1987, presents eight rules for skeptics to upgrade the quality of their skepticism:

- Be prepared
- Clarify your objectives
- Do your homework
- Do not go beyond your level of competence
- Let the facts speak for themselves
- Be precise
- Use the principle of charity
- Avoid loaded words and sensationalism

For us believing physicists the distinction between past, present and future is only an illusion, even if a stubborn one. – Albert Einstein

Fun with Psychics

A Tampa Bay skeptic visited a psychic fair in Sunrise, FL in order to seek help in contacting his father. After forking over \$20, the skeptic was told by the psychic, astrologer and spiritualist (at least according to his business card) that his father (and mother) were both on the "other side" and still in love. The psychic batted zero. The skeptic's father and mother had divorced bitterly thirty years ago, and his father (who is still alive) thought the whole thing was hilarious (Johnson, 1993).

Not to be outdone, a South Shore skeptic visited a Cleveland psychic fair where he sought guidance from a tarot reading and a numerologist. The tarot reading didn't last too long after the skeptic pointed out to the tarot card reader that she should have seen that the upturned card of the devil indicated that he belonged to a skeptics group (Emrich, 1993).

Fun with Chemistry

In order to transmute mercury into gold, an "amateur" alchemist in Larose, LA decided to do some baking in an attempt reminiscent, if not worthy, of Paracelsus. To be exact, he attempted to produce gold in a hot oven through a combination of fire, mercury, and one half of an Idaho potato. Unfortunately, the attempt failed and he was overcome by the mercury fumes. Undaunted, he sought redress through the courts for workman's compensation as a seaman under general maritime law (the episode took place in a ship's galley). The court ruled that alchemy is not within the duties of a seaman (Reese, 1993).

Truth in Advertising

According to the Newsletter of the American Scientific Affiliation (ASA), a recent advertisement run in several national publications by Prometheus books promoted the book *Gospel Fictions* by using a quote from a 1991 review in *Perspectives on Science and Christian Faith* (Feucht, 1993). The review was generally critical of the book except for one gratuitous recognition of the book's "succinctness and lucidity" that was then used in the Prometheus ad. In response to a complaint by ASA, Prometheus has agreed not to use the quote in future ads. "Was this use of text out of context unwittingly illustrative of the contextual thesis in the book itself?" asks the ASA Newsletter article.

Man can learn nothing unless he proceeds from the known to the unknown.

Last Words

The quotations sprinkled throughout this column were taken from a series of decorative posters sold through the American Chemical Society Education Division for classroom use. I find it fascinating that the most skeptical statement is made by Sherlock Holmes, a character created by Sir Arthur Conan Doyle, well-known spiritualist and paranormalist, while the most visionary statement is made by Albert Einstein. It seems that the aims of the SSE are well met in all of these expressions. My last words are a paraphrase of one of the poster quotations: *There is one fact about science that we know for sure . . . we don't know much.*

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Opinions expressed in this column are those of the author and do not necessarily represent those of the National Capital Area Skeptics, JSE, or any other organization. Copies of some articles are available on electronic media and others may be available as hard copy. Requests should be directed to:

Mike Epstein, electronic mail at CompuServe: 76640,1540 or Internet: mse@enh.nist.gov or by U.S. post at Michael Epstein, B-222 Chemistry, NIST, Gaithersburg, MD 20899.

GUEST COLUMN: UNIDENTIFIED ATMOSPHERIC PHENOMENA OBSERVED BY AN ASTRONOMER

by F. Noel

Department of Astronomy, University of Chile, Santiago, Chile

As a professional astronomer, I have been heading a fundamental astrometry research program since 1965 based on observations made using the Danjon astrolabe at the National Astronomical Observatory located in the eastern outskirts of Santiago, Chile. For several years, I myself was one of the principal observers of the program, during which I carried out several hundred nights worth of observations of stars. During such nocturnal work with the astrolabe, owing to the method of observation with this equipment, one has a rather large field of view amounting to approximately 70 percent of the sky. Therefore during my many years as an observational astronomer, I acquired considerable familiarity with phenomena, natural and artificial, which one can observe in the sky. The following observation, however, remains a puzzle to me.

Observation of a V-Shaped Group of Luminous Objects

At approximately 22:30 local time on January 17, 1980 I was in front of my home in the eastern suburbs of Santiago de Chile. The sky was cloudless, although there was some smog, especially in the west in the direction of downtown. Sunset had occurred at 20:55 local time.

At that time I observed a point-shaped luminous object at an elevation of about 20 degrees; it was moving at a rather slow angular velocity from southwest to west approximately. No noise was heard and it looked like an artificial satellite, except for the direction of its motion. Its brightness, color and angular velocity reminded me of the old Echo artificial satellite from the 1960s. The object disappeared from sight during the few seconds that it took me to call two persons to participate in the observation. It was not apparent how the object had disappeared from view since there were no sources of obscuration evident. Having become puzzled by this observation I continued watching that same region of sky from time to time.

About fifteen minutes later (22:45 local time) and more or less in the same region where the bright light had been observed I spotted a faint luminous object moving slowly toward the zenith. During the first few seconds of observation it had the appearance of a luminous thread, oriented perpendicular to its direction of motion. However, as it approached the zenith, I could see that it was in fact a group of at least 30 lights distributed in a broad, symmetrical

V-configuration, reminiscent of a boomerang. Three members of my family who were with me also observed this group of lights.

Each individual light of the group looked like a star of third or fourth magnitude; the color was a pale white similar to a neon light, with a slight tint of yellow. The brightness was rather steady, with no apparent flicker. The angular width of the group was about 4 degrees, and the central angle of the V was about 150 degrees. It was first visible at about 20 degrees above the western horizon and disappeared at about that altitude in the east. The disappearance was gradual, probably as a result of atmospheric extinction. Since the group was in view for approximately two minutes, the mean angular velocity must have been a bit more than one degree per second. All these estimates are approximate of course. No noise was heard during the observation.

The group moved as an apparently rigid configuration since no relative motions of the lights were evident. The mutual distances remained fixed as far as it was possible to discern with the naked eye as the group transited near the zenith. The lights were quite uniform, showing no appreciable difference in brightness or color. When the group was about fifty degrees above the eastern horizon the lights on the left side of the V (northern side) showed a momentary agitation, quite similar to the agitation of star images observed through a telescope resulting from conditions of poor "seeing" (an observational astronomy term).

I am not aware of any other witnesses of this phenomenon other than the three persons with me. However that is not unexpected considering the low angular velocity of the group and the faintness of each light; it would not have readily caught the attention of a casual observer. I came to notice it due to the fact that I had been monitoring that zone in the sky where these lights happened to appear. Since the V-shaped group and the previously observed single bright light are both unidentified there is no logical conclusion to be drawn about whether the two events were related. Nevertheless, the degree of strangeness, especially of the V-group, and the coincidence of appearance in a narrow zone of sky and during a short interval of time are at least curious.

Could it have been a flock of birds somehow reflecting the lights of Santiago? Overtly this seems attractive, however if one recalls the rather fixed configuration, and the steadiness and uniformity of the lights, this explanation is difficult to accept. Flocks of birds which are seen in the central region of Chile show quite different configurations than the one observed; furthermore flocks of large birds are rarely seen over Santiago.

Observation of a Flying Flashing Light

The following month, at about 20:45 local time on February 14, 1980, I was driving east on Avenida Santa Maria close to downtown Santiago. Again, the sky was clear; sunset had just occurred at 20:35. The nearest mountains of the Andes, about 15 km distance, were visible in the background.

At that time I saw, through the windshield of the car, a bright flashing light moving steadily on a horizontal trajectory from southeast to east at an eleva-

tion of approximately 10 degrees, slightly higher than the mountains in the distance. The color was white like a photographic flash. The period of the flashing was between one and two seconds; the angular velocity was about one degree per second. Again, these estimations are approximate.

The initial impression was that of an airplane light. However it was quickly clear that the flashing light was not attached to any visible object. (Recall that this was only ten minutes after sunset.) I parked the car and continued the observation outside. After some further seconds of apparently steady motion in a straight line trajectory a sudden change took place. The straight line motion changed to very erratic motion in a narrow zone of sky of perhaps two square degrees. The flashing frequency increased dramatically but the brightness diminished substantially. At the same time one could see erratic and very bright flashes of light apparently simultaneously from several separate points in that narrow zone of sky. It looked as if either multiple lights were appearing simultaneously or as if the single light were jumping quickly between two points. This continued for several seconds, and then the phenomenon reverted to its initial condition: bright and regular flashes of light with the object following a straight line trajectory, this time from east to northeast until finally the light was obscured by Cerro San Cristobal, a 290 m tall hill over otherwise flat ground close to Santiago's downtown.

This phenomenon was observed in part at apparently the same time by another witness located about 8 km from my observing site.

Using this distance and the approximate directions to triangulate I arrive at a rough estimate of about 13 km for the distance to the object from my vantage point.

It has been my experience as an observational astronomer that non-anomalous phenomena do sometimes exhibit enough strangeness to be mislabeled anomalous by an inexperienced observer. However in the context of my observational experience it is my opinion that these observations are genuinely anomalous and are not explainable by any known natural or artificial phenomenon.

Dr. Fernando Noel is Associate Professor in the Department of Astronomy at the University of Chile in Santiago. Since 1965 he has been head of a southern hemisphere fundamental astrometry program at the Danjon astrolabe of the National Astronomical Observatory. He has authored numerous papers in this field in such journals as *Astronomy and Astrophysics*, *Astronomical Journal*, etc. He is also a member of the Organizing Committee of Commission 8 (Positional Astronomy) of the International Astronomical Union.



LETTERS TO THE EDITOR

On Biological Transmutation of Elements

In a recent column ("The Skeptical Perspective," JSE, vol. 6, no. 4) Epstein presented the item "What's Hot and What's Not" in which he cites Dr. Hearn's remarks concerning Kervran's experimental techniques for the study of claimed transmutation of elements by plants. This points out a major problem: the most well-known sources are not necessarily the most correct or valuable. Kervran's publication of a book in the United States in 1972 and work of his published in France have led to the concept of "biological transmutation" being attributed to him. Unfortunately this may be an example of an interesting phenomenon being rejected on the basis of faulty sources.

It is informative to check Kervran's references to the earlier work of von Herzeele and Baranger. The following information might prove useful in spurring further investigation of this claimed phenomenon.

Between 1875 and 1883 von Herzeele authored a number of articles on inorganic metabolism in plants. While studying the levels of calcium, potassium, phosphorous and other elements in grains and seedlings of *vicia sativa*, he noticed apparently anomalous variations in the total abundances of these elements in the plants and culture media. Experiments seemed to indicate that the addition of calcium salts or of K_2CO_3 resulted in an anomalous increase in the abundance of phosphorous or calcium beyond the amount expected based on the chemistry.

The seriousness and consistency of these experiments caught the interest of Dr. P. M. Baranger some 70 years later. (Baranger earned a doctorate in physical sciences at the "Pasteur Institute" in 1931 and authored 156 publications during his years as a professor of chemistry at the Polytechnical School.) Experiments were conducted by Baranger and collaborators (Y. Lassale, M. Thoreau, J. Finelli, D. Depond, M. Rougeot, Marechal) from 1950 until about 1970. The protocols were very carefully devised and the methods involved state-of-the-art techniques at that time. Data on anomalous changes in calcium and phosphorous concentration began to be reported in 1957 and were published in 1960 and 1961.

The methods used were the following. Seeds of statistically well-known composition were placed in Pyrex glass or polyethylene dishes and soaked with bi-distilled water, with or without a known amount of $MnCl_2$. The levels of mineral traces that could be released from the reagents and apparatus and other measurements were determined with a relative error of approximately

111000. Culture dishes were placed in a "phytotron" under controlled conditions. After an interval of about 30 days plants and culture media were desiccated and subject to analysis using various methods involving appropriate chemical reagents with photometric determination or direct flame spectroscopy. During the entire process the samples were protected against contamination from traces of elements that could have been released by the materials, the reagents or even the atmosphere in the laboratories. The various apparatus were also systematically controlled.

Hundreds of replications were run with independent batches of 100 seeds of known homogeneity, and as many as ten replications of chemico-physical determinations were done for each case. Statistical distributions were carefully examined and t-tests were used to assess the significance of the results.

A statistical study of a series of experiments in 1956 was performed by Cavé (1957). This study found a moderate but significant decrease in potassium ($p < 0.01$) followed by a marked increase in potassium ($t = 8.32$ for 197 degrees of freedom) after germination in the presence of an accurately defined amount of CaCl_2 . Summarizing the results of all the experiments, seedlings were found to have undergone the following changes in composition after germination and growth: potassium showed a 10% increase in the presence of calcium salts; calcium manifested a 2.5% increase, in distilled water, and a (still significant) 1.5% increase in calcium-containing medium. No significant changes were observed for phosphorous.

Both the French Academy of Sciences and the Academy of Agriculture rejected Baranger's papers. The Academy of Sciences referee's comments criticized the presentation of percentage variation data even though the full data set was available. He also confused biological, chemical and physical sources of deviation and ignored the statistical studies. The Academy of Agriculture was probably influenced by two experts (both appointed by manure factories) who claimed that they were unable to reproduce Baranger's results on what they termed "biological transmutation of potassium." In fact, Baranger never claimed to have discovered a "transmutation of elements" phenomenon; moreover the experts did not use the same protocols as Baranger and did not present detailed reports of their own experiments. In the end, Baranger's manuscript was presented at the Academy of Agriculture by Prof. J. Guilhon, but only five years after Baranger's death and during "in camera" session. The history of this has been summarized by Agriculture General Inspector J. M. Gatheron who collected and edited a number of original reports which were privately published in a booklet by the family of Prof. Baranger in 1977.

In recent times, Prof. H. Komaki, an applied microbiologist at the University of Mukogawa, appears to have found anomalous variations of potassium and phosphorous levels during the growth of some 30 different strains of micro-organism, depending on the initial composition of the growth medium. However I am not aware of any other recent academic work in this area.

One might speculate on some kind of tunneling effect operating to overcome the barrier between atomic nuclei, but at this time it is difficult to say whether the results of von Herzele and Baranger actually provide evidence for any type of biologically-directed transmutation. Clearly Kervran's arguments were not convincing to scientists, even though he achieved some popular fame for the concept. Indeed the difference between a valid scientific theory and a compilation of unsubstantiated assertions is not fully appreciated by the general public.

Part of the problem may lie with the peer-review system by anonymous referees. Within the mainstream sciences it often serves to suppress evidence to the point that researchers are totally unaware of apparent anomalies, which makes it impossible to resolve such matters, while outside the mainstream sciences there is no system in place at all to check the veracity of such claims. A more constructive refereeing system is clearly desirable. The role of SSE and JSE are important in this regard.

Thanks are due to Philippe Desbrosses for kindly making valuable documents available.

Michel Bounias
Universite'd'Avignon
Biomathe'matique et toxicologie
23 rue du 58 ème R. I.
F 84000 Avignon, France

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Comments on Michel Bounias' Letter to the Editor

The letter by Michel Bounias regarding "qualified scientific works" that support claims for "biological transmutation" brings up some concerns that I was unable to address in the limited space of my column "Skeptical Perspective." These concerns apply not only to this particular case, but to the general case of claims of anomalous events.

The age of the scientific "generalist" is over. Scientists today cannot possibly maintain expertise in the myriad of modern scientific disciplines. Nineteenth century scientists like **Bunsen** could be "state-of-the-art" and publish in a variety of areas, since the knowledge-base was of limited size. Today, it is difficult to remain current in a single speciality within a scientific discipline (e.g., inorganic analysis in analytical chemistry). I use the latter example because that is the area in which I was trained and have worked for over twenty years. And this is precisely the area that applies to the work on inorganic metabolism in plants. Let me apply that knowledge to an examination of Baranger's experiments as described by Bounias.

Baranger's results hinge on his determination of K, P, and Ca using the analytical methods of photometry and direct flame spectrometry that were available in the 1950s. The claim that "levels of mineral traces that could be released from the reagents and apparatus, as well as other measurements were determined with a relative error of about 111000" is ridiculous. That level of precision is restricted to classical methods of analysis, such as titrimetry and gravimetry, and can only be approached today by sophisticated methods that employ internal standard ratio measurements. But measurement precision does not guarantee reliable analytical results. Measurements using both photometry and flame spectrometry suffer from significant biases that result from the effect of other elements on the measurement of the analyte. These interferences effect the "accuracy" of measurement and the magnitude of the interferences will vary depending on (a) the concentration of the interfering element and (b) the physical parameters of the measurement system. It is not unusual to see differences of more than 10 percent using exactly the same apparatus on different days, particularly when measuring calcium in the presence of phosphorus (a classic interference pair). In the 1950s, the instrumentation used to make those measurements was subject to numerous errors that were not understood at the time.

This is just the tip of the iceberg. Analytical techniques like photometry and flame spectrometry depend on comparisons of standards and samples. Accuracy of analysis depends critically on the correct and stable preparation and storage of standards. Even today, when interlaboratory comparisons are used in collaborative certification of reference materials, it is not unusual to see differences of "orders of magnitude" for some element concentrations obtained from different laboratories (and this in 1990, not 1960!).

So where does this leave us? Even though it is a well-worn phrase, extraordinary claims still require extraordinary proof. It is easy for those who are not

familiar with the critical requirements of inorganic measurements to see an anomaly when none exists. Assessment of significant differences in data sets by statistical analysis of data assumes control of measurement bias, and that is something we have difficulty with even today.

Sure, there are still anomalies. In twenty years I have experienced more than my share. Some I was finally able to explain ... others I have yet to understand. Nevertheless, those in other fields of science should be extremely cautious before accepting extraordinary claims without intimate knowledge and understanding of how they were measured. And that requirement for caution emphasizes the need for an organization like the SSE, which, unlike others that are willing to examine anomalies, can count on a broad range of scientific expertise within its membership to properly examine extraordinary claims.

Michael Epstein
Chemistry
NIST
Gaithersburg, MD 20899

Comments on Suitbert Ertel, "Puzzling Eminence Effects Might Make Good Sense"

This paper shows that apparently inconsistent findings about the influence of eminence on Mars-type (or Gauquelin-type) effects might actually be quite consonant with one another: reported decreases as well as reported increases — or little if any effect, for that matter — could equally be accommodated if there is a real phenomenon in which the relationship between eminence and the degree of the effect is curvilinear. (By Mars- or Gauquelin-type effect I mean that certain sorts of human achievement correlate statistically with planetary positions at birth of the people concerned.)

Further, Ertel adduces quite plausible specific reasons for assigning the data in several published reports respectively to the higher-eminence (decreasing effect) and lower-eminence (increasing effect) regions of such a curve.

The strength of the paper is its empiricism and pragmatism, which its title, "... Might Make Good Sense," tends to obscure: that title hints that an explanation is offered why, in causal terms, eminence has the effect that it does. I say this not for the sake of quibbling but to emphasize that Ertel's paper has primarily an empirical basis and focus. Had Ertel ventured a reason why the relationship *ought* to be curvilinear, then it would be not unnatural to suspect that his finding of that relationship might have been subconsciously influenced by pre-existing belief. If, on the other hand, someone else now proposes a causal reason, Ertel's data and discussion thereby become all the more convincing.

In point of fact, with the benefit of hindsight conferred by reading Ertel's paper, it occurs to me that one ought to expect the magnitude of Gauquelin's effect to increase and then decrease with increasing eminence.

If a Gauquelin effect exists (as other than a statistical artefact), then by definition it will initially become larger as eminence rises from zero: the Gauquelin (or Mars) effect *is* the claim that accomplished people are born more frequently than by chance, under certain planetary configurations.

But Gauquelin also found different types of activity to correlate with different planets. One would then expect to find an unceasingly increasing relation between the magnitude of the effect and eminence, if and only if the Gauquelin effect not only correlates with achievement *but also with the choices made about what activity an individual will pursue*. For example, if Mars in the right sectors actually correlates with athletic achievement, that could only be observed if people born at those sectors indeed choose (at least as often as the rest of the population) to pursue an athletic career.

Now assume that we are all born with various, different, specific capacities as well as with varying levels of *general* ability. (Studies of the heritability of "intelligence", for instance, indicate that as much as 1/2 is congenital, of the *general, non-specific* intellectual talents that intelligence tests measure.) Those who possess the highest levels of *general* native capacity are likely to distinguish themselves no matter what careers or hobbies they happen to choose, be it (say) the military or politics or bridge or painting or golf. But if *different* planetary configurations correlate with high achievement in those different activities, then the most inherently talented people will not show evidence of that relationship (so long as their choice is random, which activity to concentrate on).

Thus former President and General Dwight D. Eisenhower, we know, *did* excel in all the five areas I mentioned; yet his eminence in them could not provide statistical evidence for Gauquelin's effect because under the observed correlations (Michel Gauquelin, *Birth Times*, Hill & Wang, New York 1983) his birth-date would have to contribute both an excess under Mars (sports and military leadership) *and* a below-chance count under Mars (for painters) *and* an excess under Jupiter (favoring top executives, politicians, and military leaders) *and* a below-chance count under Saturn (painters).

On the very reasonable presumption, then, that human beings congenitally possess both a *general* aptitude and some specific ones, *if* there is a Mars-type effect it will correlate with increasing achievement only up to a certain point and then tend to revert to chance — no apparent effect — for those most highly talented people who have the native ability to succeed at whatever they turn their attention to.

Henry H. Bauer
Virginia Polytechnic Institute & State University
Blacksburg, VA 24061-0212

BOOK REVIEWS

The Burt Affair by R. B. Johnson. Routledge, 1989, 400 pp. \$35.00.

Science, Ideology and the Media: The Cyril Burt Scandal by R. Fletcher. Transaction Pubs., 1990, 196 pp, \$29.95.

A famous quotation attributed to the British physicist, Ernest Rutherford, is "If an experiment requires statistical analyses to establish a result, then one should do a better experiment." Rutherford is, of course, both right and wrong. The mark of the successful classical physicist was the ability to devise the definitive, *deterministic*, repeatable experiment which settled an issue beyond doubt. But not everything falls in the realm of classical physics and when variation exists, statistical analysis is unavoidable. In fact, so wide-spread has statistics become that it is hard to get a degree in business, biology, the social sciences, education, agriculture or of course, physics, without taking at least a course in statistics. To use an evocative phrase now popular: statistics is our way of taming uncertainty.

Unfortunately, there is a lot of uncertainty out and about, particularly when it comes to the seemingly endless nature vs. nurture debate, the hereditarians vs. the environmentalists. Unlike one's eye color which is clearly an effect of nature and unlike one's native language which is clearly an effect of nurture, intelligence is a characteristic that is plausibly both nature and nurture. Much blood has been figuratively spilt over the percentage of intelligence attributable to each. Roughly speaking, those comfortable with the economic and cultural *status quo* prefer nature while those who wish to reform the system favor nurture.

Creating an experiment, statistical or otherwise, for definitively settling the issue of nature vs. nurture is virtually impossible. Unlike the Nazis who performed any experiments their degenerate imaginations could conjure up, our civilized society doesn't allow us to do what good statistical practice suggests: at birth, separate identical twins and at random deposit each into the general population; at a suitable age measure intelligence. Identical twins are genetically identical and any similarity in intelligence is entirely due to nature because nurture has been eliminated, as it wouldn't be if the twins were raised together.

It often happens in statistics that when *experimental* studies are impossible, *observational* studies are useful; for example, the evidence linking cigarette smoking with lung cancer is strictly observational based as it is on retrospective data on people who choose or choose not to smoke. With respect to the na-

ture-vs.-nurture controversy, the trick is to find identical twins separated at birth because such people are rare, indeed. The man most identified with finding and testing identical twins separated at birth is Cyril Burt.

During the 1940s, 1950s and 1960s Burt found 15, 42 and eventually a total of 53 separated identical twins, by far the largest collection in the world. He concluded that the correlation coefficient for the I. Q. of these twins was .771 (maximum is 1) so that nature is four times as important as nurture (.771 is close to .8 which is 4/5 of the way from 0 to 1.) His results were often cited by hereditarians as cinching their case; Burt died in the early 1970s having been the first British psychologist to be knighted. Then the roof fell in.

Leon Kamin, at that time a professor at Princeton, in 1972 was the first to raise statistical doubts concerning the three-decimal place invariance of such a notoriously variable quantity as the correlation coefficient, when the number of the separated identical twins increased from 15 to 42 to 53. Kamin was also the first to inquire into the raw basic data itself, such as what kind of I. Q. tests were given, the names, addresses, ages at separation, adopting parents and so on; such data or even indications of where to find such information are virtually absent from Burt's studies.

Two years later, a journalist at the Sunday Times, Oliver Gillie, put things at a level much easier for the public to understand than the invariance of the correlation coefficient. So convincing was Gillie's proof that Miss Howard and Miss Conway, Burt's putative co-authors, were convenient fiction — Burt had a history of using pseudonymous articles to praise himself — that, in 1979, Leslie Hearnshaw, Burt's official biographer, concluded, "Burt was guilty of deception. He falsified the early history of factorial analysis; he produced spurious data on MZ [identical] twins; he fabricated figures of scholastic achievement."

Hearnshaw, an admirer of Burt's early achievements, put the blame for Burt's indiscretions on such later-occurring events as Burt's marriage breakdown, loss of research material during World War II and Ménière's disease (a disease of the inner ear.) In short, psychological, environmental and physical circumstances brought about a general deterioration in Burt's ability to separate the real world from the one he was fabricating. And that should be that.

But not forever. If Richard Nixon can rise from the ashes twice, perhaps it isn't so surprising that Cyril Burt can be resurrected. Two independent books came out recently claiming (according to Science) that Burt "may have gotten a bum rap." Robert B. Joynson's *The Burt Affair* and Ronald Fletcher's *Science, Ideology and the Media*, while quite different in tone tried to set the record straight. The former author ingenuously says of himself, "he had no position to defend on the question of inheritance of intelligence" before he began his investigation; the latter constructs his argument the way a feisty defense attorney would, by hammering away at those who unfairly accuse his client.

Neither Joynson nor Fletcher is very convincing as they try to show that Miss Howard and Miss Conway may have emigrated to Australia or Canada which weren't keeping records at that time. Or that rather than fraud, Burt is

guilty only of sloppiness in reprinting tables from a previous publication while in the body of the text referring to an increase in the number of twins.

The real villain in all of this is not so much Burt or his apologists but those who found his results so to their liking that they accepted them unquestioningly for so many years. These people now have a new focus of attention, the Minnesota Study of Twins Reared Apart, partly because Burt is still too hot to handle and partly because this study now has more than Burt's 53 pairs. Many papers have resulted from this research group but they are long on anecdotes and short on details concerning age of separation, age at reunion, who actually reared them — as amazing as it sounds, in previous (relatively small but nevertheless, actual) studies of separated, identical twins, many of the twins were never really separated but sometimes it is doubtful that they were identical and not fraternal! Furthermore, the raw data is not freely available to researchers who have a different agenda. And just to make the reader more skeptical, one of the main financial supporters is an organization with a clear racist intent.

One last point stressed by Kamin which is often overlooked as the nature-nurture proponents swap statistical and often highly technical attacks is the vital issue: should we be doing these studies at all? Intelligence is a very incendiary topic and bound to be misinterpreted no matter how carefully phrased in the antiseptic prose of statistics. There are some areas where science should not tread. That goes for statistics as well.

Paul Alper
University of St. Thomas
St. Paul, MN 55105

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The Left-Hander Syndrome; The Causes and Consequences of Left-Handedness by Stanley Coren. New York, NY: Free Press, 1992, 308 pp. \$24.95 (paper).

Frankly, we aren't fond of lefties. By "we" I refer to my well-over-forty squash buddies who are properly annoyed when a southpaw opponent forces us to play in a mirror-image manner. Instead of playing by instinct, we consciously have to reverse the court in which we start serving, reverse shot placements, etc. Besides, there is the nagging doubt that if your opponent can play so well left-handed, just imagine how much better he must be when using his right hand.

A larger "we" is the predominantly right-handed world designed by and for the overwhelming majority of us. Can openers, work benches, computer keypads, soup ladles and scissors are just a few of the implements most of us don't realize are inherently anti-left-handedness. As Stanley Coren in his book, *The*

Left-hander Syndrome, puts it, left-handers are invisible and thus no accommodation is made for them. That is, even though they consist of about 10% of the population, we — except for squash, racquetball or tennis players — either ignore them or pretend they don't exist.

But Coren gives evidence that left-handers have been with the human race ever since there was such a primate. Furthermore, the 10% figure has been more or less constant from the beginning and in addition, pertains to all races and regions of the globe.

At first blush, left-handers seem the least oppressed of minorities. No one is liable to paraphrase that famous pre-civil rights saying: "How would you like it if a left-hander married your daughter?" Even the most p.c.-oriented individual wouldn't dream of rising to prominence on the platform of an "Affirmative Action hiring policy for left-handers"; no bank has ever red-lined someone for being left-handed. In a world full of prejudice based on race, religion, gender affiliation and any other aspect of humanity, left-handedness would seem to be utterly unnoteworthy other than for the awkwardness in dealing with scissors, soup ladles and can openers.

Not so, says Coren citing the stereotypical traits and language we used to designate lefties: clumsy, weak ("left in English comes from the Anglo-Saxon work *lyft*, which means 'weak' or 'broken'"), illegitimate, flighty, temperamental, unlucky, wicked. According to Coren, "Both the Jewish and Christian traditions are strongly right-handed in their traditions and practices." The Hebrew word for left is related to Satan; blessings and benedictions for Jews and Gentiles are always given with the right-hand. Good is on the right, evil on the left; "the sign of the cross is always performed with the right hand, even if the person is left-handed." In fact, "handism in the form of bias against left-handers exists in virtually all the major religions to much the same extent."

Thus, we have a strange contradiction. Symbolically, left-handedness is looked down upon while people who are left-handed don't suffer with regard to employment, housing or any other socio-economic indicator. The quickest convincing proof that portsideers don't bump up against a glass ceiling is the fact that each of our last three president — Reagan, Bush, and Clinton — is left-handed. Nevertheless, Coren shows that statistically, being left-handed is a symptom that something has gone amiss.

In order to demonstrate this, he gives the reader a fascinating tour of neurophysiology and biology as well as psychology. For one thing, he distinguishes between handedness and sidedness; the two are not necessarily synonymous. For another, he discusses pawedness; animals lower on our hierarchical scale, unlike us, are about evenly divided. His treatment of the connection between handedness and the opposite side of the brain is perhaps the most interesting reading for subscribers of the *Journal of Scientific Exploration*. The title of Chapter 7, "Psycho-Neural-Astrology" indicates a debunking of the right brain-left brain cocktail conversations of non-scientists.

But the most interesting part of the book for the general public is his assertion that lefties die younger than the rest of us. He concludes this from analyz-

ing baseball data, one of the few collections of numbers which has handedness as a variable. "In effect, the data showed us that for any given age, the percentage of left-handers who will die run around 2 percent higher than the rate for right-handers." Further, "The oldest left-hander made it to age 91 and the oldest right-hander made it to age 109, an 18 year difference!"

To explain such a discrepancy, Coren invokes his used-piano analogy: a used piano may look fine but should the one key you press not move, the implication is that it is likely that some others won't either. Left-handedness is analogous to the faulty key with the other keys representing such things as decreased longevity and propensity to have certain diseases. Indeed, diabetes, Crohn's disease, myasthenia gravis, Hashimoto's thyroiditis, depression and alcoholism have a much higher incidence among left-handers.

To explain the above consequences of left-handedness, Coren postulates that difficult pregnancies and/or difficult births cause some malfunctions, one of which is left-handedness. The simple explanation that left-handedness is a recessive trait doesn't appear correct because if both parents are left-handed then the offspring is still twice as likely to be right-handed as left-handed. He held the simple genetic explanation for years and "Abandoning the idea that left-or right-handedness was a genetically variable trait was personally quite painful."

His pain represents an opportunity for others because the data is far from conclusive. And unlike I. Q. studies, left-handedness carries little incendiary baggage. Much more data-gathering and theorizing needs to be done before the constancy, causes and consequences of left-handedness can be settled. While Coren views left-handedness as a sign of pathology, we right-handed squash players might hold to a more historical, Judeo-Christian interpretation: diabolical.

Paul Alper
University of St. Thomas
St. Paul, MN 55105

Luna: Myth & Mystery by Kathleen Cain. Boulder, Colorado: Johnson Books, 1991, 202 pp. \$15.95 (paper). ISBN 1-55566-070-3. (Johnson Printing, 1880 South 57th Court, Boulder CO 80301.)

The moon appears larger or smaller depending on how we view it. Glimpse it near the horizon with trees or buildings intervening, and it looms large. View it high in the sky against a backdrop of stars and planets, and it seems small. Does the moon really change size? As unsuspecting photographers discover to

their dismay, the extraordinarily large moon is an optical illusion created when the eye and mind process the mix of lunar disk and intervening trees or buildings. Without the earthly distractions, the eye and mind shrink the moon to a more modest diameter — the skimpy diameter recorded on the developed film of disappointed photographers.

When Kathleen Cain looks at the role of the moon in human culture, she fills her field of vision with an array of intervening presumptions — personal presumptions that lead her to assign an extraordinarily large cultural role to the moon. Sympathetic to New Age and related criticisms of contemporary society, she finds in traditional lunar beliefs the hope for a regenerated cultural and spiritual order. Modern scientific society supposedly has lost crucial mystical, ecological, and female insights that our more enlightened ancestors embraced successfully in lunar myth, lore, and religion. Cain's response to the presumed problem is to "allure, enchant, and guide" by providing her readers with a truly massive compendium of past and present lunar beliefs from around the world.

The guidance takes the form, on the one hand, of didactic asides on the merits of New Age, environmental, and women's perspectives. On the other hand, it manifests itself in an overreaching thesis about the centrality of the moon in culture. Cain reports arguments not only for the pervasiveness of lunar myth in diverse societies but also for its priority: lunar worship purportedly came before solar worship in human history. Furthermore, drawing on recent writings about "the Great Mother goddess," she associates this antecedent lunar worship with distinctively female principles. While stressing that remnants of "Mother-moon" worship persist to this day (for example, in Christians' veneration of the Virgin Mary), she laments that it has been masked by myths and religions dominated by male and solar imagery. She advocates a rediscovery and re-emphasis of the lunar essence.

In caveats throughout the book, Cain calls not for the elimination of scientific outlooks but for the restoration of a balance between, on the one hand, "intuition, compassion, and feeling" and, on the other hand, "rationality, judgment, and logic of science." Indeed, she frequently invokes scientific studies in a seeming effort to legitimate legends or lore covering the moon's influence on everything from weather to the human mind. Similarly, she is quick to note parallels between scientific and mythic accounts of such events as the creation of the moon. She also carefully argues that NASA's Apollo missions do not in any way distract from the mystery of the moon.

Cain forthrightly acknowledges that the book is not a "scholarly work." In fact, it is a personal, rather eclectic montage of an incredible number of lunar myths, legends, and folk beliefs. Like a medieval encyclopedist, Cain seems intent on including every available piece of information, even if it means mingling disparate findings and juxtaposing dissimilar sources. Supplemented by illustrations, sidebars, and appendices, the main text unfolds in eight chapters with titles such as "Inhabitants of the Moon" and "Full Moon!" The chapters are packed — at times overpacked — with tidings and tidbits. In the latter cat-

egory fall a lunar interpretation of the nursery rhyme "Jack and Jill" and an explication of the ribald practice of "mooning." All in all, Cain has fashioned an overly earnest yet genial compendium of moonlore.

Albert E. Moyer

Department of History

Virginia Polytechnic Institute & State University

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Cross Currents by Robert O. Becker. Los Angeles: Jeremy P. Tarcher, Inc., 1990, 336 pp. \$19.95 (available from The Sourcebook Project, P. O. Box 107, Glen Arm MD 21057).

Subtitled "The Perils of Electropollution," this book calls for public action to protect ourselves against man-made electromagnetic fields (EMF). We must, according to its author, "act wisely and quickly," because the military and the electric-power industry will otherwise give "little, if any, consideration ... to the potential impact of [EMF] on the health and safety of the public." Furthermore, there is a "conspiracy" by the military to deny the existence of "nonthermal" effects of EMF on living creatures. By "nonthermal" effects of electromagnetic fields Becker means phenomena that occur because of EMF interference with the body's natural electrical and magnetic systems. Our Defense Department carefully allocates scientific research funds only to those projects that will "not challenge the thermal effects standard."

"Thermal" effects relate to the deposition of electromagnetic energy directly, as heat, into bodily tissues. In the U.S.A., government standards have required that such radiation be restricted to energy fluxes less than 10 milliwatts per square centimeter. (In recognition of the fact that that value may be rather high, the standard may in the future be lowered to 1 milliwatt per square centimeter.) In Russia, the standard is already 0.1 milliwatt per square centimeter. There is no mystery about the thermal effects. I allow myself a minor disagreement with Becker in this respect. I believe he is simply wrong when he asserts that (p. 233) "we really do not know exactly how microwaves produce their heating effect, even in the ubiquitous microwave ovens." But thermal effects are not the main objective of the book, as far as I can tell. Becker's emphasis on thermal effects is a distraction. It weakens his insights into the role of nonthermal phenomena.

As a radio astronomer interested in medical applications of radio astronomy and in body electricity in life systems, I found myself fascinated by the author's discussion of nonthermal effects. He finds nonthermal effects all over

the animal world. To his salamanders, frogs, dogs, and people I would add several examples from among fishes. Sharks use natural, built-in, electrical sensors to find prey buried beneath the sea bottom. Their sensitivity is microvolts per meter in salt water. There are also electric eels which, in addition to shocking their prey, generate and control highly-stable electrical oscillations. They apparently use these oscillations to recognize their colleagues in the muddy waters of Amazonia. I can easily imagine that their perception of their close-to-opaque world is an image constructed primarily out of its low-frequency (300 hertz) electrical-impedance characteristics. Tree stumps, rocks, and prey must be familiar to these animals — but in the impedance modality, not visually. Becker notes the use by bacteria and by birds of magnetic materials to orient themselves in Earth's magnetic field. This is as remarkable a story as is the use of electrical fields by fishes. Becker also cites cases of broken or amputated bones that regenerate or of de-differentiated cells of cancer that normalize under the influence of electrical fields. Without question within each of us lies a potential, either realized or unrealized, for electrical and magnetic sensitivity. DNA and a billion or more years of evolution make some life forms better at it than others. This capability is latent within all of us.

But now, as electrical technology surrounds us, "The exposure of living organisms to abnormal electromagnetic fields results in significant abnormalities in physiology and function." I believe that the issue remains undecided whether EMF is the actual peril that Becker describes. Health *Effects of Low-Frequency Electric and Magnetic Fields* (Oak Ridge Associated Universities, 1992; U.S. Government Printing Office, Pub. No. 029-000-00443-9) concludes that EMF "does not appear to constitute a public health problem." Nancy Wertheimer, cited by Becker as the discoverer of the effects of three-milligauss magnetic fields at 60 Hz on childhood cancer (p. 204), recently stated that "we don't know if there is any risk. I do think you have some kind of physiological effect going on" (quoted from *The Boulder Daily Camera*, 3 December 1992). In the same news article, Frank Barnes, a respected electrical-engineering professor at the University of Colorado in Boulder, is quoted as saying that "it is an open question whether these fields are dangerous and cause disease ... since we don't have a closed chain that shows how you get from mechanisms ... to health effects." While the respected publication *Consumer Reports* (November, 1989, page 716; October, 1991, page 697) stated that it would be prudent, for example, for pregnant women to avoid the use of electric blankets, it did not say that the EMF "peril" has been demonstrated conclusively.

Becker's is not an unequivocal demonstration. He is at best shaky about the laws of physics. It may be that I myself have overreacted to his errors. Because the same errors appear in many popular articles on the subject of the perils of EMF, even in *Consumer Reports*, I must cite at least one so that readers can be forewarned. In *CR*, November, 1989, page 715, I read "... that 60-Hz fields received scant attention until recently ... [because these] low-frequency fields transmit very little energy compared with other forms of electromagnetic ener-

gy. Unlike X-rays ... such fields can't break chemical bonds ... "This argument also appears repeatedly in Becker's book and is, I believe, fallacious. It is a mistake to say that low-frequency radiation fields "transmit little energy." Electromagnetic radiation carries energy in the form of photons. Each photon has energy strictly proportional to its frequency. The constant of proportionality is known as Planck's constant. This photon energy must be of the proper value for there to be radiative processes of ionization or excitation in individual atoms. X-radiation creates radiative ionization because its photon energy is high. Photons also describe electromagnetic radiation at extremely low frequencies, even 50 to 60 Hz, those associated with electric-power-distribution systems. At 60 Hz the photon energy is very low. But the field intensity can be very large, corresponding to an enormous flux of photons.

Both *Consumer Reports* and Becker appear to me to confuse electromagnetic radiation and electric or magnetic fields. Radiation fields are the playground, so to speak, of photons, and always involve both electric and magnetic fields. Fields of either variety can exist without the presence of radiation. Those fields do not represent propagating energy, that is, radiation. In well-designed power distribution systems the non-radiating fields should be very small and the radiating fields non-existent. Of course, these are ideals never actually achieved. The effects might nevertheless be as harmful as those of radiation and that is exactly why Becker's discussion could be useful. But, his failure to distinguish propagating from non-propagating fields makes his logic sometimes difficult to follow.

Becker was motivated to undertake studies of the effects of EMF on living organisms because, thirty years ago, he perceived a connection between human behavior and such cosmic phenomena as geomagnetic storms. It is clear that his conclusions about EMF pollution originate in his belief that evolution, and perhaps even the creation of life, was driven in Precambrian times by micro-pulsations in Earth's magnetic field. He believes that micro-pulsations "would have been particularly strong" then. He also believes that the past 25,000 years have been singularly free from great geological disturbances and, therefore, from effects of abnormal, natural, EMF. I cannot agree that the most recent 25,000 years have been free of great geological disturbances. Eleven or twelve thousand years ago the last ice age came to a close, caused by or followed by major modifications to the climate of all the continents, huge rebounds of land masses previously depressed under the burden of kilometers-thick ice sheets, and drowning of shore under seas that rose more than 100 meters. This is not irrelevant, because solar activity, the ultimate source of the energy that drives geomagnetic storms and micro-pulsations, may, in its ebb and flow, be responsible for some parts of the ice-age phenomenon. I know of no evidence that geomagnetic disturbances in Precambrian times would have been particularly strong (or, for that matter, would not have been).

In his call for action, Becker states that "the only persons who can make the final decision as to risk versus benefits [of electrical technology] are those who would be at risk." I would agree with him if risk and benefit decisions in-

volved only the people who are at risk. For example, a motorcyclist might riskily ride without a helmet but he may become involved with a car driver who, perhaps even recklessly, makes him crash. The driver or the general public may end up paying for the life-long rehabilitation of the cyclist only because he imprudently failed to wear a helmet. I argue that the general public therefore has an interest in whether the cyclist wears a helmet or not, or even whether he rides a motorcycle at all.

In the EMF case, the comparison of risk versus benefits is harder to work out. This risk has been very difficult to demonstrate. On the other hand the benefits of electrical technology are extraordinary. To propose changing this equation without compelling logic appears reckless to me. Becker writes well and non-scientists will find many of his arguments compelling. But they are superficial and ultimately unconvincing. To me his strong words seem out of place and somewhat irresponsible.

James Warwick
Radiophysics, Inc.
Boulder CO

Abstracts from the Twelfth Annual Meeting of the Society for Scientific Exploration

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Healing and the Mind: Is There a Dark Side?

LARRY DOSSEY

Office of Alternative Medicine, National Institutes of Health

Modern medical theory acknowledges that privately experienced emotions and thoughts can exert negative effects on the body. Indeed, the entire field of psychosomatic disease rests on this possibility. In addition, the power of one person to adversely affect another via negative suggestion appears quite real. When the negative suggestion is coupled with the administration of a drug or surgical procedure, this is referred to as nocebo as opposed to placebo effects. Negative suggestion is most dramatically evident in voodoo, in which actual death may occur.

The aforementioned effects can be considered "local" in nature. They are mediated by the sensory chain and are explainable in terms of known physical laws.

In addition, there is considerable evidence for "nonlocal" negative physiological effects, events which apparently operate at a distance and which are not mediated by any known sensory or physical mechanism. These include:

1. Controlled laboratory experiments demonstrating negative distant influences on simple and complex biological systems;
2. The apparent ability of negative or skeptical thoughts of medical investigators to penetrate double-blind experimental conditions to shape the outcome of the experiments;
3. "Telesomatic" events, in which noxious physical symptoms or harmful physical changes are shared at a distance by individuals who are usually empathically connected with one another;
4. Anthropological observations in which malign influences are transmitted at a distance without the victim's awareness — e.g., the practice of "death prayer" in the Kahuna shamanic tradition of Polynesia and the Hawaiian Islands.

All known medical and surgical interventions have negative side effects. This holds true also for consciousness in its interactions with the body.

There is a dark side to mind-body interactions. Negative mind-body events

operate both locally and nonlocally. The implications of this observation for the understanding of the origins and course of human illness are considerable.

Epigenetic Basis for Heritable Change in Cell Behavior

HARRY RUBIN

Stanley Hall, Dept. of Molecular and Cell Biology, University of California, Berkeley, CA 94720

Tumors are produced by cells which have a reduced sensitivity to the normal regulatory influences of the tissue and the organism. Since this altered state of tumor cells is transmitted to their descendants, it is considered a heritable property of those cells. Cellular heritability tends to be equated with genes in this era of molecular genetics, and there is no shortage of reported genetic changes in tumors. But in a general sense, the most common kind of heritable change in cells is their normal differentiation, so intestinal cells and skin cells retain their identity throughout a lifetime of multiplication though the genetic material of both is identical. Normal differentiation proceeds by a process known as epigenetics whose basic mechanism is unknown, although it is frequently trivialized as merely a matter of gene regulation. The major thesis of this paper is that cancer and normal differentiation are driven primarily by epigenetic processes that can be generalized as a response of cells to their local environment. That environment changes with the age of the organisms, with chemical insult and with other factors depending on the entire genotypic milieu. The genetic changes often found in tumor cells are evidence of further long term adjustments of cells to an altered environment, a sequence of events known as the "Baldwin effect" or "genetic assimilation."

The problem in analyzing epigenetic changes by the conventional methods of molecular reductionism is that those changes involve the entire metabolic machinery of the cell. Their complexity is so great that the resultant cell behavior cannot be reduced to a strictly causal chain of physicochemical reactions. Rather the observable behavior of the cells has to be the primary unit of measurement just as Werner Heisenberg, in the opening sentence of his classic 1925 paper founding quantum mechanics, declared he was relying "exclusively upon relationships between quantities which are in principle observable."

I will show that the heritable growth behavior of animal cells in culture is highly heterogeneous even when the cells are all the offspring of a single cell, i.e. a clone. A rapid and continuous diversification of cells is observed once they are released from the hierarchical ordering relations of the tissue and organism. It is the basis for understanding how they change in a heritable manner. The degree and kind of change in the cells depend on the long term history of their treatment in culture and the type of environmental constraint to which they are proximally exposed. The experimental results are represented in the concept of *progressive state selection* which in turn depends on the fluctuating metabolic state of the cells. These concepts can be situated in Walter Elsasser's holistic theory of organisms. Elsasser is the theoretical physicist who spent half a century developing a set of principles for differentiating the living

state from inanimate matter without violating the laws of physics. These principles provide the experimental biologist with a radically new way of ordering the most prosaic but frequently ignored aspects of growth behavior of cells and organisms. The application of this method to the problems of development and cancer will be illustrated.

The Importance of Animal Companionship: Implications to Human and Animal Health

ALAN M. BECK

*Center for Applied Ethology and Human-Animal Interaction,
School of Veterinary Medicine, Purdue University, West Lafayette, IN 47907-1243*

The human family has contained domestic animals for thousands of years. Presently, no less than 61% of U.S. households (52.5 million) have animals and more than half have more than one animal. But the sheer numbers of pet animals is only one facet of the "pet experience."

It is well documented that people denied good human contact and interaction do not thrive well. One way people can be protected from the ravages of loneliness is animal companionship. The early laboratory and specific setting observations of people with animals encouraged a period of research to identify, document, and assess the beneficial health implications of our relationship with companion animals. All indications are that companion animals play the role of a family member, often a member with the most desired attributes. Ordinary interactions with animals can reduce blood pressure and alter survival after a heart attack. Pets, for some, afford increased opportunities to meet people, while for others, pets permit people to be alone without being lonely. Elderly pet owners appear to experience less stress and require fewer visits to their physicians than non-owners. Animals play a major role in a child's development. In special settings, there are many therapeutic uses of animals that are only now being recognized.

The relationship between people and companion animals has significant implications for both human and animal health — humans and animals are part of each other's natural environment. Consequently, there are many therapeutic uses of animals that are only now being recognized.

Creativity in Biological Systems

RICHARD C. STROHMAN

Dept. of Molecular and Cell Biology, University of California, Berkeley, CA 94720

Modern biology is dominated by a molecular-genetic reductionism that leaves little room for the consideration of creativity as the salient feature of organisms. However, new experimental findings are forcing us to question the power of genetic thinking alone to provide a sufficient basis for the next step in understanding fundamental questions of life. Creativity is able to be captured

in terms of quantum mechanics (Walter Elsasser), and applied to an analysis of cellular and multicellular organismal behavior. This application is essentially an argument for an epigenetic rather than a genetic view of organisms, and places great importance on non-linear determination as crucial in approaching the enormous complexity typically found in the simplest living system. A brief criticism of linear genetic thinking will be presented in which all levels of modern biology are explored for contradictions to our prevailing bio-medical (genetic) paradigm. These levels include (a) evolution, (b) population genetics, (c) development, (d) cell biology, and (e) molecular biology.

Mathematical Investigation of Transcendent States

IMANTS BARUSS

Dept. of Psychology, King's College, University of Western Ontario

A research program concerned with the use of mathematics in the delineation of purported transcendent states of consciousness is proposed. This program has two interactive components.

Historically, a number of perspectives have arisen in the philosophy of mathematics with regard to the nature of mathematical activity. For example, intuitionists claimed that mathematics arose from the nature of the activity of the mind. One component of this research program consists of the use of knowledge concerning human cognition and techniques from clinical psychology to develop a psychology of mathematical foundations that could extend some of these perspectives. Phenomenological techniques, such as guided imagery, may lead to the evocation of ideas that help to clarify the essence of mathematics.

A second component involves mathematical modelling of consciousness. Within cognitive science, there are currently two computational models of mental activity. In one of these models it is presupposed that thinking is an instantiation of first-order classical logic. In another, known as connectionism, partial differential equations are used to model a caricature of neural connectivity. For neither of these models is it claimed that they represent actual physiological processes or phenomenological events. Rather, they are suspended in a reality between these two domains of discourse, in that they are said to emerge from neurophysiology and to be the information processing that is epiphenomenally expressed as subjective experiences. Because the qualitative aspects of consciousness would be reduced to calculations when using computational models, only restricted aspects of consciousness could be modelled in this way.

In the course of proving the independence of the continuum hypothesis from the axioms of set theory, Cohen developed a technique called forcing. The notions of sets with variable membership that were used in forcing was generalized by Lawvere. The resultant structures, called topoi, can be used as an alternative to sets for the foundations of mathematics. Because of the shift in emphasis from discreet elements to transformations of structures, and because

they embody a logic that is more general than classical logic, *topoi* could prove to be appropriate mathematical constructions for modelling experiential elements of consciousness. A first effort has been made to use *topoi* to model conscious mental acts as described by Husserl.

It is hoped that the interaction of these two components of the proposed research program will help to reveal the nature of transcendent states of consciousness. Previous efforts to use mathematics as a vehicle for understanding a purported transcendent reality are regarded with derision in the contemporary literature. It is hoped that this dialectical approach, focused on two well-defined goals, will be more successful.

Electromagnetic Field Interactions with the Immune System

JAN WALLECZEK

Neurobiological Research Program, Veterans Administration Medical Center, Loma Linda, CA

Weak electromagnetic (EM) energy interactions with living organisms have always fascinated scientists. Bioelectromagnetics is the scientific discipline devoted to the experimental and theoretical study of such interactions. One particular research area in bioelectromagnetics which is receiving increasing attention is that of the study of interactions between EM fields and the immune system ("Immune-Electromagnetics"). Because the immune system is of central importance in the maintenance of health, the possible influence of external EM fields on immune function could lead to negative or positive health outcomes depending on the type of interaction. In order to study the influence of EM energy on immune cell metabolism in a reliable manner, we have developed a new quantitative fluorescence technique termed differential real-time fluorescence spectroscopy (DRFS). With DRFS we are able to monitor real-time fluorescent changes in an EM field-exposed cell sample and in an identical isothermal control sample simultaneously. The results from these experiments clearly demonstrate that nonthermal intensities of 60-Hz magnetic fields may influence calcium regulation in cells of the immune system under certain conditions. The purpose of this presentation is (i) to review the evidence for electromagnetic field effects on isolated immune cells as well as on the intact organism and (ii) to present our own findings with human leukemic T-cells and rat T-lymphocytes. The question of the underlying biophysical mechanism will also be addressed.

Cranial Electrostimulation (CES)

MARY ELLEN O'CONNOR

The University of Tulsa, 600 South College Ave., Tulsa, OK 74104

The use of low level electrical stimulation as a treatment for a variety of ailments goes back to the ancient Greeks and their use of the electric fish, *Torpedo*. The current most accepted uses of electric therapy in the United States are

in the areas of pain treatment and bone healing. Some medical devices are available for application of low-level electrical stimulation to the cranium and they are sanctioned but not certified by the U.S. Food and Drug Administration for the treatment of insomnia, depression and anxiety. In Asia and in Eastern Europe, electrical stimulation applied to the cranium has been used for many years for situations from anesthesia to alleviation of withdrawal symptoms in substance abuse.

A literature search revealed that no double blind clinical trial had been reported for CES and that there were only thirteen (13) studies that provided enough information to be included in a statistical meta-analysis of existing information. A meta-analysis of the few available studies indicated significant effects for CES treatment of substance abusers, particularly opiate addicts. A double blind clinical trial of CES treatment of withdrawal symptoms for substance abusers is being conducted currently in Tulsa, OK. Preliminary results on approximately 50 subjects will be reported.

The Question of Biophoton Emission: Superfluous Energy, or Electromagnetic Bio-Information

BEVERLY RUBIK

The Center for Frontier Sciences, Temple University, Philadelphia, PA 19122

Acupuncture, homeopathy, and the interaction of very weak extremely low frequency electromagnetic fields with organisms are among a growing number of phenomena that have no known mechanistic explanation and continue to challenge conventional biochemical explanations of life. Indeed, they may work fundamentally at the level of the whole organism, for example, interacting with the electromagnetic properties of the whole. The phenomenon of ultraweak light emitted from virtually all organisms, also called "biophoton emission," may provide clues to a possible organizing energy field within the whole organism. Furthermore, an elaboration of energy and field considerations in biology may yield a new paradigm for biology and medicine.

Presently there are two schools of interpretation of this ultraweak light phenomenon. The biochemical or mainstream school holds that the light is simply an insignificant waste energy of metabolism. The biophysical school maintains the controversial position that the light is indicative of an endogenous electromagnetic field pervading the entire organism that acts as an emitter and receiver of the "biophotons" used in regulating life processes, i.e., electromagnetic bio-information. Each school has a number of supporting arguments that are compared, contrasted, and critically evaluated in this paper. The main argument from the biochemical viewpoint is that a number of independent, free-radical reactions within the cell, which are well characterized and known to emit light *in vitro*, are believed to contribute to the overall ultraweak biological light emission. From the biophysical viewpoint, experiments including those on cell culture communication performed in Eastern Europe, swarm for-

mation in *Daphnia*, comparisons of cancer and normal tissues, and studies supporting claims of unusual physical properties of the light demonstrate properties difficult to explain in conventional chemical terms.

One main focus of the biophysical school has been to demonstrate that the light emitted is coherent, as they hypothesize it to arise from a physically coherent state of the whole cell or tissue. Because the intensity of the light is very weak, quantum optics and photon count statistics are required to study it and analyze the data. However, there is some controversy in the physics community over the interpretation of the decay kinetics of photon distributions of induced ultraweak biological light emission.

Although there is an increasing amount of evidence to support the biophysical view that the living state may be physically coherent, more direct biological evidence is needed about the role of the light within organisms in order to resolve the controversy between the two schools of interpretation. Definitive demonstrations that the light carries bio-information and contributes to an *in vivo* communication system are critical to the biophysical interpretation.

In order to accommodate important evidence on both sides, a reconciliation between the two schools of interpretation is offered. It is proposed that the living state is neither purely chaotic nor purely coherent, but exhibits quasi-stable spatio-temporal cooperative or coherent domains that fluctuate dynamically. This viewpoint is also consistent with a growing body of evidence from other areas of biophysics in which features of life may be modelled from the framework of nonequilibrium nonlinear dynamical system theory.

Co-Operator Experiments with an REG Device

BRENDA J. DUNNE

Princeton Engineering Anomalies Research, Princeton University, NJ 08544

Experiments in anomalous human/machine interaction wherein two operators simultaneously attempt to shift the means of output distributions produced by a microelectronic random event generator (REG) yield statistically significant correlations with the operator's shared intentions. The overall results of 256,500 trials of 200 binaries each, produced by 15 co-operator pairs in 42 independent experimental series, are statistically significant and can be compared with those of a benchmark database of 2,520,000 trials generated on the same device by 91 individual operators. The patterns of achievement are characteristic of the particular operator pairs, but bear no evident resemblance to those of either of the two individuals operating separately, or to any simple combinations thereof. The composite performance of eight operator pairs of the same sex is opposite to intention, while that of seven opposite-sex pairs conforms significantly to intention, with an average effect size 3.7 times larger than that of the single operator data. Of the opposite-sex pairs, four "bonded" couples achieve average effects more than twice the size of those of three unbonded pairs and nearly six times those of the single operators. These

results contrast with those of a substantial body of remote perception data, where effects produced by agent/percipient pairs of opposite sex are smaller than those generated by pairs of the same sex, suggesting that gender-pairing is a complex parameter in consciousness-related anomalies research.

The PEAR REG Experiments: Database Structure

ROGER D. NELSON

Princeton Engineering Anomalies Research, Princeton University, NJ 08544

In order to examine its structure, a comprehensive analysis of variance has been applied to a random event generator (REG) database comprising over 5.6 million trials, accumulated in 1262 experimental series performed by 108 individual operators over a period of 12 years. In these experiments, operators attempt to shift the means of output distributions of 200-sample binary combinations drawn from random sources in accordance with prespecified intentions, using a variety of devices and protocols designed to address particular questions. Experimental designs are sufficiently similar to allow all data to be pooled in a multiway analysis of variance to examine the factor of primary interest, operator intention, and its modulation by various secondary parameters, such as individual differences among the operators, the specific device and protocol, operator location, run length, assignment mode, control mode, series order, and feedback.

These analyses confirm the existence of anomalous results with operator intention as the primary correlate, at a p-value of 2.4×10^{-4} in the full database, and reveal internal structure that is dependent on some of the other parameters. Most striking of these is a significant difference among prolific operators, supporting earlier evidence for individual operator performance "signatures." Results are very similar for a diode-based white noise source and a hardwired, deterministic pseudorandom source, indicating that the effects are not specific to a particular device. Local and remote operator databases show similar patterns of interaction with intention, suggesting that the anomalous effects are insensitive to separation between the operator and the device. Of the other factors considered, the feedback mode displays some relevance in prolific operator databases, and series number shows a suggestive pattern across operators. Random vs. volitional instruction, manual vs. automatic trial initiation, and run length differences have no significant influence in the overall concatenations, but are important determinants in some individual operator databases.

The analysis, like the experiments, has been designed to negate various skeptical explanations for the deviations from chance expectation, including machine bias, informed operational stopping, data selection, or cheating. The effect of disproportionate operator contributions can also be resolved, leaving firm evidence for a broad array of operator-specific anomalies in the experiments of this class.

Small Scale Variations in REG Data

YORK H. DOBYNS

Princeton Engineering Anomalies Research, Princeton University, Princeton, NJ 08544

Earlier work by P.E.A.R. has examined the possibility of variations in operator performance for REG series run by different operators or under different experimental conditions. The current analysis attempts to extend this by examining a comprehensive database of REG experiments for small-scale time-dependent variations within individual series. This database includes both local and remote experiments with both true random and pseudorandom noise sources. Tests against the hypothesis that REG output is consistent within any particular series are conducted at two scales, examining the possibility of variations between individual trials and between 50-trial "runs." Models of variation employed include both unstructured variations and temporally structured variations such as decline or incline effects.

Information, Knowledge and Quantum Mechanics

S. JEFFERS AND J. SLOAN

Dept. of Physics and Astronomy, York Univ., North York, Ontario, Canada

The original accounts of the double slit experiment invoked the Heisenberg Uncertainty Relations to assert the impossibility of simultaneously observing interference fringes with 100% contrast and determining, with absolute certainty, through which slit the radiation actually passed. Recent two-beam interference experiments have been carried out in which, in principle, path information may be obtained without introducing any uncontrollable disturbance in the interfering beams in the sense implied by the Uncertainty Relations. However, any attempt to extract this information from the experimental apparatus does in fact result in the reduction of the contrasts of the interference fringes. This remarkable fact is consistent with the principle that the quantum mechanical wave function represents completely what can be known about a quantum mechanical system.

It follows that, if it were possible by any means, including ostensibly anomalous means, to extract path information from a two-beam interference experiment then this would be manifest in a reduction of the contrast of the interference fringes. Measures of contrast of interference fringes may thus serve as a direct indication of whether accurate path information is being abstracted from a given experimental situation by either conventional means or ostensibly anomalous means. A simple two-beam optical interferometer has been built which employs a He-Ne laser to illuminate a double slit. Detection of the interference fringes is made using a linear photodiode array. The whole experiment is controlled by a micro-computer. Preliminary calibrations show that

high contrast fringes (contrast=95%) can be recorded with high accuracy (1 part in 10^3) and with temporal resolution of 3 seconds.

Experiments involving human operators are planned in which the operator will attempt to abstract information concerning the flow of radiation in the interferometer. This experiment should serve as a sensitive indication of anomalous phenomena and of the accounts given of such phenomena which invoke quantum mechanics.

The Pygmy Elephant: A Species Banished from Zoology

J. RICHARD GREENWELL

International Society of Cryptozoology, Tucson, AZ

By convention, there are several fundamental requirements necessary for an animal form to be accepted as a species and be included in zoological inventories. These requirements are governed by rules established by the International Commission on Zoological Nomenclature. First, it is desirable — although not absolute necessary — for a species to be represented by a full or partial specimen. It should also be subject of a scientific description in a scholarly journal, and it must be given a binomial scientific name in Latin.

The African elephant, *Loxodonta africana*, is the only currently recognized elephantid species in that continent. One subspecies, the larger bush elephant, *Loxodonta africana africana*, is found in the savannahs of East and Southern Africa. A second subspecies, the smaller forest elephant, *Loxodonta africana cyclotis*, is found in the Central African rain forests. The pygmy elephant of the Central African swamp forests was described and named *Loxodonta pumilio* by Theodore Noack in 1906 based on a living specimen which grew to only 6.5 feet at the shoulder in 11 years. Many such pygmy specimens have since been observed by Westerners, shot by hunters, and even kept alive. Native peoples regard them as a different "animal," and give them different names. Available skulls also show anatomical differences. Even so, the pygmy elephant is not included in standard mammalian reference works, and, if mentioned at all, is simply dismissed as resulting from misidentified juveniles of the forest elephant.

A new evaluation by Eisentrat and Bohme (1989) and Bohme and Eisentrat (1990) reviews the historical literature and recent evidence, and concludes that the pygmy elephant is, in fact, a separate species. In particular, a 1982 photograph clearly shows a troop of adult, tusk-bearing pygmy elephants with their own juveniles. A great white egret, *Egretta alba*, appearing in the same photo confirms an adult pygmy elephant shoulder height of about 5 feet.

The pygmy elephant appears to meet all the criteria for elevation to a species. Its banishment from zoology must therefore be due more to social than scientific factors.

UFOlogy: What do We Really Know?

RICHARD F. HAINES

Los Altos, Calif.

This paper reviews several selected areas within the field known as UFOlogy. Unidentified Flying Objects (UFO) and a broad array of related phenomena are considered by many to be one of the greatest mysteries of our time. A review of various statistics is presented first concerning many diverse features of UFO phenomena and its hundreds of thousands of witnesses. The relevant "data" is broad indeed and consists of eyewitness reports, photographs, radar contact data, magnetic and gyro-compass deviation data, ground impressions and other environmental changes, physiological and psychological changes, and a myriad of others. Computerized databases are being developed to discover obvious and subtle patterns that exist; public opinion surveys are being administered to monitor its impact upon society. Psycho-sociological investigations are being carried out by graduate students in pursuit of advanced academic degrees. Public UFO museums and traveling photograph exhibitions are being established, with the result of not only informing the public about the nature of the phenomenon but also conditioning them to accept its nature as being only of limited form when, in fact, it may actually consist of a vast diversity of forms. UFO investigators are now receiving specialized training and using computerized data recording forms. Refereed UFO journals now exist to disseminate the latest finding to the relatively small number of serious individuals who are brave enough to become involved in the field. The second area discussed is a review of selected pilot sightings. Cases will be presented which illustrate an interesting array of electromagnetic effects on board the aircraft while the UFO is nearby. The third area to be presented is that of high quality UFO photographs which have been subjected to systematic and thorough study. The UFO mystery has persisted for many decades and is likely to continue unresolved on into the future.

Patterns in UFO CE-2 Vehicle Electrical System Interference Reports

DONALD A. JOHNSON

State College, PA 16804-0621

UFO reports occasionally present evidence beyond the anecdotal accounts of eyewitnesses. Sometimes this evidence takes the form of physical traces, such as marks left on the ground or damage to trees, buildings, or motor vehicles. Sometimes reports involve physiological effects to the witness(es), such as nausea, burns, temporary paralysis, and conjunctivitis. These reports are labeled "close encounters of the second kind," or CE-2 reports. Such reports offer the best possibility of evidence for the physical reality of the UFO phenomenon.

Reports of automobile electrical system interference involving the engine, headlights, or radio are also common in this category. This paper will examine 200 reports of UFO close encounters with witnesses in motor vehicles. These 200 UFO reports were selected from UFOCAT, a computer catalogue of reports, on the basis of their completeness of data for estimates of (1) the duration of the event, (2) the size of the object reported, and (3) the distance of the witnesses from the UFO. Various statistical tests are applied to test whether these estimates are significant predictors of the intensity of the electrical system interference effect and the presence or absence of physiological effects.

We will compare these reports on a number of other qualitative characteristics, such as description of the reported UFO (shape, color, presence of light beams, presence of occupants, etc.) and characteristics of the witnesses (age, sex, civilian versus police/military). Discussion will focus on what if anything can be learned about the nature of the UFO phenomenon from the systematic treatment of such data.

UFO Abductions and Abductees: More Questions Than Answers

MARK RODEGHIER

Dept. of Sociology, University of Illinois at Chicago

Accounts of humans forcibly abducted by intelligent non-human beings from UFOs began over thirty years ago. At first such reports were viewed as anomalies within the anomalous field of UFOlogy, but over time they came to be accepted by many as an integral part of the UFO phenomenon. UFO abduction reports did not become widespread, though, until sometime in the 1980s. There is great controversy within the UFO field as to the meaning of abduction reports, their ultimate cause, and what relationship, if any, they bear to less sensational, more mundane, UFO reports.

This talk will first present a brief sketch of the history of abduction reports and their investigation. Then results of various studies will be presented, concentrating less on particular cases than on research that has searched for patterns in the reports. A typical abduction scenario will be presented (although there is much disagreement as to how commonly particular events occur during any abduction). Studies of witnesses, of the actions and appearance of the abductors, and of the aftermath of the abduction experience will be reviewed. Potential physical evidence or multiple witness abductions that may offer some special insight into the phenomenon will also be discussed. Other thorny issues such as claims of missing fetuses, implants, and the ethics of abduction investigation will be briefly considered.

Various potential explanations of abduction accounts will be examined, such as repressed sexual abuse, sleep paralysis, temporal lobe epilepsy, or alien visitors. All, though, will be found to have serious flaws, but few hypotheses can yet be discarded. The problem is that we don't know enough about abductions and abductees to discriminate among these hypotheses, so I

will be suggesting fruitful lines of inquiry, including potential crucial studies that might allow us to move forward.

Subterfuge at Roswell: Aftermath of Extremes

DONALD R. SCHMITT

Hubertus, WI

Conspiracy or cover-up; what actually took place during July of 1947 near the very military-sensitive region near Roswell, New Mexico? Conspiracy by definition is a preordained plan to carry out certain unlawful actions. On the other hand, a cover-up is a response to conceal an action or incident. And as I will clearly demonstrate, the U.S. military apparatus went to extreme lengths to cover-up the events which took place outside of the base where the first nuclear strike force was stationed.

With the first press acknowledgment by the military of the fantastic event, Washington D.C. was preparing to clamp the lid down. Newspapers from all around the globe trumpeted the banner headlines only to retract them the very next day. The unbelievable news from Roswell quickly faded with hardly a whimper into obscurity. Meanwhile, the military coyly maneuvered into position to further reinforce the cover story. Phony news-releases and articles were circulated and staged demonstrations were conducted for the benefit of the press. In the meantime, the Army Air Corps base at Roswell was at full scale alert. Unscheduled special flights streamed in and out of the facility, highly-trained teams cordoned off all access roads to secret areas where experienced personnel worked around the clock to contain the unusual evidence. Later, those involved were sworn to secrecy, and civilians, including children, were threatened with physical harm to insure their cooperation. All of this and many more drastic measures for the recovery of a very common, easily identifiable radar-reflector balloon. And so the official explanation goes. But today, 46 years after the long accepted non-event at Roswell, over 500 witnesses support the original press release— those unprecedented words which shook the world: "the Roswell Army Air Field had recovered an actual UFO."

Can Extraterrestrial Intelligence be Expected to Have Humanoid Form?

MICHAEL D. SWORDS

Natural Sciences, Western Michigan University, Kalamazoo, MI 49008

Objections to the Extraterrestrial Hypothesis in UFO research have often proceeded on absolutist deductive grounds. Two of these anti-ETH opinions relating to biology are prominent:

1. Bio-evolution is so random, and life forms consequently so varied, that the likelihood of UFO-nauts with anything like our own developmental pattern would be vanishingly small. Therefore, all reports which include

a "humanoid" body plan are *prima facie* the product of our own imaginations.

2. As a special subset of the above view, when combined with further speculations about planetary development, alien biospheres would be so different from our own, especially as regards atmospheric constituency, that no intelligent aliens could breathe our air. Therefore, all reports of humans and extraterrestrials exposed to the same atmospheres are *prima facie* erroneous.

This paper presents an entirely different view. Its thesis is that proper reading of biology and evolution (especially when considering the type of terrestrial planets likely to evolve technological intelligences) points to (although cannot prove) the exact opposite hypothesis: i.e. humanoid form is a logical, physically favored result of long-lasting ecologies, and that technological development requires a certain type of atmosphere. The argument is based upon current best hypotheses for the development of Life Zone terrestrial planets and their atmospheric evolution, as well as expected gravity burdens and energy-fluxes. It proceeds to an analysis of the usefulness (survival value) of key elements of biological forms. It questions the overemphasis in today's biology and paleontology on the randomness of evolution and the relative ignorance of the power of physics, geometry and natural selection. It also reminds us of the necessity for oxygen-creating life to allow fire-sustaining atmospheres, and thereby, land-based technology advances beyond the primitive.

Rather than seeing extraterrestrial intelligences as evolving erratically into the bizarre forms of science fiction, the author sees the general humanoid body form (for technological intelligences) to be, perhaps, universally forced. At the least, it is argued that such issues are quite open for discussion, and one is left to wonder how and why such extreme negativism can have arisen. It is suspected that the answer to the latter concern lies in the regions of bias and the sociopolitics of academe.

A Classical Electrostatic Generator, with Application to Production of Geophysical Luminosities

JOHN S. DERR

U.S.G.S. Albuquerque Seismological Laboratory, Albuquerque, NM 87115

Lord Kelvin's "water dropper," a forerunner of the Van de Graaff generator, suggests a possible mechanism for the production of geophysical luminosities seen in association with earthquakes. This apparatus transforms mechanical energy into electrical energy, using mechanical force to separate electrical charge of different sign, while pushing charge of the same sign together. Mechanical work is thus used to overcome the mutual repulsion of like charges. A simple machine will be displayed, demonstrating this effect.

In the earth, ground water pressure may force water through cracks in the rock as they are created by tectonic stress. If the electromechanical properties are analogous to the water dropper configuration, it might be possible in some circumstances for charge to be separated and electric currents generated. Kelvin's machine serves as an analog and visualization aid to help in understanding the application of the electrokinetic effect to the generation of telluric currents and possibly also to the production of earthquake lights.

Proposed Protocol for Evaluation of the Significance of UFO Reports

P. A. STURROCK

Stanford University, ERL306, Stanford, CA 94305-4055

UFO reports exist, and hypotheses concerning the UFO phenomenon exist. The problem is how to evaluate the hypotheses on the basis of the reports. This is the basic problem of scientific inference, and one may therefore attempt to set up a procedure for the evaluation of the UFO problem based on the rules of scientific inference. The basis of scientific inference is Bayes' theorem, that shows how the probability of a proposition should be changed in response to new information. There are many ways to use Bayes' theorem. One procedure, that was developed for use in astrophysics (*Ap.J.*, 182,569, 1973), may conveniently be applied to the UFO problem.

This procedure will be explained and illustrated by means of a simple set of hypotheses and a simple "interface" between the hypotheses to the relevant data. Participants in the meeting will be invited to make their assessments of the relevant probabilities on the basis of their prejudice and two simple classic UFO cases. These assessments will be combined, at a later date, to yield a collective judgment of the post probabilities of the considered hypotheses.

Any attempt to set up a formal procedure to evaluate the UFO problem can be helpful in unearthing considerations that may otherwise remain hidden. This is in itself a useful exercise, whether or not the numerical output is taken seriously.

Small Comets

L. W. FREDERICK

Dept. of Astronomy, University of Virginia, Charlottesville, VA

At the 10th annual SSE meeting L. Frank and P. Huyghe presented a paper citing evidence for frequent small comets striking the earth. The evidence was rather sparse so a systematic search was undertaken. The search covered a period of eleven months and used the search technique suggested by Frank and Huyghe as well as several other strategies which will be explained. In the search several faint asteroids were recovered, numerous pieces of space debris

were observed, several satellites and a number of meteors were recorded, but no small comets were found.

Remote Monitoring of Emotions by Oral Leucocytes: An Unreported Anomalous Event

JOHN B. ALEXANDER

Los Alamos Natl. Lab., NM

In 1985 the National Research Council (NRC) was commissioned to study Enhanced Human Performance. As part of that study, in February 1986, the panel witnessed a serendipitous anomalous event at the laboratory of Cleve Backster. Although the event was significant, no mention of it appears in their report.

Cleve Backster is best known for his early experiments involving transmission of thoughts and emotion between plants and humans. His initial findings were not only greeted with skepticism, but frequently outright hostility. Backster withdrew and continued his work in private.

Backster progressed from monitoring plants to other organisms. Eventually he studied the impact of human emotions by monitoring the oral leucocytes of the subjects. In 1983 I replicated his system at Ft. Belvoir, Virginia. We both demonstrated that a significant electrical signal could be measured when an individual experienced stress. This measurement could take place at remote distances varying from a few feet to several miles.

In the protocol a donor would provide oral leucocytes. Then a concentrated sample was placed in a small test tube. Two electrodes were placed in the tube to measure any resulting electrical potential. That apparatus was protected by a Faraday cage with wires leading to an EEG. The signal was amplified and recorded.

On the day of the meeting, I donated cells to insure the system was operating. The system ran continuously for a period in excess of two hours. When the NRC panel arrived we took them to the classroom in another wing of the building. Backster's lab assistant, Steve White, took two members of the panel into the laboratory. The apparatus continued to monitor my cells throughout this process.

Backster began to brief the remainder of the panel for about 45 minutes on the history and progression of his research. Upon his completion, I provided a quick overview of my replication of Backster's work. Approximately a minute and a half into my presentation, White ran into the room and asked what had happened "about a minute and a half ago." We noted that the time was about coincident with my initiation of the presentation.

My talk lasted about six minutes. Then the panel moved to the laboratory for the demonstration phase. Leading the group was Ray Hyman, a founding member of the Committee for Scientific Investigation of Claims of the Paranormal (CSICOPs) and NRC panel member. When I arrived he was studying

the chart which had been ripped from the recorder. The signal that had been recorded was so pronounced it did not take a signal analyst to see the difference. During the time I had been speaking the signal was several orders of magnitude stronger than anything previously recorded that day and was clearly coincidental with my presentation. If the theory is right, and I can state that "reporting craziness" to the NRC is an emotionally stimulating event, then we were remotely monitoring my state at a distance of about 300 feet.

Despite witnessing this event, the NRC did not report it. They were very critical of the entire experiment.

SSE NEWS

13th Annual Meeting: Announcement and Call for Papers

The Thirteenth Annual Meeting of the Society for Scientific Exploration will be held in Austin, Texas on June 9 - 11, 1994. This year's meeting will address three major themes, The Role of Anomalies in the History of Science, Water and Memory, and Alternative Energies. A number of outstanding invited speakers on these topics have been identified, several of whom have already accepted the Program Committee's invitation: Henry Bauer, Virginia Polytechnic Institute, Blacksburg, VA; H. D. Betz, Ludwig Maximilians Universität, Technische Universität, Munich, Germany; Edward Brame, CECOM Group Inc., Wilmington, DE; Lorraine Daston, University of Chicago, Chicago, IL; Emilio Del Giudice, Istituto Nazionale Fisica Nucleare, Milan, Italy; Harold Puthoff, Institute for Advanced Studies, Austin, TX.

Contributed papers from SSE members, on these or other topics, will be welcomed by the Program Committee. Abstracts should be sent by April 15, 1994 to:

Dr. Beverly Rubik, 1994 Program Chair, Center for Frontier Sciences, Ritter Hall, Temple University, Philadelphia, PA 19122.

Second Euro-SSE Meeting: Preliminary Announcement

A European SSE Meeting is being planned for August 24 - 26, 1994, in Glasgow, Scotland. The tentative range of topics include: Human-Machine Interaction, Alternative Energy Sources, Alternative Medicine, UFO-Related Phenomena. Program Chairman for this meeting will be Professor Dr. Suitbert Ertel, Institut für Psychologie, Gosslerstrasse 14, 2400 Göttingen, Germany. Chairman of the local organizing committee is Prof. Robert Morris of the University of Edinburgh, Scotland. Further information about the meeting will be found in the next issue of the journal.

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ERL 306
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