Journal of Scientific Exploration, Vol. 22, No. 3, pp. 415–454, 2008 0892-3310/08

BOOK REVIEWS

The Origin, Persistence, and Failings of HIV/AIDS Theory by Henry H. Bauer. Jefferson, NC: McFarland, 2007. 282 pp. $35.00 (paper). ISBN 0786430486.

> *Science, 8 February 2008*. In the wake of yet more disappointing results from human studies of AIDS vaccines last fall (*Science*, 16 November 2007, p. 1048), the U.S. National Institute of Allergy and Infectious Diseases (NIAID) plans to hold a daylong summit on 25 March to reassess how it invests the nearly $600 million it spends annually on the field. The summit, which will be webcast and open to the public, came about after 14 leading AIDS researchers sent NIAID Director Anthony Fauci a letter contending that NIAID was investing too heavily in developing products and should spend more of its budget on basic research. "The real issue is the balance that we want between discovery research and development," says Fauci. "We need to take a time out and talk to people in the field."

Awareness of AIDS emerged in the US in the early 1980s. Its existence was initially denied by public health authorities, and then in 1984 both an identification of its cause and a definitive diagnostic test were presented at (of all places) a press conference. Given the amount of scientific interest in HIV/AIDS and the wildly disproportionate amount of research funding (well more than $100 billion) that has been devoted to it, one can perhaps be legitimately puzzled by the *Science* quote above. How is it that with at least 25 years of experience with HIV/AIDS, the experts are talking about "discovery research" as a current priority?

The conventional thinking about HIV and AIDS was solidified by about 1987. HIV was a virus, invariably lethal due to its ability to destroy the immune system, and it was transmitted through exchange of bodily fluids, especially homosexual contact. Tests could accurately detect the virus, and there was a first-line drug therapy, AZT, later supplemented by protease inhibitors. The aim of the treatment was to raise the CD4 count (a certain type of T-cell that is important in the immune response, and which is specifically attacked by HIV), and to lower the viral load of HIV (the density of copies of the virus). The outlines of the public health response were clear; develop programs to identify HIV-test-positive individuals and treat them. Within a few years this expanded to plans to compel HIV-test-positive individuals to subject themselves to treatment, including routine screening and involuntary treatment of newborns.

Relatively slowly over the years the HIV dissident movement has attracted more and more adherents. In general the dissident position involves the following assertions. HIV has never been properly isolated as a virus, and so there is no evidence that it actually exists. Alternatively, if HIV does exist, then

it is not infectious, and it is not sexually transmitted at a rate sufficient to spread, no less to produce an epidemic. AIDS is a disease resulting from repeated assaults on the immune system, which eventually destroy its ability to mount an immune response to opportunistic infections. It is progressive and often fatal. The assaults are: recreational drug abuse; promiscuous homosexual behavior; repeated infections by sexually transmitted diseases followed by heavy doses of antibiotics; tuberculosis, malaria, or chronic parasitic infections; lack of sanitary water; malnutrition, and probably others. The HIV test does not test for the presence of a virus, but rather for antibodies to a variety of retrovirus particles, which are harmless, but increase in the blood as a consequence of the cellular response to the repeated assaults on the immune system. Low CD4 counts may be markers for this assault. Viral load does not actually measure the degree of HIV infection. Finally, the drugs that have been developed to treat HIV disease are toxic to cells, and kill the people to whom they are given by destroying vital cells in organs such as the liver and pancreas.

Bauer's book is one among several that have been published in order to make the HIV dissident case, to warn HIV-test-positive individuals about the dangers of prematurely taking conventional HIV therapy, and to re-focus the scientific community on the fundamental issue, which is finding the true causal pattern underlying AIDS. I found it difficult to construct a systematic review of the book, because the book itself is not organized systematically. It has the character of a collection of stitched-together essays, with the same arguments looping and weaving across multiple chapters. It is, therefore, somewhat repetitive, and while this makes it less systematic, it may be a help for those who want to skip around, reading sections that seem most interesting. Indeed, the book seems to be oriented toward the general, science-aware reader, since it draws much of its material from secondary sources.

The book is divided into three parts; "Does HIV Cause AIDS?", "Lessons from History," and "How HIV/AIDS Theory Took Hold and Kept Hold." The first part comprises about half of the volume, and will be the focus of this review. This should not be taken to imply that the second two parts are either unimportant or less interesting. Indeed, once one has accepted the HIV dissident position, they can be read independently of the first part.

A critic of the conventional HIV/AIDS theory has to face several challenges. One is that many of the issues regarding the existence and testing of HIV are highly technical. Indeed, even in the area of retrovirus research there are individuals who differ on fundamental issues that go beyond just HIV. Bauer mentions these controversies and discrepancies, but does not present them in any detail. A second challenge is that public health authorities probably rushed the decision to make HIV-test-positivity a criterion for defining AIDS, and then compounded the problem by later re-defining AIDS to be any of a fairly long list of common diseases if they happen to occur in an HIV-test-positive person, a departure from the original definition of AIDS that Bauer notes and criticizes. On the one hand, we must recognize that diseases caused by specific microbes

tend to be specifically identified with those microbes, so this is a familiar pattern in our understanding of these diseases. If AIDS is a virally-caused disease, then it does not seem unreasonable that it should follow the same pattern, and so the identification of AIDS with a specific microbe seems like a reasonable thing to do. But, on the other hand, one should require very strong evidence before making this kind of commitment. If lung cancer researchers had taken the extremely strong relationship between cigarette smoking and lung cancer as definitive, and defined "lung cancer" as a lung tumor in a cigarette smoker, then they would have made subsequent research on the relationship between cause and effect extremely difficult. They did not make this mistake, but if the HIV dissidents are right, AIDS researchers did.

Bauer does not take on these two challenges directly. He steps aside with regard to the debates among retrovirologists, and he presents very little analysis of the consequences of the changing definition of AIDS. Instead, his fundamental arguments are indirect. While this might be frustrating to the general reader, it seems worthwhile to remember that Bauer's approach is, to a certain extent, constrained by the maneuvers of AIDS researchers and public health officials in defining the disease and its cause.

Bauer's first assault on HIV/AIDS theory is to point out that HIV-test-positivity has some rather strange distributional characteristics. First, male/female ratios of HIV-test-positivity tend to be constant over time, not what one would expect of an epidemic, and they are unrelated to male/female ratios of AIDS cases, which have in contrast varied over time. HIV-test-positivity occurs at very low levels in all screened populations in the US, and its relations to age, gender, ethnicity, and calendar time are all very reproducible. Secondly, the geographic distribution of HIV-test-positivity has not shifted appreciably over the years. Thirdly, studies of the rate of sexual transmission of HIV-test-positivity give a value of about 1/1000 sex acts, very much lower than recognized sexually transmitted diseases, and not high enough to generate an epidemic. From these results Bauer makes the argument that HIV-test-positivity does not have the epidemiologic characteristics of an infectious, sexually-transmitted disease vector, and so HIV is not an infectious, sexually-transmitted virus. This seems to fight with a contention he makes later on, that HIV-test-positivity does not mark HIV, since if this were the case the epidemiologic properties of HIV-test-positivity would be irrelevant to HIV. I think what Bauer means to say is that if HIV-test-positivity marks HIV then HIV does not have the characteristics attributed to it, and if it doesn't then the HIV/AIDS theory collapses at its core.

Bauer spends a lot of time on the fact that HIV-test-positivity rates show a consistent pattern by race, with whites lower than Hispanics, who are lower than Blacks. Indeed, in some sections that start out on a different topic, the discussion keeps turning back to race. He seems to believe that because multiple data sources, with different mixtures of non-racial factors, consistently show the same race effects, this implies that there is something about race itself that is

associated with HIV-test-positivity, but analysis like this would probably not stand up in studies attempting to parse out pure race effects from the effects of factors persistently associated with race, as they are in the US.

It is not until p. 80 that Bauer gives attention to the central point, what HIV-test-positivity means. Here we find only four pages on the key issue, isolation of HIV and its identification by the HIV test. It would have been useful to have a more precise accounting and detailed data on the specific assertions made by HIV/AIDS advocates (that isolation has been accomplished), and why the dissidents disagree. Most of the provided argument is epidemiological, based on the distribution of HIV-test-positivity, and again with a section on race. There is reference to the research of the Perth group (visit http://www.virusmyth.com), casting serious doubt on the scientific procedures used to establish the HIV test, but perhaps less space is given to it than it deserves. Even though the issues here become very technical, the general reader would still like to have them explained to the extent possible, if for no other reason than because they are so important.

Starting at p. 104 Bauer argues that AIDS and HIV are not correlated. He correctly argues that the question can only arise in the period before HIV-test-positivity was taken to be part of the case definition of AIDS. The only actually correlational argument that Bauer offers is that in the very early HIV papers not all AIDS patients were HIV-test-positive, ruling out HIV as a sufficient cause of AIDS (but not ruling out that it might play a causal role in AIDS). He cites that there are HIV-test-positive non-AIDS individuals, but this kind of reasoning would also remove cigarette smoking as a cause of lung cancer. The remainder of the arguments are replays of earlier themes, involving gender, race, geography, temporal variability, and relation to sexually transmitted diseases. All of these cases are ecological (they reason about average characteristics of groups of people, rather than the people themselves), and so they provide at best indirect evidence. No actual analytic epidemiologic studies are introduced, but it is not clear whether this is by choice, or because they do not exist.

In the last section of the "causal" chapters, Bauer takes aim at AIDS in Africa. In addition to citing some of the epidemiologic anomalies that exist with US AIDS, he makes the claim that the African disease is better explained as a consequence of the relatively hard living conditions in that continent, particularly endemic diseases, malnutrition, and unsanitary water supplies. Apparently African AIDS is usually diagnosed based on the joint occurrence of a few fairly common (in Africa) conditions, and does not even require an HIV test. Bauer cites sources which claim that there are powerful pressures on African governments to find as many AIDS cases as possible, in order to attract funding from outside the continent, which would not be forthcoming for less glamorous diseases, like tuberculosis or malaria.

There are several themes that weave in and out over the whole book. One is the persistent inability of HIV/AIDS proponents to provide convincing scientific evidence for their claims. If a public health Ph.D. student wanted to study the

phenomenon of article abstracts (and the news stories they spawn) not coinciding with the results of the actual articles themselves, there is probably no richer field in which to look than HIV/AIDS. Another theme is the repeated claim by HIV dissidents that the evidence in favor of any current or past HIV/AIDS treatment is essentially non-existent. A detailed explication of this would have been welcome, and perhaps it might have gone some distance toward bolstering the anti-HIV position.

While I generally recommend reading Bauer's book, I also recommend some prior preparation. The website of David Pratt (http://ourworld.compuserve.com/homepages/dp5/aids.htm) lays out the issues in an exceedingly clear fashion, which will perhaps make Bauer's approach more understandable. Another very important site is http://www.healtoronto.com, which together with http://www.virusmyth.com fills in much of the technical issues about retroviruses, that Bauer leaves out.

Regardless of whether one ends up agreeing with Bauer or not, I think it is indisputable that he has shown that HIV/AIDS scientists have not really done their work properly, in the sense that they have left logical and scientific gaps in their development of a response to the AIDS threat, and spent too much time reviling the HIV dissidents with vituperative attacks, as if that were a substitute for rational discussion. This review is being written in advance of the 25 March 2008 summit conference cited above, and so it is too early to tell what will happen. If history is any guide, however, it will be mostly comfortable thinking inside the box, and searching for ever more intricate refinements of an overall strategy that is failing. One is left with the hope that Bauer and his like-minded colleagues will eventually succeed in provoking clarity in the HIV/AIDS debate. If they do, then they will have done a considerable service to humankind.

Mikel Aickin
University of Arizona
Tucson, Arizona
maickin@comcast.net

The Black Swan: The Impact of the Highly Improbable by Nassim Nicholas Taleb. Random House, 2007. xxviii + 366 pp. $26.95 (hardcover). ISBN-10: 1400063515, ISBN-13: 978-1400063512.

The Black Swan is part entertaining rant and part serious epistemology. Almost all of my reading time was spent smiling.

Historians of Greek philosophy sometimes tell the story of one of the first philosophers, Thales of Miletus, that he once was watching the stars, and fell

into a well. The citizens of Miletus made fun of him being an impractical philosopher. To prove them wrong, he used his knowledge to corner the market in olive oil, and made a fortune.

Not a very plausible story, but appealing to us academics. (Like Thales, we like to think we could all be rich, if we didn't have higher goals.) Well apparently Taleb is the real Thales. He wanted to be a philosopher, got rich on Wall Street using his epistemological insights, and is now using his wealth to finance his musings on whatever he cares to muse on.

Taleb's central thesis is that we systematically overestimate our knowledge, and underestimate the probability of unexpected events. The book's main title comes from the story that Europeans once thought that all swans were white, until the first black swan was sighted in Australia. As defined (and capitalized) by Taleb, a Black Swan has three characteristics: "rarity, extreme impact, and retrospective (though not prospective) predictability." (p. xviii)

The book's subtitle, "The Impact of the Highly Improbable," is a bit misleading, because a part of the book's message is to argue that what is usually considered highly improbable is actually more probable than is usually thought. This argument is expressed in various versions. One of them is to say that we think we live in "Mediocristan," although we actually live mainly in "Extremistan." Another is to argue that we apply the bell curve everywhere, even though it is an increasingly inaccurate representation of most of what is important in our world.

The bell curve implies that events away from the mean quickly become so improbable that they effectively can be dismissed or ignored. For example, when I was a graduate student in economics, we were sometimes taught that it was acceptable to delete outliers from the statistical analysis, because any observation so far from the mean was most likely due to measurement error. Such reasoning may be sensible in a world ruled by the bell curve, but it is pernicious if observations away from the mean are more common than the bell curve implies.

Taleb goes further. Not only are uncommon events more common than is usually believed, uncommon events are also more important to the history of the world, and to our practical well-being, than is commonly believed.

He argues that we attribute undue certainty to the currently-dominant theories, and accord undue prestige to the leading academic purveyors of the currently-dominant theories. We do this partly because we find it reassuring to believe that we have a high level of understanding of our world. And maybe we also partly do it because the "experts" have an interest in convincing us that the theories that the "experts" have mastered, are worthy of our time and financial support.

The book does not have a strongly disciplined focus. For example, in different parts of the book, Taleb argues for his vision of the good life (pursue erudition, do not race to catch trains, etc.); tries his hand at fiction, in recurring brief scenarios about a novelist Yevgenia Krasnova; and gives advice in personal

finance (combine safe investments, with a diversified portfolio of bets on "black swans").

So it would be no surprise if different aspects of Taleb's book end up appealing to different audiences. He writes with wit, sarcasm, and over-the-top opinions, so those who find Voltaire entertaining, may also enjoy Taleb. His belief in irreducible uncertainty, makes him skeptical of most long-term planning, and sympathetic to individual experimentation and entrepreneurship. This leads him to a skepticism toward big government and big corporations, that may be appealing to libertarians who favor an entrepreneurial form of capitalism. He also has advice for successful financial investing; most notably, to make many modest investments in low-probability, but high-payoff, Black Swans. Such advice may be of interest both to professional investors and to ordinary citizens looking to improve their personal finances.

But parts of the book are also likely to appeal to members of the Society for Scientific Exploration (SSE). According to Taleb, many academics ascribe more certainty to their models than is justified by the evidence. As a result, they find ways to ignore, or dismiss, observations that are inconsistent with the theories. Taleb contends that such observations are both more common, and more important, than mainstream theorists allow.

Taleb's explicit scientific methodology is largely Popperian. The problem of induction implies that you can never confirm theories. The best you can do is to refute false ones, by seeking observations that do not fit. Since the members of SSE take observations seriously that do not fit current theory, we are doing what Taleb says must be done to advance truth.

Although Taleb's broad arguments are relevant to SSE's aims, his specific examples are usually less so. He mainly presents examples from areas in which he has experience and interest, and these areas tend to be mainly from the social sciences, especially economics. When he does use a non-social-science example, it is frequently from medicine, often involving his praise for an obscure empirical, practice-based school of medicine associated with the names of Menodotus of Nicomedia, and Sextus Empiricus.

In a chapter section on "Inadvertent Discoveries" Taleb argues that important scientific discoveries are not usually the result of scientists finding what they are looking for, based on current theories. Instead, most important discoveries result from serendipity. If Taleb is right, then what is important for the advance of science, is that we try to expose ourselves to discrepant and unexpected phenomena, and that we be open and alert to such phenomena, when they turn out to be present.

Other respectable students of science have reached a similar conclusion. Taleb credits Sir Francis Bacon, and Arthur Koestler, especially in his book *The Sleep Walkers*. More recently, in his much-anticipated *The Dimming of Starlight*, Gonzalo Munévar argues that a primary benefit of continued space exploration is that it greatly increases the opportunity for us to encounter serendipitous discoveries.

Besides a view of the world that supports the SSE research program and methods, *The Black Swan* may be of secondary interest to SSE members for its skewering of the academic establishment. What he offers here, serves both to amuse and to provoke thought.

As for amusement, that is partly a matter of taste. I, for one, smiled broadly at Taleb's discussion of "peer cruelty," from which I extract a characteristic sentence:

> If you are a researcher, you will have to publish inconsequential articles in "prestigious" publications so that others say hello to you once in a while when you run into them at conferences (p. 87).

Taleb provokes thought through comments that are skeptical of the incentives, traditions and institutions of academia. He believes that the best science and philosophy are done by those, like himself, who understand the practical significance of their research. In contrast, he believes that many academics, like the early medical doctors, are pompous frauds, whose theories have no record of practical success. (In modern times, he especially goes after economists, whose failures to predict, result in excuses, but not in major revision of theories, or greater modesty in making further predictions.)

In the end, I fear that I have not done justice to a book that I believe is full of important, and well-defended, insights. I thoroughly enjoyed the nonlinear style of the book; and the style, arguments and evidence, produced a substantial cumulative case. But the lack of linearity, makes that case very hard to adequately summarize; or, at least, such is my excuse.

Arthur M. Diamond, Jr.
University of Nebraska at Omaha
Omaha, NE 68182-0048
adiamond@mail.unomaha.edu
www.artdiamond.com

Thinking about Gödel and Turing: Essays on Complexity, 1970–2007, by Gregory J. Chaitin. World Scientific, 2007 (hardcover). $98.00 ISBN 978-981-270-895-3; (paper) $48.00 ISBN 978-981-270-896-0.

On the occasion of Gregory Chaitin's 60th birthday, World Scientific has published a two volume festschrift. The book reviewed here is a collection of survey papers and lectures by Chaitin, showing how his thinking progressed over a period of 37 years. The other volume, writing mainly by others about Chaitin's work, is "Randomness and Complexity, from Leibniz to Chaitin", edited by Cristian S. Calude.

As a collection of papers and talks, the book is highly repetitive; Chaitin feels strongly about the importance of a few of his central results, and thus explains them over and over. But the book builds, things get clearer and new ideas enter in as the papers progress from 1970 to 2007, and the repetition does help the reader understand things better than a much shorter exposition might do.

Chaitin works at the IBM TJ Watson Research Center, for which I have the highest respect. He has had books published by such respected publishers as Springer (4 books!) and Cambridge University Press, and articles in, e.g., Scientific American. Could such a person claim not to be taken seriously, not to be listened to? But he writes in the year 2000, (p. 149–150) "But I must say that philosophers have not picked up the ball. I think logicians hate my work, they detest it! And I'm like pornography, I'm sort of an unmentionable subject in the world of logic, because my results are so disgusting."

I've been a long-time teacher and researcher in theoretical mathematics and computer science. Many of my best students have gone to work solving large, highly complex, real-world problems. I find Chaitin's work fun, interesting, enlightening, conceptually helpful–and almost useless in the real world. I hope in a limited space I can explain why I hold some of those seeming contradictory views.

In several of his talks and essays, Chaitin refers to the unsolved problem of whether P = NP. He cites it as an example of a problem that might be formally unsolvable, a candidate for being a new axiom. Bear with me a moment while I tell what this problem is about, because it is a problem of major practical importance. There are a great many problems that a computer can solve in "reasonable" time. Sorting n items on a computer can be done in time proportional to $n\ log(n)$, although some of the naive algorithms take time proportional to n^2. Harder problems take longer. If a problem can be solved in *Polynomial time*, that is, in time proportional to some fixed power of the problem size n, we say the problem is in the class P.

There turn out to be significant and practical problems that no one has found a way to solve in polynomial time. A simple example is: given a large graph (a network, with some points connected by edges) find a maximal independent set of points (a largest set of points no two of which are connected by an edge). A more practical sounding one: Given a set of cities, find the shortest set of roads that connects all of them. These problems typically have the property that if you could guess very well, you could guess the answer very quickly. Many also have the property that if you somehow knew the answer, you could convince someone it is right in polynomial time. Checking an answer is often far easier than finding it: think of finding a prime factor of a very large number. Finding the factor may take many, many, trials; checking it is a simple matter of division.

Loosely, a problem is in the class NP (the initials stand for *Nondeterministic Polynomial*) if you could guess and check an answer in polynomial time, if you made a lucky guess. There are a great many problems in this class, and many of

them are *polynomially equivalent*: that is, they are similar enough that if you could solve any one of them in polynomial time, you could solve the others too. Surprisingly, a large class of these are "at least as hard as any other problem in NP," in the sense that if you could solve any one of them in polynomial time, you could also solve the others in polynomial time. No one has ever found a polynomial time solution for any of these so-called *NP-Complete problems*, and no one has shown that there is no polynomial time solution. So, since the problems are important, people spend a lot of time seeking fast approximate solutions, or seeking solutions that are fast enough given the size of the actual problems their organization must solve. A major basic reference on NP-completeness is the monograph by Garey and Johnson [1].

The subject we have just been talking about is called *computational complexity*. Chaitin's main thrust, *algorithmic information theory*, is a little different. He is concerned not with how fast a computer program is, but with how large it is–that is, the length of the computer program, in bits or bytes. If we care about speed, shouldn't we care also about size? I'll return to that question later.

Why is Chaitin such fun to read? He asks some conceptually interesting questions, and phrases them to give unexpected and interesting answers. Some of his answers are short and simple enough that I can't resist repeating them. I can compare the joy I felt in seeing some of his ideas to the experience I had at age 11 or 12 reading George Gamow's introduction to mathematical ideas, *One Two Three . . . Infinity* [2] (as Chaitin would say, anything that has stayed in print for over 50 years deserves mention occasionally; anyone who has read this far and hasn't read Gamow's book, ought to.)

For example, you may well have encountered "random number generators" as a class of computer programs. Depending on your use of random numbers, you may need a bigger and more complex random number generator, one that passes "more tests" for randomness. Why is making a random number generator hard? Why can't you get a perfect one? One possible explanation is an insight that Chaitin reports he had at the age of fifteen.

Consider as an example of a random sequence a very long (e.g., ten thousand digit) binary string. We would consider "010101. . . .01" (the repetition of "01" many times) not random. By contrast, "010010111011001010 . . . 0" might be random. Chaitin gives a definition. Call such a string "random" if you cannot describe it much more briefly than by stating it in full. Or more precisely: if a computer program to generate the string is almost as long as the string. Then almost all numbers are random, for the simple reason that the number of short descriptions (or of short computer programs) is much less than the number of long strings.

On the other hand, you can never prove that any particular long binary string is random! The proof is only slightly more technical. Loosely, "The first string over 10000 digits that you can prove is random" would be a short description of that string, so it wouldn't be random. A more formal proof involves defining

what you mean by a "proof" (a string of symbols following certain rules, that can be checked by a specified procedure.) You can write a computer program, of fixed length, that simply examines all possible proofs (shortest ones first) until it finds a proof (necessarily very long) that some very long string is random. If there were such a proof, the program could stop and print the string in question. But then the program constitutes a description (of fixed length) of that string. As long as we set the target string length much longer than the program, we'd have a contradiction if the program ever found a proof. So we know that it never does.

Chaitin explains even more clearly Turing's proof of Gödel's Incompleteness Theorem. It uses the fact that there is no general method for determining whether a given computer program will ever stop (as opposed to running forever). Suppose we had a program *G* that can test any program and say whether it terminates. Then we could in principle list in order of length all possible computer programs that take an integer as input and either never stop, or stop and produce one integer as output. Define *F* as the function that such that *F(N) = 3* if program number *N* run with the integer *N* as input never stops (*F* can determine this by running *G*), and *F(N) = K+1* if program *N* given input *N* stops with output *K*. If we have a *G* that works, it is easy to program *F*. So a program for *F* must be in the list. But it cannot be in the list–it differs in output from program number *N* for every *N*.

Now, if we are given a finite axiom system and symbolic rules of inference, we can write a computer program which produces in order, and checks, all possible proofs. If Program *P* stops and can be proved to stop, our program will find that proof (and thus determine that *P* stops.) But we've already seen that we can't systematically check whether each program stops. So there must be some program for which there is no proof whether it stops or not. Hence there is a true statement "Program *P* does not stop" which cannot be proved using our axiom system. This is Gödel's *Incompleteness Theorem:* For any strong enough axiom system, there are true statements that cannot be proved.

Chaitin goes on–using slightly more technical strategies, he defines a number omega, Ω, which expresses the probability that a computer program of a given length will terminate. He proves (not in this book) that it is supremely random–and also supremely noncomputable. In fact he feels it is useful as a measure of how strong a set of axioms is–the more and stronger axioms you have, the more digits of Chaitin's Ω you can compute.

Ok, in what sense do I find this fun, useful, instructive, and yet of no practical value? Why are Chaitin's ideas not as well followed up by others as he might like? Four points:

One: While strict constructivism isn't much followed by many working mathematicians, those working in the real world do want a certain amount of constructibility. Would an axiom about whether P = NP help? In practice, working computer scientists act as if P is not equal to NP–that is, as if problems known to be NP-complete cannot be solved quickly, and we should look for

good approximate solutions. Making an axiom "P = NP" would not give us any quicker way of solving those problems. If making a new axiom doesn't give us a handle on a practical problem, why bother?

Two: Yes, pure mathematicians are interested in the size of axiom systems. Papers of the form "in axiom system A1, A2, A3, A4, A2 can be derived from the others", are sought after, enjoyed, and valued. One was in a recent issue of the widely-circulated *American Mathematical Monthly* [3]. Do such results have much practical importance? Not usually–they are simply fun and increase our understanding. So are concrete proofs that axiom A2 cannot be derived from the others. Are these often cited in other papers? In most cases, no. Obviously, there are critical exceptions: non-Euclidean geometry, assuming something other than the usual parallel axiom, has very practical applications in the real world. Similarly, a recent issue of the *Notices of the American Mathematical Society* [4] has an article showing how to construct undecidable problems in number theory. It helps the reader understand undecidability, but probably isn't very helpful in doing number theory. In any event, Chaitin's results on unprovability have not, as yet, found the practical applications that would make them widely valued.

Three: Does solving a problem with a small computer program have value? Again, it is often a great deal of fun. I have enjoyed assigning such things as programming exercises. Such exercises are a big help to students in understanding numerous techniques in practical programming (recursion, self-reference, time of evaluation of functions and variables). But it has never caught on as a "mainstream" area of computer science. Why? It has very little practical significance. Given a (typically large) real-world problem, there are two actual main constraints in the real world: how much time does it takes to solve, and how much space in memory or on disk does it take while being solved? With some rather technical exceptions, taking huge amounts of working space also takes huge amounts of time; so computation time is the subject on which most research concentrates. Finding a computationally fast solution to a large practical problem is of great value, so polynomial time solutions with low exponent are valued. Showing that a problem is NP-complete (and hence, presumably, not in P) shows that we should look for approximate solutions or other ways to attack the practical problem. Making the program source code a great deal smaller (as Chaitin seeks to do) is of no practical use, especially if it dramatically increases the time and space needed to run the program.

Four: Knowing that there are a great many more theoretically unsolvable problems out there, and describing a class of them, is certainly of philosophical interest, but doesn't much affect day-to-day work. Chaitin believes that it is a further justification for experimental mathematics–for example, extensive computer calculations looking for examples in number-theoretical questions that we cannot at present solve. That may be, but the people doing those calculations typically justify them by the hope of finding an example that will disprove the

conjecture, or the hope that knowing more cases will lead to an insight that will prove it. Even if Chaitin's work led them to feel it was more likely that the problem is unsolvable–that the result they seek is unprovable with our presently accepted axioms–it doesn't as yet give them tools to prove that. If Chaitin's methods are used to demonstrate that some long-standing problem is in fact unsolvable with the standard axioms, his work will get much broader attention than it does now.

In the latter part of the book, Chaitin speculates about the implications of his thoughts for biology and physics. Paul Davies, in his introduction, goes even further. Modern physics is characterized in part by the very high computational demands of some current theories. If one regards the universe as a large computer, one may believe that only a finite time and finite number of states have been available since the Big Bang to carry out computations. Can a physical law be so demanding that the required computation cannot have been carried out? Of course, applying Chaitin's ideas to this presumes that the universe is a digital, not an analog, computer–that space and time are made of small discrete quanta. Even so, we are a far cry from being able to see whether the properties of the real world required to produce the facts we care about (the "axioms", as it were) are large or small in size and complexity. (The smaller they are, the fewer and simpler the basic principles are, the less "random" the universe is.) We are even farther from being able to understand how much computer power is needed by the universe to carry out the computations. Speculation of this sort may be fun, but it is far too early in the process to regard it as part of science.

There is another sense, however, in which speculative scientists may draw support from Gödel's incompleteness theorem, and Chaitin's exposition of it. There is an image of the scientific method that is well summarized in a recent essay by Paul Grobstein in *Soundings* [5], p.10: "[I believed as a student] that there actually was a well-defined and unique set of properties and rules, the discovery of which would eventually make the mysterious and not yet understood more predictable and ultimately completely so." He goes on to explain why he became skeptical of this program as his career progressed. The discussion here suggests very strongly that *the behavior of physical systems cannot all be deduced from any finite set of rules*. For a computer with a program can, after all, be reasonably realized as a physical system: and if mathematical logic cannot tell us if the program terminates or not, we can't tell if the computer will ever stop running–unless it perhaps fails due to lack of power, mechanical breakdown, or perhaps the end of the universe. So at the scale of mechanical or electronic computing devices, our axioms systems can never be complete. Even at finer scales–so long as the system allows for a potentially infinite sequence of units of time–we cannot expect all problems to be solvable, and we can have mathematical certainty that any "complete theory" will leave some questions unanswered.

Those interested in more will find a great deal of material on Chaitin's

website [6]. The table of contents and the first chapter of the book under review are on the World Scientific website [7].

EDWARD ORDMAN
etordman@memphis.edu
Prof. Emeritus of Mathematical Sciences
University of Memphis

References

Michael R. Garey and David S. Johnson (1979), Computers and Intractability: A Guide to the Theory of NP-Completeness, W. H. Freeman.

George Gamow (1947), One Two Three . . . Infinity: Facts and Speculations of Science, Viking Press. Numerous subsequent reprintings.

Stefanie Ucsnay (2008), A Note on Tarski's Note, *The American Mathematical Monthly, 115,* 66–68.

Bjorn Poonan (2008), Undecidability in Number Theory, *Notices of the American Mathematical Society, 55,* 344–350.

Paul Grobstein (2007), From Complexity to Emergence and Beyond: Towards empirical non-foundationalism as a guide for inquiry, *Soundings 90,* 9–31.

Chaitin's website, http://www.umcs.maine.edu/~chaitin/

World Scientific website, http://www.worldscibooks.com/compsci/6536.html

Voodoo Science: The Road from Foolishness to Fraud by Robert L. Park. Oxford University Press, 2001. 230 pp. $17.95 (paper). ISBN-10: 0195147103.

Dr. Robert Park is recognized by many people as an advocate for science and for the integrity of science. He does not like to see it abused and misrepresented by unethical opportunists, and he tries to serve the public good by alerting the public to what he believes is false science. Park is also well-known for using wit, sarcasm and name-calling to make his points.

This review looks only at the parts of Park's book that covers cold fusion. In many sections of this book, Park expresses himself with derision and invective, likening himself to a hostile juvenile in a schoolyard.

The hypothesis of fusion, as the underlying process to the research ascribed to as "cold fusion," is suspect and unproved; it is now and has been from the day Martin Fleischmann and Stanley Pons made their pronouncement in a press conference in 1989 at the University of Utah. On the other hand, proof of nuclear reactions exists and has existed in this field for many years, long before Park published this book in 2001. This review will pick out important sections of his book that mention "cold fusion" and will provide related comments.

On pp. 13–14 Park writes, "Each year at the cold fusion conference there is great excitement over new results that are said at last to show incontrovertible

proof that fusion is taking place at low temperatures. Perhaps it's new evidence of neutrons or gamma rays characteristic of deuterium fusion; or helium . . . But by the time of the next meeting, many of these papers will have been discredited or withdrawn."

It is clear that when Park writes this, he is expressing his disbelief. That fact is that each year, there have been new, exciting results. Neutrons, helium and energy gains have been reported – rigorously. As far as his assertion that many of these papers "will have been discredited or withdrawn," Park does not provide any reference for this statement and it is inconsistent with the facts known to this author. Park comments at the end of his book about scientists' responsibility to society. Journalists or people who write about science also have a responsibility to society. Park's comment about these papers indicates a failure on his part to report accurately.

On p. 14 he writes: "Cold fusion is no closer to being proven than it was the day it was announced." If Park is referring to a fusion mechanism, he is correct, that theory is not proven. If he is referring to a novel nuclear reaction, he is incorrect; evidence for nuclear reactions is plentiful and was so at the time he wrote his book.

On p. 14 he writes, "These are scientists; they are presumably trained to view new claims with skepticism. What keeps them coming back each year with hope in their breasts? Why does this little band so fervently believe in something the rest of the scientific community rejected as fantasy years earlier?" And on p. 27 he speculates that they "found in cold fusion relief from boredom."

The questions Park asks are good questions. Unfortunately, he unilaterally divines the answer and assesses the researchers as merely foolish and given to fantasy. This author asked the same question but asked the researchers directly. In general, they responded that they persisted because they saw a positive result in their experiment(s), they checked their instrumentation carefully and they found no source of error. They trusted their methods and instruments, despite their own or others' preconceived notions.

On p. 18 Park makes a snide remark about his presumption that Fleischmann and Pons were ignorant about the fundamentals of their work. "How," Park wondered, "could Pons and Fleischmann have been working on their cold fusion idea for five years, as they claimed, without going to the library to find out what was already known about hydrogen in metals?"

Fleischmann began his long investigation in Pd/D effects when he was 20, in 1947, reading, among other works, that of Percy Bridgman, a Harvard professor of physics and a Nobel Prize winner. Park's comment couldn't be more wrong. Fleischmann spent his entire life studying hydrogen in metals and was awarded nearly every prize in the field. Not only that, but Fleischmann and Pons were working in a regime beyond that which was known. This is what pioneering science is all about.

On p. 18 Park talks about the inexplicable fact that the expected fusion byproducts should have killed Fleischmann and Pons, and there they were,

living proof, in direct contradiction to the presumed theoretical explanation for their results. Park conveys a related dark joke which became known as "the dead graduate student problem."

In many ways, Park is correct. For 19 years, excess heat without harmful radiation has been a consistent fact in LENR research. However, informed scientists no longer consider this a joking matter.

Park writes on pp. 18–19 how he was interviewed by NBC News chief science correspondent Robert Bazell just a few days after the cold fusion press conference. In the interview, he writes, he "summarized why the cold fusion claim had to be wrong." Odds are that Park could have been completely right. The Fleischmann-Pons claim did seem completely unlikely. However, for Park to expound unequivocally that it was entirely wrong was a risk he took in the absence of data, which was yet to be published, and direct experience.

Park wrote in his book that he suggested to the NBC cameraman that it would likely turn out to be fraud. Nobody has ever proved that Fleischmann and Pons committed fraud and the people who directly accused them of such – researchers from MIT – quickly retracted their slander in May 1989 when they came around to their senses. Why did Park not remember this when he wrote his book in 2001?

On p. 19, Park derides Fleischmann and Pons for having an incorrect hypothesis. "One reason Pons and Fleischmann had to be wrong," Park writes, "was because the number of neutrons they claimed to see was at least a million times too small to account for the energy they reported."

Fleischmann and Pons were wrong about their theory. They were correct about their main experimental results, the claim of excess heat. Do not be confused by Park's derision. Is it unscientific to be wrong about one's hypothesis? Of course not. Should Fleischmann and Pons bear shame or dishonor for this? Again, no.

"Still," Park continues, "if the experiment produced any neutrons at all, it would be proof that a nuclear process of some sort was taking place." For the record, neutrons were reported in 2007 by the U.S. Navy SPAWAR San Diego group, SRI International and the Russian Academy of Sciences. They were not the sort of energetic neutrons you would expect from thermonuclear fusion but small fluxes of low-energy neutrons, which qualifies for Park's proof of a nuclear reaction.

On p. 24 Park discusses the delicate matter of the light (ordinary) water "control" experiments. He explains their significance quite appropriately.

"Since the hydrogen atoms in ordinary water have no neutrons," Park writes, "they cannot directly fuse to form helium, which needs either one or two neutrons in the nucleus. If something you have been attributing to deuterium fusion is observed with ordinary water, it means you've been fooling yourself."

In response to critics, Pons performed an experiment using ordinary water.

"A few days later when he was asked by a reporter what the result had been," Park writes, "Pons's only comment was a muttered, 'We did not get the baseline we expected.' Apparently the experiment behaved about the same with ordinary

water as it had with deuterated water. Pons and Fleischmann would never mention the light-water experiment again."

Park guesses that Pons saw "about the same" results with the ordinary water. This is a wild and irresponsible guess. In contrast to Park's uncited reporter's response, Nature wrote on April 27, 1989, "Pons tantalized his audience by indicating that preliminary results from just such a comparison suggested an 'unexpected' production of heat in the ordinary cell."

The differences in the two accounts provide three interesting pieces of information. First, they show that Pons had the integrity to report what he knew would be perceived as bad news. Second, they show that there was early information to suggest that a non-fusion process may have been at work. And third, the representation of Pons made by Park significantly differs from that made by *Nature*, displaying a significant journalistic bias.

Park assumes that their hypothesis failed and therefore that their experiment did so as well. But Pons reported the facts as he saw them: an unexpected production of excess heat. Pons remembered the foundation of science (experiment), Park did not. In addition to their fusion hypothesis, Fleischmann and Pons had also speculated, "a hitherto unknown nuclear process." Whether it was belief or open-mindedness, Fleischmann and Pons, and those who followed them, found something, as we now know, something quite real.

Park conveys a dramatic short account of Edward Teller's protégé, Lowell Wood, who was eager to attempt the Fleischmann-Pons experiment. Wood's experiment resulted in an explosion and Park writes that "the blast shattered his apparatus and ended his quest for cold fusion." News update: Wood apparently has resumed his "quest for cold fusion." Wood attended the ICCF-10 conference in August 2003.

Park describes a congressional hearing on cold fusion on p. 94 in which Fleischmann and Pons were called to testify. "Neither admitted to the slightest doubt concerning their discovery," Park writes.

News flash: Fleischmann and Pons were skilled experts in electrochemistry and calorimetry. They knew their apparatus and methods – cold. Of course they were confident about the fundamental aspect of their claim, excess heat.

On p. 97 Park discusses the disastrous American Physical Society meeting in Baltimore, MD, on May 1 and 2, 1989. "It had not gone well for cold fusion," Park wrote, "theorists had reported that cold fusion violated not one but several accepted physical principles; chemists seemed to he able to account for all of the heat without invoking nuclear reactions; elementary flaws in the Utah experiment were laid bare."

Park implies a bastardization of the scientific method; experiments do not "violate" theory. Park, by reporting this, and the theorists, by proposing such, abused their privilege as scientists and weakened public trust. The "chemists" Park alludes to were Caltech's Nathan Lewis and Stanford's Walter Myerhof. In no way did they debunk Fleischmann and Pons' heat measurements. Both, as their main argument, speculated that Fleischmann and Pons failed to stir their

cell, which led them to a mistaken measurement and incorrect interpretation. A week later, at the Electrochemical Society meeting in Los Angeles, Fleischmann and Pons dispelled this erroneous speculation.

On p. 97 Park says he was interviewed again by NBC's Bob Bazell on the Today program a day after the Baltimore American Physical Society meeting. Park recounts his message to American public in 1989 about cold fusion, reiterated again in 2001 in his book:

"Cold fusion . . . is dead," Park writes, "but the corpse won't stop twitching. Inept scientists who had rushed to report confirmation, greedy university administrators who had tarnished the reputations of their institutions, gullible politicians who had wasted the taxpayers' dollars and careless journalists who had accepted every press release at face value . . ."

This would be an appropriate time to mention Sir Arthur C. Clarke's comment in the foreword to "The Rebirth of Cold Fusion":

"[Such] debacles fall into two classes, which I will call Failures of Nerve and Failures of Imagination." In 1989, the cold fusion controversy fitted into the second category, Failures of Imagination, which comes into play when all the available facts are appreciated and marshaled correctly but when the really vital facts are still undiscovered and the possibility of their existence is not even admitted.

"Today, the cold fusion controversy falls into the first category, Failures of Nerve; many vital facts have been discovered, yet sceptics lack the courage to acknowledge them or their immense implications."

"I never imagined," Park writes, "that a decade later there would still be scientists championing cold fusion, or that companies claiming to have developed cold fusion devices would attract investors." News flash: It's nearly two decades later and there are still champions and investors.

Park mocks James Patterson on p. 115 for his claims that his experiment showed the possibility of low energy nuclear transmutations, particularly transmuting radioactive materials to non-radioactive materials.

"That would be a surprise . . . to every nuclear physicist in the world," Park writes. "It would, in fact, be a miracle. The only way to 'neutralize' radioactivity is to transmute radioactive isotopes into stable elements, something which, to the extent that it's possible, requires intense neutron bombardment from a nuclear reactor or a powerful nuclear-particle accelerator."

Park reveals an apparent aversion to scientific discovery in this section, as well as an ivory-tower attitude which presumes that "every nuclear physicist in the world" knows all there is to know about the natural universe. Transmutation experiments now show that Patterson was probably onto something.

On Feb. 20, 2004, Lewis G. Larsen of Lattice Energy LLC presented an invited talk at a Department of Energy and Electric Power Research Institute Workshop in San Diego, CA, in front of 80 senior scientists from national laboratories, universities and commercial enterprises.

He said, "You can argue about excess heat measurements and ponder near absence of "normal" nuclear products, but transmutation experiments involving

LENRs are irrefutable." He cited the published transmutation work of Yasuhiro Iwamura of Mitsubishi Heavy Industries showing evidence of low energy nuclear transmutations. Larsen stated that there was no laughter in the room when he spoke.

Park recounts on p. 119 Fleischmann's frustration with his search for a plausible explanation.

"'What else could it be?' Fleischmann asked, repeating the claim that the heat produced was much too great to be due to chemical reactions," Park wrote. "It must be, he said, that two deuterium atoms fuse to form helium-4 by some previously unknown process that generates heat but little or no nuclear radiation."

A scientist must start somewhere, this is the hypothesis. His inquiry was honest. Park's skepticism was not; it was cynicism. Fleischmann's question then, as it is now, was a valid one. To expect a science discovery to arrive pre-packaged with a tidy explanation is unscientific.

Near the end, on p. 212, Park makes his case for his role in the betterment of science and society. "Voodoo science," Park writes, "is a sort of background noise, annoying but rarely rising to a level that seriously interferes with genuine scientific discourse. Something like cold fusion might interrupt the flow of science for a few months, but those who make extraordinary claims must eventually produce the evidence. The more serious threat is to the public, which is not often in a position to judge which claims are real and which are voodoo. Those who are fortunate enough to have chosen science as a career have an obligation to inform the public about voodoo science."

Park's book was published in 2001, 12 years after the introduction of Fleischmann and Pons' claim. Park implies that the cold fusion episode seriously interfered with "genuine scientific discourse."

Certainly the scientific discourse surrounding the cold fusion controversy left much to be desired. Could it have run a better course? Only in hindsight. On the other hand, has Park's failure to do his homework, failure to report responsibly, use of character attacks and defamation benefited the "genuine scientific discourse?" Not in this author's opinion.

Steven B. Krivit
steven1@newenergytimes.com

References

Beaudette, C. G. (2002) *Excess Heat & Why Cold Fusion Research Prevailed* (2nd ed.). City, ME: Oak Grove Press.

Krivit, S., & Winocur, N. (2005). *The Rebirth of Cold Fusion*. Los Angeles: Pacific Oaks Press.

Marwan, J., & Krivit, S. (Eds.) (in press). *American Chemical Society Low Energy Nuclear Reactions Sourcebook*. Oxford University Press.

Storms, E. (2007). *The Science of Low Energy Nuclear Reaction: A Comprehensive Compilation of Evidence and Explanations about Cold Fusion*. London: World Scientific.

Climate Confusion: How Global Warming Hysteria Leads to Bad Science, Pandering Politicians and Misguided Policies that Hurt the Poor by Roy W. Spencer. Encounter Books, 2008. ix + 191 pp. $21.95 (hardcover). ISBN-10: 1-59403-210-6.

Roy W. Spencer received his Ph. D. in Meteorology in 1981, was co-developer of the original satellite method for measuring atmospheric temperatures from orbiting satellites, was a Senior Scientist for Climate Studies at NASA, and is a Principal Research Scientist at the University of Alabama, Huntsville. He was careful to note that his research funding was from NASA, not Exxon, a frequent accusation by climate alarmists. He also notes that *no* fossil fuel company has ever asked him for help.

This book was intended to be and is humorous with irony, ridicule and sardonic cartoons. There is an index, but for the intended audience of educated non-scientists, *no* citations. A long explanation of how science works, how predictions often fail and the severe limitations of computer modeling of climate, is followed by the most detailed explanation of how weather works I have seen. Most paragraphs are quotable. From p. 37: "We scientists can usually be divided into two main camps—male and female . . . But in contrast to most humans, who must provide useful goods and services in their jobs in order to earn a living, the government-funded scientist's job is to spend your money. . . . this tends to make most scientists relatively clueless about basic economics."

Spencer explains that research on "global warming" appears to support a manmade cause because this is what the funding was intended to support, abetted by groupthink. He noted a ballyhooed 2004 study which claimed that, of 928 abstracts of articles dealing with "climate change", *none* disputed the "scientific consensus" that recent global warming, presumably from 1978–1998 (Kauffman, 2007), could be attributed to humans. He then noted that he has a stack of such non-consensus papers in his office, as do I (p. 44).

After the chapter on weather, there is one on how global warming and cooling works, with due respect to water vapor as the main greenhouse gas and the source of the Earth's thermostat by making more or less sunlight-reflecting clouds and by moving heat around by precipitating and then evaporating. Spencer's explanations are very clear. Problems in the thermometric temperature record caused by increasing urbanization of weather stations are aired (p. 83).

Chapter 5, "The Scientists' Faith, the Environmentalists Religion", goes further than any other writing I have seen on the lack of consensus on the causes of climate change. Predictions of warming made in the late 1980s were shown to have been exaggerated. "Some climate scientists act like they are doing something worthwhile for humanity by expressing alarm about global warming. Just like most environmentalists, they seem to think that risks should

be reduced through more government regulation [based on unproven assumptions] ... Several have said, in effect, 'Even if global warming isn't going to be a problem, reducing fossil fuel use is the right thing to do anyway.'" (p. 88). The unscientific decision to ban DDT use, resulting in the deaths of millions of Africans, is used a dozen times to show how "do-gooding" and political correctness kills people. On p. 93 the intimidation of climate skeptics (who now prefer the term climate realists) by climate alarmists was said to be dangerous. *Ad hominem* attacks on realists were said to accuse them of behavior as bad as genocide or tobacco smoking promotion, while a major characteristic of alarmists was said to be their difficulty in addressing evidence. Spencer than goes even further than Michael Crichton in showing how stubborn views on evil mankind causing global warming are more characteristic of religion than science (pp. 95–102). An example was given of an alarmist calling for outlawing climate change denial (p. 94). Pressure from Al Gore, the Royal Society of London, and the biases of the news journals *Science* and *Nature* are cited. Rather than attacks, segregation, and ostracism, Spencer asks: "Why not put our [climate realist] claims in the spotlight and under the microscope, and show everyone the stupidity of our positions?"

Then a chapter on Economics 101 explains that everything has a cost and a risk. More wealth means more can be done to solve problems, and that capitalism and free enterprise generate the most wealth, even if it is unevenly distributed. The market system that allows the price of oil, for example, to rise, automatically causes individuals' demand to fall. Interference causes shortages, and Spencer gives the example of California fixing electricity prices, which led to blackouts in 2000. And that the EPA makes regulations with no regard to cost (p. 120).

A chapter on the politics of climate change makes the suggestion that the federal government begin requesting climate research proposals that fall into two equally-funded groups: one to investigate climate destabilizing mechanisms (the alarmist view) and the other for climate stabilizing mechanisms (the realist view). Current peer review is said to be incestuous (p. 127). Indirectly, Spencer shows the continuing folly of having scientifically ignorant (or phobic) politicians or their appointees make policy decisions. He thinks it is desirable that politicians learn to run a business before running for elected office.

Finally, there are chapters on "dumb" global warming solutions and "less dumb" ones, even though Spencer wrote that there is no reason for climate panic. This includes the enviro and alarmist positions that *no* practical large-scale energy source is acceptable. And there is an excellent conclusion.

My only misgivings were Spencer's failure to present either the carbon dioxide assays from 1812–1965 and the non-correlated global cooling from 1940–1978 and from 1998–2008 (Kauffman, 2007), or the warming in the 19th century without much carbon dioxide from burning (Robinson et al.,

2007). But this book was written to persuade undecided non-scientists to see climate reality, for whom more facts may not be as helpful as the social commentary.

JOEL M. KAUFFMAN
Chemistry & Biochemistry Dept.
University of the Sciences in Philadelphia
kauffman@bee.net

References

Kauffman, J. M. (2007). Climate Change Reexamined. *Journal of Scientific Exploration, 21*(4), 723–750.

Robinson, A. B., Robinson, N. E., & Soon, W. (2007). Environmental Effects of Increased Atmospheric Carbon Dioxide. *Journal of American Physicians & Surgeons, 12*(3), 79–90.

Origin of Inertia: Extended Mach's Principle and Cosmological Consequences by Amitabha Ghosh. Montreal: Apeiron, 2000. 147 pp. $20.00 (paper). ISBN 0-9683689-3-X.

Mach's Principle has proven a perennial taking-off point for attempts at new physics—at one stage infecting even Einstein. The Principle has received many formulations. Mach himself apparently never wrote it down, but Philipp Frank (in lectures given at Harvard in the 1940's) attributed to Mach its verbal expression as follows:

Mach's Principle (per Mach): "When the subway jerks, it's the fixed stars that throw you down."

The point of putting it this way is that the subway is buried in the earth, and the earth's matter does not shield nor delay the instant action of the distant matter of the universe ("fixed stars"). This graphic expression sounds to me like echt Mach—it speaks directly to the physics in simple language anyone can understand, in contrast to innumerable subsequent sicklyings-o'er with the pale cast of mathematical thought. There's also a certain poetry in it, which I hope is not lost on the reader, though doubtless lost on the mathematizers. Anyone not infatuated with "accepted" physics may be forgiven the suspicion that the last word, even yet, has not been heard from Mach, the Banquo's Ghost of physics.

Among the present-day authors who agree with this assessment are Andre K. T. Assis in Brazil and Amitabha Ghosh in India. (Note that no original thinking can be expected to arise in the USA, where all physicists are indoctrinated to accept whatever they are taught as being so near the truth as to make no practical difference). Assis (1999), in his implementation of Mach's

Principle, harks back to the earlier work of the original genius of electrodynamics, Wilhelm Weber. Weber proposed a "velocity-dependent potential," from which he deduced a force law for electrodynamics that invited extension to gravity theory. Assis has shown how this theme can quantify Mach's principle and thus account for observed inertial phenomena. (Mach does not use the word "inertia," but the physical origin of inertia is what his Principle is about.) In the book under present review, Ghosh investigates a slight generalization of the Weber mathematics and shows a truly remarkable range of agreements with observation, some never previously suspected.

Mathematically, these modifications of Newton's law of gravity, which also were explored by Schroedinger, all spring from Weber, and all involve three force terms of the following characteristics: (1) The dominant term is the old standby, Newton's law of inverse square attraction. (2) The next term is smaller by a factor of c^2, varies inversely with the square of separation distance, and is proportional to the square of relative velocity. (3) The third term is likewise smaller than the first by c^2, varies inversely with separation distance, and is proportional to relative acceleration. All three terms act instantly along the line connecting two masses, in the manner of traditional Newtonian forces, and are proportional to those masses and to "big G," Newton's constant of gravity. Ghosh also introduces angular factors, of similar form in the second and third terms, dependent on the angles between the vectors involved.

Ghosh refers to the second term as an *induction* term. With all such, the question is what the "relative velocity" refers to. For Weber, who developed the first and only true "relativity theory" of electromagnetism, it referred to relative velocity of one mass (or charge) with respect to the other. Ghosh makes the same interpretation. Where the Machian idea of relevance of the distant matter of the universe comes in is through an integration over all space of a model universe, considered infinite, homogeneous, and isotropic. My impression is that such integrations, performed also by Assis, were pioneered by E. Schroedinger (1925). They conform to the concept of a *fundamental frame* defined by the center of mass of the universe. This fundamental frame idea is quite similar to Newton's conception of absolute space. It benefits from the Berkeley-Mach critique of the latter mainly on the plane of verbalism. The same could be said of Einstein's substitution of "geometry" or "relativistic ether" for Newton's "space." Einstein, incidentally, had to retreat from his original optimistic intention to implement Mach's Principle, since his "geometry" turned out not to support it. Ghosh's approach, in contrast, does support it very well.

In both Ghosh's and Assis's treatments, the acceleration term (3) accounts for the $m\vec{a}$ in Newton's $\vec{F} = m\vec{a}$, because the $1/r$ dependence of this term causes it to dominate at long ranges over all $1/r^2$ effects. Thus Newton's second law is not a postulate but a deduction from the gravity force law, and inertial systems (as well as Mercury's precession, etc.) are tied to an identifiable-in-principle Ur-inertial system, the mass center of the universe. And Mach was right, that the fixed stars do throw you down, by their $1/r$ action.

Having accepted this about the third term, Ghosh devotes most of his book to exploring examples of action of the second, or inductive, term, proportional to v^2. This gets into much mathematics, which the reader may find tedious. However, the results, and the sheer variety of independent phenomena he finds supporting this term, are most impressive. His industriousness is exemplary. I shall not attempt to list the confirmations he finds for gravitational induction (ranging from cosmic drag, to gravitational redshift in a non-expanding universe, matter-matter interactions, galaxies-sans-dark-matter, etc.) It is a phenomenon unrecognized in traditional physics . . . although General Relativity offers certain "drag" effects of the same general nature but of less physical pedigree, since they refer to a pure geometer's notion of "frame." Ghosh's confirmations are obtained without fiddling adjustable constants—the bane of most new theories, as well as of such established verities as the physicists' Standard Theory, responsible for the costly Higgs hunt.

Ghosh must be credited with a most impressive accomplishment. There is a great deal of smoke coming from the Machian woods, and I for one am ready to believe there is fire there. Weber would be proud to see what has come of the sticks he rubbed together.

THOMAS E. PHIPPS, JR.
tephipps@sbcglobal.net

References

Assis, A. K. T. (1999). *Relational Mechanics.* Montreal, Canada: Apeiron.
Schroedinger, E. (1925). ''Die erfüllbarkeit der relativitätsforderung in der Klassischen mechanik.'' *Annalen der Physik, 77,* 325–336.

Muses, Madmen, and Prophets by Daniel B. Smith. Penguin, 2007. 254 pp. $16.00 (paper). ISBN 978-0-14-311315-7.

In the old days, if you sought professional help from a psychiatrist, he would talk to you himself and find out many details of your life, your experiences, loves, relationships, ambitions, failures, troubles, sorrows, and joys. In fact, he would get to know you as the real person you are. If you seek such help from a psychiatrist these days, you won't find much of that. Your symptoms will usually be elicited by a nurse ticking off boxes on a form. This will be evaluated impersonally by finding the closest diagnosis by the current Diagnostic and Statistical Manual (DSM) criteria; from which the appropriate medication will be prescribed. The psychiatrist will be allotted a fraction of an hour by the financial management of the hospital to talk to you. Modern pharmacotherapy certainly does wonderful things.

However, in this streamlined process, devised by accountants, something is lost. Daniel Smith's book is written to draw our attention to one aspect of this loss—the current faulty evaluation of auditory hallucinations by mainstream psychiatry. He rightly states that auditory hallucinations occur in sickness and in health, and, in either event, simply using them to stigmatize normal people as "insane", or in patients as data to check off on a DSM V checklist is not enough.

Smith's own father and grandfather had experienced auditory hallucinations much of their lives, although they were clearly not psychiatrically ill in the ordinary sense. Nevertheless they endured much pain through the general lack of understanding of what was going on. This situation made three demands on Smith when he decided to try to understand it—and by extension—such events in the lives of humans in general. The first demand was intellectual. Why do people hear voices? What does it mean? The second demand was emotional. These experiences of voices "is woven into their sense of self, whether they want it to be or not." The third demand was moral. People who "hear voices" tend to come "in stark and tragic conflict with the culture of their times." How can this be avoided?

There are many types of auditory hallucination. Smith first gives an excellent general account of the psychology, and religious and social history of auditory hallucinations. He then concentrates on the cases of people, as in his own family, whose voices simply tell them what to do. He gives rich and scholarly accounts of four people to illustrate his points—Achilles, Socrates, Joan of Arc and Daniel Paul Schreber—whose voices had a notable impact upon their times.

He then returns to modern times and describes the National Empowerment Center, whose mission is "... to carry a message of recovery, empowerment, hope and healing to people who have been diagnosed with mental illness." In particular they have a "Hearing Voices Curriculum" that teaches mental health personnel what it is like to hear voices. Other similar (international) self-help organizations—the Foundation Resonance and the Hearing Voices Network—arose out of the work of a Dutch psychiatrist, Marius Romme, and his partner Sandra Escher. Their message is that simply to give the stigmatizing and harmful opinion to the patients that their voices are merely "pathological", and must to be eradicated as one would a cancer is inadequate: particularly as many people with "voices" show no other signs of mental disturbance. This attitude ignores the ineradicable importance the voices play in these people's lives. Romme and Escher found that the people who coped with their voices best were those that found ways to "bring their voices in line with their experiences. Those who tried to escape suffered the most." Similar conclusions have been based on studies carried out in England by Max Birchwood and Paul Chadwick, and by Anthony Morrison. As a retired neuropsychiatrist myself, I fully agree with all that Smith says.

My only criticism is his support (p. 23) for the metaphysical mind-brain Identity Theory—the current fashionable dogma in neuroscience and psychol-

ogy. He says that no cognitive processes or mental events (thoughts, beliefs, emotions, etc.) "can exist without the brain." Thus he is saying that brain events are both *necessary* and *sufficient* for mental events to occur (for which there is actually no evidence). In the next sentence he puts forward a different theory: "No matter our religious beliefs, the unassailable physical fact is that the [mental] phenomenon must have a neurological *correlate* that, in theory, we can explain, chart, and describe." This theory entails that brain events are *necessary*, but not *sufficient*, for mental events to occur—since if A is a *correlate* of B, A cannot be B. And this is all that the evidence allows us to support. In terms of the discussion about where "voices" come from, this correlate theory means that they may be (and usually are) generated by brain process. Alternatively they may represent an example of a failure of Aldous Huxley's (1954) "reducing valve" function of the brain (based on ideas put forward by C. D. Broad [1953] and Henri Bergson), whereby the brain does not only represent external reality but normally also excludes unwanted mental sensations.

Another metaphysically dubious comment is contained on p. 28 in which he says, "The pain, if we choose to name the sensation of and by itself, is always in the brain." This is certainly an improvement on the faulty "common sense" mistake of locating the conscious sensation in the physical body, when of course the neurological evidence shows that the pain is actually located in the body image and not in the physical body. But to state that the body image itself is literally located in the brain is not a scientific fact but a metaphysical assumption. Events in the brain are the neural correlates of events in consciousness.

The book is beautifully written and every page has something of interest. It should be on the "must read" list of every psychiatrist and all others in the mental health system.

JOHN SMYTHIES
Center for Brain and Cognition
University of California at San Diego and
Institute of Neurology
University of London
smythies@psy2.ucsd.edu

References

Bergson, H. Matter and Memory. London: Allen and Unwin, 1962.
Broad, C. D. (1953). *Religion, Philosophy and Psychic Research*. Routledge.
Huxley, A. (1954). *The Doors of Perception*. Chatto and Windus.

The Head Trip: Adventures on the Wheel of Consciousness by Jeff Warren. New York: Random House, 2007, 400 pp. $24.95 (cloth). ISBN: 978-1-4000-6484-7

This is a book by a journalist who investigated various alterations of consciousness by reading about them, interviewing experts, and, in some cases, deliberately trying to experience them for himself. In the last category we find hypnagogic states, fragmented sleep, lucid dreaming, hypnotic trance, specific biofeedback induced states, and transcendent states, which are discussed in considerable detail. Less attention is paid to other sleep phenomena, hypnopompic states, daydreaming, and some instances of flow. Absent are drug-induced states, some of the more dramatic dissociative states, and altered states associated with psychopathology or death. Overall, the first part of the book is about sleep and sleep-related phenomena, whereas the second part is concerned with waking phenomena or phenomena initiated from a waking state. The book terminates with a conclusion chapter, an epilogue in which the author presents his own typology of consciousness, extensive notes documenting his research and sources, a bibliography, and an index.

Overall I liked this book, and there was a lot to like about it. The factual information itself, as far as it goes, is reasonably accurate, and that is no small feat in an area of investigation that is as fragmented as consciousness studies. The author's efforts to experience alterations of consciousness for himself made the book more interesting to read. For example, he spent 20 nights during one autumn in his family's cabin in the wilderness in order to determine the extent to which sleep remained consolidated upon going to bed at sundown. He interviewed numerous experts, in some cases luminaries, such as Allan Hobson, Herbert Spiegel, and Charles Tart. There are extensive footnotes at the bottoms of many pages with good discussions elaborating upon the body of the text or taking it in interesting directions. The author has included numerous drawings that he made in order to illustrate the written material. He also created an irreverent persona for himself with a breezy writing style that makes the book easy to read. And the book has been meticulously prepared for publication, so that, for example, I did not find a single typing mistake.

My main criticism is that Warren too readily embraces mainstream ideology and avoids dealing with anomalous phenomena, which occur sporadically but frequently in altered states. For instance, on page 332 he states that, during sleep, "the mind is immersed in a model of the world built from memory" in contrast to "a model of the world built from sensory input" during waking. But there are reasonably well documented cases of anomalous perception during sleep, e.g., in the form of precognitive dreams, so that our models of the world during sleep are not entirely dependent on memory. He could have tried to interview Stanley Krippner, for example, about the famous Maimonides dream experiments, in which reasonable evidence for precognitive dreams was obtained. Failing to address anomalous phenomena is a serious omission.

However, this book is a good conventional introduction to altered states, which can provide the reader with a context for considering the more unorthodox manifestations of consciousness.

Imants Barušs
Department of Psychology
King's University College at The University of Western Ontario
baruss@uwo.ca

The Witch in the Waiting Room: A Physician Investigates Paranormal Phenomena in Medicine by Robert Bobrow. Da Capo Press, 2006. 256 pp. $15.95 (paper). ISBN 1560258144.

In 1997 an English housewife heard a voice in her head one evening when she was quietly reading at home. "Please don't be afraid," the voice said politely. "I know it must be shocking for you to hear me speaking to you like this." The voice explained that it was only trying to help, that the poor woman had a brain tumor and should immediately seek a CAT scan at a certain London hospital. The panicked lady called her psychiatrist who quickly diagnosed "functional hallucinatory psychosis" and loaded her up with anti-psychotic medication.

But the voice persisted, the woman insisted on a scan, and you can guess the rest. Neurosurgeons spotted something suspicious, they opened her skull, and discovered a meningioma brain tumor the size of an egg. When she awoke from anesthesia, the voice spoke one last time. "We are pleased to have helped you. Goodbye."

Her experience is just one of many puzzling, health-related, paranormal experiences Dr. Robert Bobrow M. D. describes in his delightful, thought-provoking book *The Witch in the Waiting Room.*

More surprising than her bizarre story is the fact that the respected, mainstream *British Medical Journal* published it. Bobrow offers skeptical colleagues sober reports describing a plethora of "paranormal" experiences patients share with their physicians and psychiatrists – voodoo spells, telepathic dreams, déjà vu, acupuncture and hypnosis cures, self-predicted deaths, energy medicine cures and faith healings, near death experiences – all drawn directly from refereed medical journals accessible through MEDLINE, an internet database and "our profession's Gospel, from which all our knowledge derives, from which our textbooks are largely written." This cabinet of curiosities deserves exploring by the medical profession, he argues. Patients' paranormal beliefs and experiences can directly affect their mental and physical health; and the anomalies themselves suggest new avenues of research which may advance medical science.

MEDLINE stubbornly refuses to index leading anomalies journals like the *Journal of Scientific Exploration* or the *Journal of Parapsychology*, depriving Bobrow and his readers of a wealth of additional evidence. But the paradigm-changing work of a number of luminaries in anomalies research still manages to sneak into the medical community's canonical literature – Ian Stevenson's reports of childhood memories and birthmarks suggesting a past life, and Bruce Greyson's near death experience scale (*Journal of Nervous and Mental Disease*); Dean Radin's psi studies using EEGs (*Journal of Alternative and Complementary Medicine*); and Michael Persinger's one-theory-fits-all attempt to use the earth's magnetic properties to explain everything from poltergeists and UFOs to sightings of the Virgin Mary (the journal *Perceptual and Motor Skills*).

Bobrow's writing style is crisp, but his topic selection quirky. He devotes a chapter to lycanthropy, describing patients with "species identity disorder" who believe they're wolves, cats, birds or gerbils. But he oddly fails to cover patients who claim alien abduction experiences, courageously investigated by the late Harvard psychiatry professor John Mack; or the landmark surveys of death bed visions conducted by Osis and Haraldsson. Surely physicians encounter these paranormal claims more frequently than werewolf confessions. And why no reference to Michael Murphy's classic exploration of extraordinary human potential, *The Future of the Body* (Tarcher/Putnam Publishers, 1992)?

Still, the author's cauldron bubbles with a heady brew of odd, unsettling experiences worthy of more stirring and tasting by a Western medical establishment bewitched by hubris and scientific reductionism.

MICHAEL SCHMICKER
Honolulu, Hawaii
schmicker@navatekltd.com

The Trial of God by John Smythies. BookSurge Publishing, 2006. 90 pp. $12.99 (paper). ISBN 1419629476.

"Voltaire" and "the Archangel Michael" prosecute and defend, respectively, the God Jehovah, calling witnesses from the scientific and political world, in a brief and moderately entertaining work by the distinguished neurologist and philosopher John Smythies.

Although "Voltaire" declares that his case is to be mounted against the God depicted in the Bible, his principal charge is the familiar one, that this world is too full of pain and misery to be the creation of any half-way decent deity. The equally familiar answer, that it needed to be a troubled and troubling world to permit the evolution of beauty, and the exercise of human

wit and courage, he dismisses with the remark that, even if this were so, far more of the world could be merely virtual, a pretence, without the millions of years of misery we now presume existed. "Michael" replies, alarmingly, that the Creator is an artist, loving blood-and-thunder epics, and sometimes dissatisfied with how boring His creatures have turned out to be (which is why, so "Michael" speculates, He wiped out the dinosaurs). In any case the death, even the troubled death, of innocents is less significant if they have immortal souls.

The most interesting and valuable sections of the play are devoted to John Smythies' answers to currently fashionable materialist analyses of mind. Each of us inhabits a virtual reality, constructed for us (maybe) by our brains in response to material stimuli, and not *identical* with the material world we think we see. These virtual worlds exist, as it were, at right angles to the ordinary material dimensions, and "consciousness modules" might migrate to other worlds and bodies on the death of one.

"Michael" grows more exasperated with Voltaire's witnesses ("Darwin", "Freud", "Crick", "Marx" and "Machiavelli") as the play progresses, and himself calls on "Aldous Huxley", "Mohammed", "the Archangel Gabriel", an unscrupulous young Essene conjuror called Jesus, and finally "Winston Churchill". Darwin and Marx between them, so he suggests, were responsible for much of twentieth century misery. Oddly, the whole discourse is gradually transformed into a sort of defense of President Bush's war against Saddam Hussein (after a brief diversion into the mad idea that Bush staged the whole thing so as to create a Fascist theocracy in the USA), and the question of the God Jehovah's guilt is left undecided. Even "Voltaire" proclaims himself a theist or a deist of some sort, half-persuaded that a moral monotheism is a necessary social tool to keep the peace, and even that indeed our souls, our consciousness modules, are immortal.

It is perhaps a pity that, by imagining Jesus as an Essene conjuror (admittedly with fairly good intentions), Smythies has ruled out the one distinctively Christian answer to the charge against the Creator: namely that He did at least accept the rules He had made and "was obedient to death, even the death of the Cross". Personally, I find that a more companionable and decent deity than "the Boss", the blood-and-thunder artist that the archangels of this fable serve. Nonetheless, the fable is worth reading, and especially so for its rebuttal of monistic materialism.

Stephen R. L. Clark
University of Liverpool
srlclark@liverpool.ac.uk

FURTHER BOOKS OF NOTE

Do You See What I See? Memoirs of a Blind Biker by Russell Targ. Hampton Roads Publishing Co., 2008. 296 pp. $23.95 (hardcover). ISBN 1571745599.

Russell Targ co-developed with Hal Puthoff the clairvoyant technique of remote viewing that spawned the U. S. military's secret, 20-year, $20-million "psychic spy" program called Star Gate (*Journal of Scientific Exploration*, Spring 1996). Targ's impact on psi research rivals J. B. Rhine's, but Targ's personal life appears infinitely more colorful, based on this tell-all autobiography. The book is a grand goulash of Jewish family history, childhood memories, marriages, romances and affairs, travels, liberal political opinion, and Eastern spiritual-philosophical musings, spiced up with some serious name-dropping (eccentric chess great Bobby Fischer was Targ's brother-in-law; his publishing industry father William Targ discovered Mario Puzo, advancing him $5000 on the basis of the plot that became *The Godfather*; Russell and Russian-born novelist/libertarian philosopher Ayn Rand quarreled over Einstein's theory of general relativity at her mid-Manhattan salon haunted by future Fed chairman Alan Greenspan; the young actor Alan Alda lived across the hall from their apartment.). Targ's pioneering psi experiments receive second billing here. You will find them covered much better in scientific papers and other books he has co-authored: *Mind Reach, Miracles of Mind, Limitless Mind*. This book is instead an uneven but ultimately enjoyable celebration of a legally-blind, 74-year-old former Cub Scout, magician, Columbia University drop-out, physicist, optical engineer, drug-experimenter, ESP researcher, lover, book publisher, biker, song-writer, treasure hunter and truth seeker who is still seeking. May we all enjoy and accomplish so much in our allotted time.

MICHAEL SCHMICKER
Honolulu, HI
schmicker@navatekltd.com

Ancient Stone Sites of New England and the Debate Over Early European Exploration by David Goudsward. McFarland, 2006. 247 pp. $32.00 (paper) ISBN 0786424621.

New England is well-dotted with old stone chambers and walls plus a bounteous assemblage of mysterious petroglyphs. Did early Norse visitors leave these ruins behind—or perhaps even earlier European adventurers? Or, as most professional archaeologists would have it, everything is merely the work of post-Columbian settlers.

Goudsward's book combs the relevant literature, most of it almost impossible to find today. This unique compilation contains many photos of classic sites, inscriptions, and the people involved. The contents are invaluable to an anomalist.

Some of the "classic sites" covered: Dighton Rock; the Newport Tower; Mystery Hill; the Gungywamp Complex; and the fabled "Norse" city of Norumbega.

William Corliss
Sourcebook Project
Glen Arm, Maryland
science-frontiers.com

Outside the Gates of Science: Why It's Time for the Paranormal to Come in from the Cold. By Damien Broderick. Thunder's Mouth Press, 2007. 368 pp. $16.95 (paper). ISBN 1560259868.

Psychologists, physicists, astronauts, engineers, clergymen, medical doctors, and magicians have all pontificated on psi, so why not a sci-fi writer? After all, who else spends as much time at the far edges of science with such a wide open mind? Author Damien Broderick is acclaimed for his science fiction (*Godplayers, K-Machines, Schrödinger's Dog)* and futurist musings. Here, he tackles the paranormal, devoting the first half of his book to recapping the scientific evidence for ESP and PK. Dean Radin and Richard Broughton do a better job of it in my opinion, but Broderick reaches the same conclusion: the phenomena "point to some central failure in the way reality is represented by orthodox science." He spends his remaining ink tramping through the thicket of some half-dozen theories–from fraud, to quantum physics, to Decision Augmentation Theory–which attempt to "knot together psi and the rest of physics." Broderick raises but ignores the possibility of a spirit surviving physical death (e.g., is a poltergeist RSPK, or the dead at work?); ditto divine intervention. "Let us keep gods, demons and tricksters at bay as the hypothesis of last resort." His musings are thus disappointingly drier than might be expected from a science fiction author, despite the occasional, delightful allusion to the Matrix, Akashic Records, and H.G. Wells.

Michael Schmicker
Honolulu, HI
schmicker@navatekltd.com

From Other Worlds: Aliens, Abductions and UFOs by Hilary Evans. Carleton Books Ltd. 1998. 224 pp. (hardcover; out of print). ISBN 1858685141.

This book is one of the more recent in a long series of picture volumes on the notion of strange visitations to our planet by beings from other worlds. Evans carries us on a visual journey from ancient times to the present, combining authentic early artworks of alleged flying saucer and alien imagery in combination with attractive modern depictions where photography was lacking. The quality of the illustrations is generally very good.

Sometimes there is difficulty telling the genuine artworks from the modern illustrations of older incidents due to the lack of references for most of them. For example, it would have been nice to know from where the "Aliens from Nice" discussion came since it related a rather remarkable story of an alien encounter in 1608. Carl Sagan once thought in his early discussions of extraterrestrial life how it seemed possible aliens might have visited the planet in the distant past, but that it was certainly not happening in the modern era. The Nice encounter might have caught his attention, assuming that it was based upon more than folklore.

The book remains a colorful introduction to the visual history of otherworldly aliens and the UFO experience, as told by a long time researcher into the phenomenon.

Barry Greenwood
uhrhistory@comcast.net

ARTICLES OF INTEREST

"The Near-Death Experience: A Cerebellar Method to Protect Body and Soul – Lessons from the Iboga Healing Ceremony in Gabon" by Süster Strubelt and Uwe Maas, *Alternative Therapies in Health and Medicine, 14*(1), 2008, 30–34.

The authors of this article marshaled an impressive body of neurophysiological data and constructed a plausible hypothesis to explain Iboga rituals and near-death experiences. However, they did not establish that we know enough yet to answer their first question: "Are Iboga visions and near-death experiences based on a common neurological mechanism?"

The authors noted that the Gabonese Iboga ritual elicits experiences with many of the features and aftereffects of spontaneous near-death experiences. The Iboga and near-death experiences are not identical, however. For example, Iboga ritual participants maintain the ability to communicate verbally throughout their visions, whereas spontaneous near-death experiencers usually lose communication with the physical world around them; and the transformative effects of the Iboga ritual are dependent on weeks of continuous emotional processing by healers, whereas the effects of near-death experiences follow the experience itself. It is unclear whether these distinctions are merely a matter of focusing of attention or whether they reflect fundamental substantive differences between the two kinds of experience.

The authors noted that ischemia is thought to release potentially neurotoxic glutamine, leading to some unknown neuroprotective mechanism that through some unspecified pathways may produce near-death experiences. They further noted that ibogaine, through unknown mechanisms, may also induce glutamatergic excitotoxicity, and they hypothesized that the glutamatergic toxicity that may be induced by ibogaine provokes the same neuroprotective mechanisms that are presumed to occur in ischemia.

This is a promising hypothesis, but there remain gaps in the story. First, glutamine release has never been demonstrated in near-death experiences. Second, we have no idea how the putative but unidentified neuroprotective mechanism might produce near-death experiences. Others such as Karl Jansen have speculated that an endogenous ketamine-

like neurochemical may play a role, but no such chemical has yet been found. Third, it is unclear how a neuroprotective effect that can be observed for weeks following focal ischemia could account for an experience that lasts only seconds to minutes. And fourth, this hypothetical ischemic neuroprotective mechanism would not explain the majority of NDEs that do not involve ischemia, such as in falls or automobile accidents.

The authors cited in support of their hypothesis a recent study by Dennis Schutter et al purportedly inducing an out-of-body experience by transcranial stimulation of the cerebellum. In fact, during a relaxation period following the stimulation (but not during the stimulation itself) Schutter et al's subject reported a sensation as of "her body falling/drifting side wards and even out of the chair." That is, relaxing after the electrical stimulation induced an illusory feeling of the body moving, but it did not produce a feeling of leaving the body. Thus this interesting kinesthetic illusion is not relevant to out-of-body or near-death experiences.

The Iboga ritual does appear to induce experiences with many features and aftereffects typical of spontaneous near-death experiences. Thus the neuropharmacology of ibogaine and related psychoactive alkaloids may provide valuable insights into the mechanisms of near-death experiences. But illusions of bodies falling out of a chair should not be mistaken for out-of-body experiences, and proposed links to speculative hypotheses and unidentified pathways should not be mistaken for evidence of common neurological mechanisms. The potential benefits of studies such as this are wide-ranging, and deserve thorough exploration, particularly the authors' call for attention to spiritual experience in the clinical use of ibogaine therapies for drug addiction, and for further research combining physiology and spirituality.

Bruce Greyson
Division of Perceptual Studies
University of Virginia Health System
210 10th Street, NE, #100
Charlottesville, VA 22902
cbg4d@virginia.edu

"Kelp Highways" by Heather Pringle. 2008. *Discover* June:38–43.

In contrast to earlier scenarios of Ice Age hunters following big-game animals on foot across the then-dry Bering Strait into North America when sea levels were lower some 12,000 years ago, many archaeologists have recently come to favor an earlier initial human entry into the Western Hemisphere, via the Pacific littoral and employing boats. Science writer Pringle describes this developing theory and one of its researchers. Jon Erlandson of the University of Oregon speaks of a trail of rare but distinctive dart points with flaring barbs that dot the Pacific Rim from Japan to Chile. This may reflect an 18,000-15,000-years-ago coastwise movement, although most of the potential evidence now lies deep underwater owing to post-Pleistocene sea-level rise. Much of the route would have paralleled a familiar and biotically productive offshore ecosystem, the "forest" of kelp seaweed. Southern Chile's near-coast Monte Verde site dates to over 14,000 years ago and (as reported after Pringle's article appeared) has yielded 9 species of marine algae.

This vision of a maritime movement to America is but a part of the growing consciousness of very early human use of, and movement by means of, watercraft. Modern humans evolved in Africa some 200,000 years ago. Shellfish-gathering as old as 164,000 years has been identified in South Africa. Genetics indicates that people moved from Northeast Africa to the Arabian Peninsula as much as 70,000 years ago across the then-narrower Bab el Mandeb at the Red Sea's mouth, spreading coastally eastward and across the Strait of Hormuz, ultimately reaching Australia by traversing Indonesia's water gaps prior to 50,000 years ago. Humans settled islands well off Japan's shores by 32,000 years ago (and, as the article fails to note, reached some of the islands of Melanesia equally early). There are even indications that the pre-*sapiens* human–*Homo erectus*–arrived on the Indonesian island of Flores by water over 800,000 years ago. In the New World, greater genetic diversity of coastal Native Americans supports the notion of the first entrants having been shoreline-dwellers. Although "Twenty years ago, most archaeologists would simply have laughed at the idea of Ice Age mariners colonizing the globe" (p. 43), that is exactly the picture that is emerging.

"In Search of the World's Most Ancient Mariners" by Michael Balter. 2007. *Science* 318(5849):388–89.

Balter reports on a Cambridge conference on Global Origins and Development of Seafaring. There was disagreement among the conferees as to whether early crossings of Southeast Asian straits were accidental or purposeful. Because of the lack of comparable evidence elsewhere, many felt that archaeologist Michael Morwood's 800,000+-B.P. *Homo erectus* tools on water-surrounded Flores represent a fluke, involving not maritime technology but rather accidental drift on floating natural rafts of vegetation; modern humans (*Homo sapiens*) did not cross the water gaps of Wallacia (eastern Indonesia) from Sunda to Sahul until 50,000 or 60,000 years ago. (Sunda was the landmass exposed off Southeast Asia by Pleistocene lowered sea levels, and Sahul was that off Australia and New Guinea.) Accidental drifts on family-operated bamboo rafts might have been adequate to establish minimum viable populations of from five to ten persons. Around 30,000 years ago, people of Sahul developed the watercraft and the navigational ability to colonize the islands of Near Oceania (present island Melanesia). Island colonization in the Mediterranean was much later (ca. 13,000 years ago), perhaps because this much less productive water body did not encourage the development of sea-going.

"Radiocarbon and DNA Evidence for a Pre-Columbian Introduction of Polynesian Chickens to Chile" by Alice A. Storey, José Miguel Ramírez, Daniel Quiroz, David B. Burley, David J. Addison, Richard Walter, Atholl J. Anderson, Terry L. Hunt, J. Stephen Athens, Leon Huynen, and Elizabeth A. Mattisoo-Smith. 2007. *Proceedings of the National Academy of Sciences USA* 104(25):10,335–39.

Most scholars have assumed that the chicken, a Southeast Asian domesticate, was absent in the pre-Columbian Americas. Now, a team of researchers from several

countries has identified and analyzed bones from a minimum of five chickens in the near-coast site of El Arenal-1 on south-central Chile's Arauco Peninsula in historic Mapuche Indian territory. The site was occupied between A.D. 700 and 1390, and a chicken-bone sample yielded a carbon-14 date of 622 ± 35 B.P., i.e., a calibrated date of A.D. 1321–1407. Mitochondrial DNA obtained from the bones proved to be identical to that of older pre-Columbian chicken bones from Polynesia's American Samoa and Tonga, some 8,050 kilometers (5,000 mi.) away, and slightly different from DNA in bones from Hawaii, from Easter Island, from Yunnan in China and from Vietnam. Chile's contemporary blue-egg-laying Araucana chicken, taken note of over half a century ago by geographer Carl O. Sauer, appears to descend from the Polynesian breed. The mitochondrial DNA of later chickens from Easter and Hawaii resembled that of chickens from Indonesia's Lombok, from the Philippines, and from Thailand, suggesting two separate introductions of *Gallus gallus* into the Pacific islands–a suggestion first forwarded by George F. Carter in 1971 on linguistic grounds. Earliest archaeological dates for chickens in Oceania so far are from the Reef/Santa Cruz islands circa 3,000 years ago and from Vanuatu shortly afterward. Various cultural phenomena among the Mapuche also suggest Polynesian inputs, and hints of such bones have existed for some years. Chile is, of course, also the home of the *dalca*, the Western hemisphere's only sewn-plank canoe other than the Chumah *tomol* of Southern California; sewn-plank canoes are characteristic of Oceania.

"The Origin of Two Purportedly 'Pre-Columbian' Mexican Crystal Skulls" by Margaret Sax, Jane M. Walsh, Ian C. Freestone, Andrew H. Rankin, and Nigel D. Meeks. 2008. *Journal of Archaeological Science* (online May 2008; http://dx.doi.org/10.1016/j.jas.2008.05.007).

A carved-rock-crystal life-sized human skull of mysterious but allegedly ancient Mesoamerican origin and now housed in the British Museum has, over the years, had various mystical powers attributed to it (see, for example, Chris Morton and Ceri Louise Thomas, 2002 [1997], *The Mystery of the Crystal Skulls: Unlocking the Secrets of the Past, Present, and Future* [Rochester, VT: Bear & Company]). The genuineness of the supposed pre-Columbian provenance of this sculpture, as well as of a second one held by the Smithsonian Institution–for which no proof has been ever been forthcoming–has been much debated. Now, a team has subjected the skulls to scientific examination. The researchers found that the carving was accomplished with the use of rotary wheels, which did not exist in pre-Columbian America, and that carborundum, a modern synthetic abrasive, was used at least on the British skull. The material of the latter appears to have come from either Brazil or Madagascar, not Mexico; the quartz of the former could have originated in Mexico or the U.S.A. The British skull was made sometime before 1885, presumably in Europe, and the Smithsonian one probably in Mexico during the 1950s. The authors do not address the paranormal effects sometimes assigned to the skulls.

Stephen C. Jett
Professor Emeritus of Geography and of Textiles and Clothing
University of California, Davis
scjett@hotmail.com

DVD REVIEW

Out of the Blue: The Definitive Investigation of the UFO Phenomenon. 5-Year Anniversary Edition (2-disc set; includes the original version of this video).

Although I couldn't echo the subtitle (isn't every documentary billed as "the definitive. . ."?), I would have to say that this is one of the best UFO documentaries I have seen. This is not because of the choice of UFO imagery or the quality of the graphics, both of which are good, but because of the quality of the quality of the witnesses, the investigations of the various sightings and the narration.

The video begins with speculation as to whether there is life elsewhere. Edgar Mitchell, Frank Drake, Amir Aczel and Bernard Haisch introduce the idea that there are huge numbers of stars with odds approaching unity that there is life out there. This, of course, has, over the years become more or less conventional and uncontroversial. What is controversial is the claim that UFO sightings indicate that "they" have arrived here. The video approaches this problem by providing many witnesses and their reports. Several are presented in considerable detail with witnesses of high credibility, even including former President Jimmy Carter and former Governor Fife Symington. Later in the video there is a discussion of the problem of traveling astronomical distances. Bernard Haisch and Brian Greene discuss the possibility of using exotic methods such as warping space.

The review of sightings begins with the claim that 95% of sightings have been explained as misidentified aircraft, balloons and other natural or manmade phenomena as well as some hoaxes and perhaps a few delusions. However, the remaining 5% indicate something, probably extraterrestrial, is going on. (There is an argument in the UFO community as to the correct percentages: some would say that the truly unexplainable cases are as many as 15–20% of the total. Of course, the real question is simply this: is there at least ONE case that absolutely cannot be explained and indicates the presence of "other intelligences"?)

The first sighting which is discussed in detail is what has been called the "Phoenix Lights" of March, 1997. Having done research on this myself (see http://www.brumac.8k.com/phoenixlights1.html) I can especially appreciate the fact that this documentary emphasizes the sighting by many people (hundreds?) of a boomerang or triangle shaped object that passed over Phoenix at about 8:30 PM and essentially ignores the much more popularized video sightings of distant lights that were seen by several people at about 10 PM. The 8:30 PM object had some lights but, for the observers who were under or nearly under it, it was also a dark shape that blocked their view of the stars. The 10 PM lights, on the other hand, were low on the southwestern horizon, were only noticed by a few people with videocameras and were most likely bright flares dropped by the Maryland National Guard during training exercises about 80 miles southwest of Phoenix. In the "heat of the moment" during the first few days following the sightings the UFO investigators and the news media essentially combined these two independent incidents and publicized them as the "Phoenix Lights." The 10 PM videos have been shown repeatedly over the years in news stories and discussed in books as if they showed the 8:30 PM object. However, this documentary only briefly mentions them. Instead, it presents numerous witnesses of the 8:30 PM sighting, including former Governor Fife Symington. In July, 1997, Symington poked fun at the witnesses by having a press conference in which he presented an "alien" as

the "guilty party." In the spring of 2006 he was interviewed by this documentary crew and, for the first time, publicly admitted that he, too, had seen the huge triangular object. (Symington appeared at a press conference in November, 2007, along with an international group of UFO witnesses, mostly military pilots.) In this video he states that what he saw was incredible and remains unexplained.

After describing the Phoenix case the documentary presents a historical overview starting back in 1947. One of the historical sightings presented in detail is that of Graham Bethune. He and others in his aircraft saw a huge circular craft as he was flying south of Greenland. After landing he was interviewed and then told to never talk about it again. His story is one of hundreds of comparable military sightings after which the witnesses were told to "shut up" about it.

Then there is a discussion of theories about how "they" might have gotten here. Brian Greene, author of *The Elegant Universe* and other books, is featured speaking about distorting space.

Then back to the sightings. Gordon Cooper tells his story of a movie made of a landed UFO at Edward Air Force Base. Although he was not a witness to the actual landing, his film crew did see and film it and Cooper did examine a few frames of the newly developed film and could see the object. After debriefings the film was taken to Washington, DC, and "has not been seen since." The Apollo 14 astronaut, Edgar Mitchell, who was the 6th man on the moon, makes some comments on people he has met who have studied UFO reports but, being in intelligence agencies, have not been able to talk about what they know.

Robert Jacobs discusses his experiences after filming a UFO that seemed to circle a missile just after its launch, in 1964, from Vandenberg AFB. It seemed, from the film taken through a large telescope watching the missile, that the UFO emitted a beam that hit the missile and seemed to cause it to veer off its planned path. Jacobs was aware of this event because he was in charge of a photo-telescope team that filmed the missile. His story was backed up, in letters written years later, by his boss, Major Mansmann. Mansmann also wrote in a letter that the film was taken away, the portion showing the UFO was cut out, and the remainder was given back to him as the "complete" footage for that day's activity. Mansmann told Jacobs to never mention the event again and that, as far as Jacobs was concerned, it was an event that "never happened."

Russian sightings are presented. Cosmonaut Pavel Popovich tells of his own sighting from an airplane. General Alekseyev at Star City in Russia said that there were sightings which caused the people at the higher levels to realize that there was something that both the American and the Russians would have to "deal with." In the 1970's the Russian military began to show real interest in the UFO phenomenon. An eight month study by the Russian military concluded that there really was a "there, there."

Then the scene shifts to Southeastern England in December, 1980, the site of the famous Rendlesham Forest/Bentwaters sightings. The documentary presents an excellent series of interviews and analyses which make clear that this was no lighthouse beacon on the coast or any of the other explanations offered for these events. Beginning as a report of a downed aircraft in the forest, this developed into several of the most amazing military sightings ever, complete with multiple military witnesses, a landed UFO, landing traces, etc. Col. Charles Halt made a voice recording during one of the last sightings and later wrote an official memorandum about the sightings. Within a few years of the event ufologists had obtained both the tape recording and the memorandum. However, for the

first ten years or so after the event several of the primary witnesses were reluctant to go public. But not, more than 20 years later, they have stepped forward to tell the story. This documentary presents one of the best-available recitations of this case.

Next comes a 1998 video from Britain. A few minutes of the 12 minute video are shown. The video was taken to Industrial Light and Magic Company (Lucasfilms Special Effects Division) for analysis. The conclusion was that this is an actual video of an unknown object in the sky.

Michigan sightings in 1966 are briefly reviewed, including J Allen Hynek's "swamp gas" explanation. They led to the Air Force sponsored Colorado University UFO study headed by Dr. Edward Condon (1967–1968). Dr. Peter Sturrock of Stanford University analyzed the Condon study and concluded that it was not as scientific as one would expect. Although Condon wrote that nothing of value had been learned by studying UFO reports, Sturrock argues that this is not a fair evaluation of the sightings studied in the report, roughly 1/3 of which remained unexplained after the analysis. (If you can find a copy of the report, look in the index for "unexplained cases.") Yet, Condon's report is still, after nearly 40 years, being used to justify claims that there is "nothing" to UFO sightings and that no reported UFO is a "flying saucer" or alien craft.

Then follows a series of photos and videos, some of which are likely explainable. Some are internally lighted blimps (Beijing, 1995; Stuttgart, 1993) and some appear to be outright hoaxes (Mexico City, August 6, 1997).

The documentary shows Eisenhower warning against the military-industrial complex (MIC). While a worthy subject on its own (the dominance of the MIC), its inclusion here, I suppose, implies that the MIC is controlling UFO evidence. Then reference to Carter's sighting and his promise to release information suggests that even the President doesn't have a sufficient clearance or "need-to-know" to be told about top secret UFO information. Moreover, other powerful people have been prevented from getting information. Former New Mexico Congressman Steven Shiff ran into a wall when trying to investigate the Roswell Incident. Years earlier J. Edgar Hoover was denied access to a model disc. Bill Clinton asked Webb Hubble to investigate UFOs but he got no information.

There is a discussion of the French UFO "COMETA" study which investigated over 500 cases from throughout the world. The COMETA group did its work secretly and then published a very strongly worded document written by numerous French military and scientists. Their conclusion? UFO sightings are worthy of serious research because some could be evidence of non-human technology. They also implied that the US government is covering up information. (Now, where would they get an idea like that?) One case featured in the documentary is the Trans-En-Provence case of Renato Nicoli who saw a circular craft land in his garden. Plant samples showed that the plant chlorophyll had changed in the area beneath the object as compared to the plane away from the object. Another case from the COMETA report that is discussed in this video is the Iranian Jet chase of September, 1976, when two F-4 jets lost avionics while approaching a high speed UFO. In 2007 France put all their UFO reports on line.

Then the documentary switches to the Disclosure Project in 2001 which features numerous witnesses who were professionals in aviation and military intelligence who had personal knowledge of UFOs or the cover-up. There is also brief mention of the O'Hare Airport sighting of November, 2007.

Then follow a series of videos which may be identifiable. The Juarez, Mexico video of 1994 was probably a lighted blimp.

Closing comments reiterate that there appears to be a cover-up and the government, or someone associated with the government, knows a lot more than they have publicly revealed.

BRUCE MACCABEE
http://brumac.8k.com

Readers are encouraged to submit for possible inclusion here titles of articles in preferably peer reviewed journals (typically, which do not focus on topics about anomalies) that are relevant to issues addressed in JSE. A short commentary should accompany. The articles may be in any language, but the title should be translated into English and the commentary should be in English.