including the exchange of ideas. This made their time also a kind of "global village", even if we now tend to think that it is modern technology that makes such a notion valid. The many ethnicities had opportunities to translate each others' languages and exchange each others' ideas, of course leading to their respective developments, often in diverse directions. The breadth and wealth of McEvilley's scholarship, his ease of interrelating ideas expressed in a variety of idioms, and his lucid, intelligible style are impressive and instructive. This book should have a considerable impact on academic intellectual historians, which, in turn, will shape everyday attitudes as these scholars educate their students. The impact of the book should moderate our notions of a radically separate Eastern and Western tradition. Both the Greeks, with their Orphic and Dionysian practices, and the Indians, with their Tantric lore, have non- and trans-rational elements; but both have a logical, rational tradition as well. It is we who tend to see and prefer one over the other, depending on our own cultural and individual inclinations and selecting what is importantly present in which tradition. The truth is that thought, and in this instance, ancient thought, is cross-cultural, multi-ethnic and incredibly rich.

We will conclude by disagreeing slightly with Radhakrishnan, claiming that Eastern philosophers are not merely parallel to Western ones, but rather that they include a vast variety of details and nuances which are specific to them. McEvilley's thorough and detailed presentation of Indian and Buddhist traditions gives ample evidence to support this claim. Were Radhakrishnan really right, we should not be obligated to study the heritage from the East as our own, since these would already be incorporated in the Western corpus. But in reality, our exposure should be to the whole global intellectual heritage, since we are essentially human beings rather than merely Westerners or moderns.

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**The Rebirth of Cold Fusion: Real Science, Real Hope, Real Energy** by Steve B. Krivit and Nadine Winocur. Pacific Oaks Press, 2004. xxii + 298 pp. $25.95 (paper). ISBN 0-9760545-8-2.

A great deal of misunderstanding about the New Science of Condensed Matter Nuclear Reactions has been caused by the early—and partly erroneous—name: Cold Fusion. It is true that the phenomenon upon which so much attention has been placed—heat produced in a system in which deuterium is transmutted under electrochemical conditions in helium–works by the fusing of two D atoms, but it has transpired that this is one of a series of nuclear reactions that can be stimulated to occur in solids and some of these involve fission rather than fusion. Physicists who were first appraised for the discovery of a method of producing nuclear heat at an electrode-solution interface at room temperature had expectations that they derived from a theory based on the happenings in plasmas at solar temperatures. For example, when told of the heat being claimed by the electrochemists, they calculated that such a heat would produce a neutron stream fatal to its recipient. Only gradually—and grudgingly—did it have to be admitted that nuclear phenomena were occurring in this very simple electrochemical system and that the equations and mechanisms arising from the theory of fusion in plasmas did not apply.

Not only has there been a doleful exhibition of slowness of comprehension on the side of the classical nuclear physicists (facing revolutionary change introduced by two chemists), but there has been a great deal of hubris mixed up in the gory mess, and a stubborn resistance to recognize new experimental facts being replicated the world over and published to the extent of several thousand papers. It seems that what has happened, essentially, is a stunning example of copying the ostrich, which, when threatened, is said to make a hole in the ground and put its head therein, thus shutting out ugly reality.

Krivit is a journalist and Winocur is a psychologist. They are married and it is clear that their bond has helped lead to a superb example of journalistic investigation in bringing the facts of a new branch of Science to any member of the reading public. This branch is little explored because the reigning nuclear physicists advised government fund givers that the new facts were due to misunderstanding among unwelcome newcomers to nuclear science.

The book divides the material into four parts. The first is called Global Energy and brings out the parlous position the world is in regarding its oil-based energy supply. The closeness to the maximum of the world oil supply rate (which has been increasing exponentially) has brought no productive reaction from the present administration, which seems to act as though protecting the dwindling oil supply is the important thing. Nuclear energy from fission reactors is referred to briefly—a more than doubtful alternative in view of terrorist threats on the reactors. The continued promises of the hot fusion workers are diminished in importance because of repeated promises that success is only 20 years away. The authors call hydrogen "the limited promise" because they imagine hydrogen—meant to cure pollution because the product of its combustion is only water—is itself made (at present) from natural gas with co-production of $CO_2$.

Part Two centers on the dark forest of the US physicists and all they did to try to show that what had been discovered was bogus. If we manage to overcome world devastation by rising temperature, melting ice, and world wide coastal inundation, there will surely never again be such a dreadful picture of scientists suppressing new facts and persecuting those who gave rise to them, thereby delaying the development of SOLID STATE NUCLEAR SCIENCE by at least

15 years. The evidence of biases attempts to win by ridicule is laid bare. A feeling of horror comes over me when I look back on those awful days of 1989 to 1990. There were the ridiculous attempts of the head of DOE, handing out a command to the government labs to replicate the Fleischmann and Pons electrochemical experiments. This meant that hundreds of physicists, totally ignorant of Electrochemistry, attempted to build electrochemical cells and operate them. They stuck neutron counters near the apparatus to detect the expected stream of neutrons, which their plasma theory of fusion predicted. When none were observed, the conclusion was: No Fusion. In the famous McNeill-Lehrer announcement Fleischmann (one hopes accidentally) had left out a vital fact: You have to wait. Indeed, with the wire electrode then used, you might not see anything nuclear for 100 hours or more.

Krivit and Winocur select the Physical Society's Meetings on May 1, 1989, as the lowest point of the behavior of the physicists. They cite William Harper of the Princeton Plasma Laboratory as a leader of the demeaning physicists. He is quoted as saying, "Just by looking at these guys on TV, it is obvious that they were incompetent boobs.'" I am quoted as reporting my observation of Nathan Lewis (Cal Tech) changing color in fury at the suggestion that the heat produced under certain conditions was nuclear in origin. Cal Tech's Stevan Koonan, Krivit and Winocur say, likened the claims of those reporting nuclear heat to pigs flying. "We are suffering from incompetence and perhaps delusion from Fleischmann and Pons," Koonan is reported as saying.

A whole chapter of this book is given to the Department of Energy's Committee sent out to investigate Cold Fusion as it stood in November 1989—a mere 8 months after the Discovery. I was one of those "investigated" and I must say that the attitude of those doing the interviewing was similar to that of police questioning a suspected miscreant. Bias was clear and extreme. It seemed that the investigators—and they include electrochemists—saw that a Big Discovery was possible and did everything to prevent its progress.

There are other events of dishonor which Krivit and Winocur are brave enough to reveal. Thus, one Lawrence Livermore physicist reviewed a Cold Fusion paper, rejected it (i.e. blocked its publication), and then sought government funds to start work on Cold Fusion himself!

Of course, once one gets away from the entrenched nuclear physicists, a sense of (scientific) objectivity and normal behavior is re-established. Already in 1991, a group of scientists from the Naval Weapons Center at China Lake pointed out that an examination of the anti-cold-fusion experiments from Cal Tech and MIT "contained serious errors." But we again come up against unethical behavior when he who was at the time the American associate editor of Nature submitted a paper sent to Nature, which showed the errors in the Cal Tech work, to ardent Cold Fusion attacker Nathan Lewis as referee! The refusal of Nature and *Science* to publish positive Cold Fusion work is well known. In fact, Maddox, the long-time editor of Nature, told me he wanted to give Cold Fusion "a good thumping" in his editorials.

Perhaps the principal piece of misinformation used to discredit the existence of the new field arose from prestigious MIT. Krivit and Winocur quote Ronald Parker, a key figure in MIT's anti-Cold Fusion activity: "Everything I've been able to track down has been bogus, and I think we owe it to the community of scientists to begin to smoke these guys out." It was Gene Mallove, formerly chief science writer at MIT, who did the smoking out. As told in the book, he got the raw data from the MIT experiments and compared them with what had been published. The raw data showed the excess heat, but these published data had been changed to show "no heat."

One of the best features of this book is the strong use of quotations from the dramatis personae. William Harper is one such example. (Krivit and Winocur quote him as referring to Fleischmann and Pons as "incompetent boobs.") Under questioning from Krivit, it turns out that Harper cannot name any Cold Fusion papers that he has read. Part Two ends with a brief statement about 15 years of progress in the new field, 3000 and more papers; Cold Fusion has been tested with confirmation of heat in most of the technologically active countries of the world; and the nuclear character of this heat has been multiply proven by measurements of tritium, and helium accompanying the heat.

The first and second parts of this book (energy situation and refusal of US physicists to investigate the new results) take up about half the space. Part Three, Discoveries and Mysteries, brings out the progress made and problems yet to be solved. An interesting table on page 263 gives 13 US universities involved, 21 in other countries. Government support is in 16 laboratories abroad but only three in the US. In terms of the number of researchers, Japan and Italy tie at 13 each although Krivit and Winocur claim 46 in the US (31 being "private"). I found this figure much bigger than my own estimate, but the "military" division at 11 may well have grown and the "private" one is difficult to estimate.

Part Four is devoted to the knotty subject of reproducibility. In the early years one was happy if one experiment in five "worked." Things have got a lot better and four laboratories are now claiming to have reproducible results. It is clear we are in the middle of a battle to find out just what does happen at the surface of metals to initiate spots of nuclear reactivity. Also, some clearer definition of "reproducibility" is needed. For example, it has been established that if the D/Pd concentration ratio exceeds 0.9, excess heat is reliably produced. Reproducibility? Not really, because many experiments do not reach 0.9. What can be said is that the phenomena are repeatable, that is, the same results are observed world wide although, as yet, the ability to produce those results on a given day may still be in doubt.

Finding tritium formation is the easiest sign that a nuclear reaction is occurring, and when that was all in doubt (until 1992) it was the most appealing experiment. Detection of tritium is easy because it is radioactive. A much more difficult experiment is helium detection and measurement; whereas tritium was first reported in a refereed paper in 1989, it took until 1992 for a reliable experiment to indicate helium, which, however, turned out to be the main product of D + D reaction in the palladium surface. (Joyfully received, for helium is a benign element and hence a wonderful waste material should the process be developed on a large scale.)

The sad story of journalist Gary Taubes in his book on Cold Fusion *(Bad* Science) is told in detail. Taubes set out to bring evidence that he thought would show that the first measurement of tritium, made in my laboratory at Texas A&M, was fraudulent and had been contrived by the graduate student who made the measurements (adding tritiated water to his cell). The outcome of Taubes' investigation led to an article accepted by Science without my being asked to comment on the contents. The article clearly infers that I (the Supervisor) was an ignorant fool or took part in a fraud. Even sadder is that a member of the Cyclotron Institute at Texas A&M took part in the accusation. Seeking redress, as was multiply pointed out to me, would be pointless—the only point was, would tritium also be found by other laboratories (I stopped counting replications at 47). The last straw in this part of the story was a letter sent to Science by Dr. Ed Storms of the famous government laboratory at Los Alamos describing a diagnostic experiment that distinguishes between tritium put in the cell from tritiated water and tritium produced in the electrochemical cell. Although Storms' experiments clearly proved that the later was occurring (invalidating Taubes' article), Science refused publication of Storms' letter. The account of all this in the book (19 pages) has many quotations from several of those individuals involved.

A quantitative measurement of helium by Melvin Miles at China Lake is described in some detail. Helium—atomic weight 4—may be confused with deuterium—molecular weight 4—and the distinction needs very sensitive mass spectroscopy. The importance of the quantitative measurement in the gas phase (compared with Texas A&M's earlier 1992 paper establishing helium in electrodes that had been evolving deuterium) is that the amount of helium produced matched the requirement of a D + D → He reaction and thus established that, in this case, it was indeed Cold Fusion.

Theoretical developments of these nuclear reactions in solids has not yet led to a consensus, and the Krivit-Winocur Chapter *25* on this is correspondingly weak—reading it does not really enlighten one about how two deuterium ions can get close enough to fuse (Coulomb-Barrier).

Weaker still is the section on the transmutation of metals. This is because the material in the book concentrates on the Japanese work and Mistubishi's remarkable results (e.g. cesium to praseodymium). But I was disappointed that the work originally done at Texas A&M (H in Palladium → numerous nuclei, by Miley at the University of Illinois and by Mizuno at Ilokkaido) was left out of the review. Such work—which had to overcome yet another prejudicial shouting of "Alchemy!"—could be the basis of a new and powerful method in metallurgy apart from the work already reported by Gleason and by Fox on the results of the treatment of nuclear wastes.

In Part Four, the authors sum up. The first point must necessarily be the threat to academic freedom. The disgusting affair of the attacks made upon me and by my peers in the Distinguished Professor group at Texas A&M is related in detail and includes a letter I had not seen before describing pressure brought upon those who refused to sign a petition asking for my demotion for having carried out research on this "cold fusion caper."

Chapter 30 is a powerful chapter and reflects on the threats to National Security that exist when the US government refuses to grant support to the New Science, whereas in Japan, Italy, France, China, and Russia this support is being given.

What has been learned? First, Science is a changeable body of knowledge. The rate of change in most areas is such that what is right for one generation is often nonsense for the next. Second, Progress comes with the Investigation of Anomalics. I suppose a third and doleful conclusion is that our greatest institutions (MIT, Cal Tech) are not capable of adjudicating anomalous results. They flee from the challenge and support the old story.

The last chapter (speculations about the future) dwells on Power Density and mind boggling numbers are given (e.g. $10^4$ watts $cm^{-3}$ from Fleischmann and Pons and $10^5$ from Preparata). But these numbers are not worth as much as what the authors infer. Heat in Cold Fusion is given in bursts and, of course, is steady power delivery that is sought. Again, the watts per cc may not be the thing to watch. It is clearly a surface phenomenon and watts per $cm^{-2}$ is more relevant. Most would admit reproducibility is still wobbly.

In summarizing what I think about this book, one has to have a comparison standard. The only competitor is Beaudette's ***Excess Heat and Why Cold Fusion Research Prevailed.*** Both books are very good at shining a strong light on what must surely be the worst period ever in American Physics. But I think Krivit and Winocur take the lead in lucidity. There is very little in their book that could be said to be difficult to understand by any member of the reading public. Were I to seek faults, I see only two. (a) The concentration is predominantly on the US and its work. Japanese work is touched upon (Mizuno and Inamura) but the Japanese have been outstanding in their government-supported contributions. The copious Russian work is hardly mentioned. (b) Again, although most would agree with the concentration on heat from D-Pd, I personally would like to have seen more on transmutation and an on-the-edge-area that is so counter intuitive that many in the field do not talk of it. I refer to the evidence for nuclear reactions in Biology.

Anyway, there is no doubt that this is the book for all who value freedom of thought and who appreciate an exciting and lucid account of the greatest development in experimental Science in the later half of the 20th Century.

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[1] Fleischman — among other achievements — is a fellow of the Royal Society and a Director of the Max Planck Institute in Berlin.

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This is an excellent survey of the current status, history, and implications of "cold fusion". Given the range and substance of the book, it is surprisingly easy to read, even a page-turner as some of the stories unfold. For the general reader, the necessarily technical bits of science — which are only a few — are explained lucidly and accurately. For scientists who have not followed this topic, the book tells what they should know, namely, that these extraordinary new explorations have been given too short shrift by the mainstream.

The general problem for "cold fusion" will be thoroughly familiar to anyone who has encountered a phenomenon that seems inexplicable and occurs capriciously. The mainstream discourages rather than encourages risk-takers to explore whether the capriciousness can be overcome, and so it may take much longer than would otherwise be necessary to ascertain whether or not the phenomenon is real. In the case of cold fusion, this book notes how casual yet effective the official discouragement may be: Acknowledgements that some crucial experiments seem sound are not accompanied by any recommendation that further study is desirable (pp. 109, 136–137). The whole scientific community absorbs, through a sort of osmosis, that "cold fusion" is iconic of the unreal (p. 146). Reviewers of manuscripts give silly, invalid, improper reasons why papers should be rejected (for instance, that reported data are not accompanied by a final explanation, p. 124; that a paper on electrochemistry is not "a good match" with the Journal of the Electrochemical Society, p. 128).

The scientific mainstream remains perpetually ignorant of crucial bits of its own history: that the most significant advances are typically resisted or ignored (Barber, 1961; Hook, 2002; Stent, 1972) and that the ability to reproduce an experiment may depend on factors yet to be discovered — it was three decades before the relevant observations of semiconductors could yield workable transistors because the effects appeared only capriciously, owing to the need for an unprecedented degree of purification. Funding agencies — with very few but noble exceptions like the Defense Advanced Research Projects Agency (DARPA)— have not come to grips with the fact that it is precisely the far-out ideas that need to be explored if genuine advances are to stimulated; devoting 5 or 10% of research funding to unorthodox approaches would pay off at least as handsomely as the touted "spin-offs" used to justify otherwise unjustifiable ventures.

For these commonplace reasons, it is taking a couple of decades or more for the several relevant disciplines to become aware that the "cold fusion" announced by