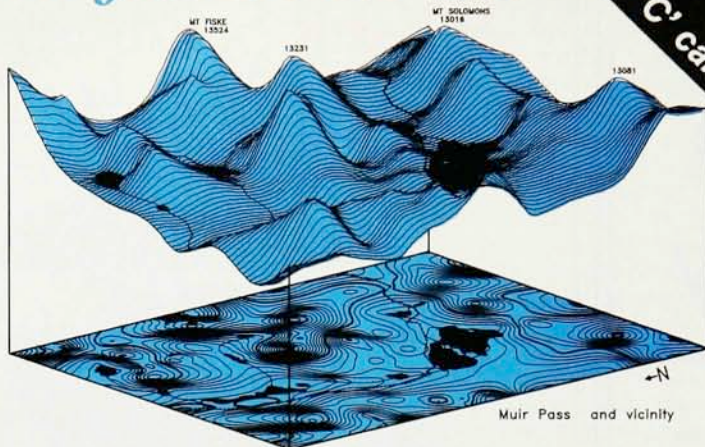


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based on empirical data, not theory.

I certainly have no aversion to studies of risk, although I am less of a supporter of "acceptable risk" studies. However, as I wrote in my article, "scientists and engineers have the most to learn about the process of making sound technology policy." I believe we do not understand that in most disputes involving technology, the public is arguing values, not facts—and technologists seldom address values. Developing the best available information and performing thorough analyses, including identifying uncertainties in knowledge, remains necessary for understanding the technical aspects of the issues. However, such analyses often (usually? always?) overlook the value issues that are at the heart of most public concerns.

I agree that there is a need to get disinterested advice. Organizations such as the National Research Council and the Office of Technology Assessment serve that purpose, as would the "friends of the court" system I advocated in my article.

Finally, 40 millirems per year is not "above"—or at—the evacuation level for nuclear power plant accidents. The Nuclear Regulatory Commission regulation requires that emergency plans be developed for areas surrounding nuclear power plants in case of an accident that might release radiation. As part of these plans, utilities are required to have a process for providing recommendations for action to the local and state governments. The regulations do not specify any levels for action. However, in discussion with the planning groups, the Federal representatives will suggest using as trigger levels the action levels provided in draft EPA regulations. These levels are based not on dose rate, but on projected doses received by the general public. Following these action levels usually would lead to recommending sheltering when the estimated dose exceeds 1 rem to the whole body, or 5 rems to the thyroid gland, and recommending evacuation when the projected dose exceeds 5 rems to the whole body, or 25 rems to the thyroid. The actual recommendations would be based on the state of the reactor, weather conditions and other local factors.

JOHN F. AHEARNE

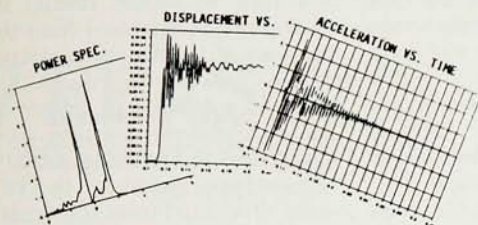
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6/89

Thoughts Unbottled by Cold Fusion

I very much enjoyed reading Barbara Goss Levi's characteristically fine re-

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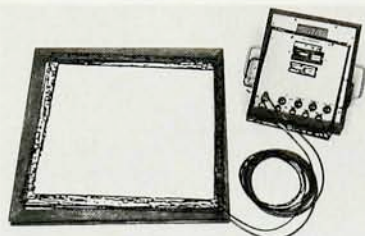
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view (June, page 17) of the experimental and theoretical situation with respect to the recent flurry of activity in the realm of cold fusion.

A possible explanation for the cold fusion phenomenon that seems to me to be missing from merely two-body explanations, such as those given by Clinton Van Siclen and Steven Jones or Steven Koonin and Michael Nauenberg and referenced by Levi, is that of enhanced transmission of deuterons through the deuteron Coulomb barriers because of resonances on the atomic scale. For example, instead of an exponentially small transmission coefficient (such as yields the fusion rate of 10^{-70} /molecule-sec to which Levi refers), conventional quantum mechanics yields a transmission coefficient of unity whenever the resonance condition of $\int k(x)dx = (n + 1/2)\pi$ is satisfied by the wavenumber of the particles crossing the potential well between two barriers.

Herein lies the possibility that if deuterons are situated at interstitial sites in a Pd lattice, a diffusing deuteron may have a de Broglie wavelength that permits resonance in the wells formed by the ascending walls of neighboring Coulomb barriers. Suppose we estimate that for a low-energy resonance, the de Broglie wavelength is $h/p \approx L$, where L represents the characteristic width of these wells. Then $3k_B T/2 = p^2/2m_D \approx h^2/2m_D L^2$, where k_B is Boltzmann's constant, m_D is the deuteron mass and T represents the temperature of a thermal distribution of deuterons whose root-mean-square momentum is p . Thus $T \approx h^2/3m_D L^2 k_B = 318/L^2$, with T given in kelvin and L in angstroms. For an L of 1.04 Å, which is larger than the deuteron separation of 0.74 Å in a deuterium molecule, one finds that $T = 20^\circ\text{C}$, characteristic of an ambient room temperature. Hence, because of the many-body nature of the interactions of a room-temperature deuteron diffusing through a lattice having deuterons in many of the periodic interstitial positions, the diffusing deuteron can resonate on the atomic scale and thereby penetrate the Coulomb barriers in a fashion vastly underestimated by two-body calculations, in which wells for possible resonance are absent. Although cavalierly derived here, this formula indicates the increasing importance of large-scale correlations (many-body coherent effects) with decreasing temperature.

Thus I am focusing attention here on the fact that a negligible transmission coefficient of an isolated Coulomb barrier can be boosted many orders of magnitude to attain a value

of unity as a result of resonance effects on an atomic scale in the wells bounded by the adjacent Coulomb barriers of a periodic lattice. (Indeed, the lack of robust results in cold fusion might originate from the narrowness of transmission resonances.) If the experimental results that purport to exhibit cold fusion become more definitive, this idea should be quantified to treat the actual three-dimensional nature of the Pd system. One must treat the dynamics of the Pd and D nuclei and the electrons. Coulomb-screening effects due to pair correlations will naturally arise out of such a treatment. Additionally, one must include the omnipresent factor of deuteron transport resulting from either an applied external voltage or an applied thermal gradient. To calculate the effect of this enhanced transmission coefficient on the rate of cold fusion, one needs to know not only the structure of the transmission resonances as well as the distribution of states occupied by the deuterons in the neighborhood of these resonances, but also the D-D fusion cross sections at thermal energies. Of course, this model would not account for the disparity between the amount of heat released and the number of neutrons produced that has been alleged by researchers at the University of Utah, Texas A&M University and Stanford University.

LEAF TURNER

Los Alamos National Laboratory
Los Alamos, New Mexico

6/89

I believe we can learn a number of lessons from the recent flap over low-temperature fusion, even though the dust has not yet settled.

First, newspapers and press conferences are no way to disseminate scientific results. They are a way of seeking publicity, plain and simple, and need to be discouraged. In fact, our whole society overemphasizes fast thinking, at the expense of sound judgment. It seems to me that we have entered a stage in human development where a premium must be placed on obtaining correct results rather than rapid guesses. No one seems to have been at all concerned about what the world would be like in the event that cheap fusion extrapolated to hydrogen bombs that could be built in any chemistry department.

Second, it would seem that small-scale exploratory experiments are still a viable way of doing science. Indeed, putting hundreds of millions of dollars into a single fusion experiment makes sense only after we have come to understand the fundamental transport characteristics of plasmas.

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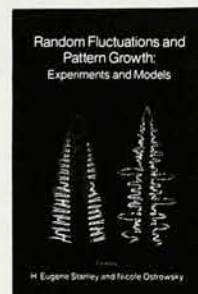
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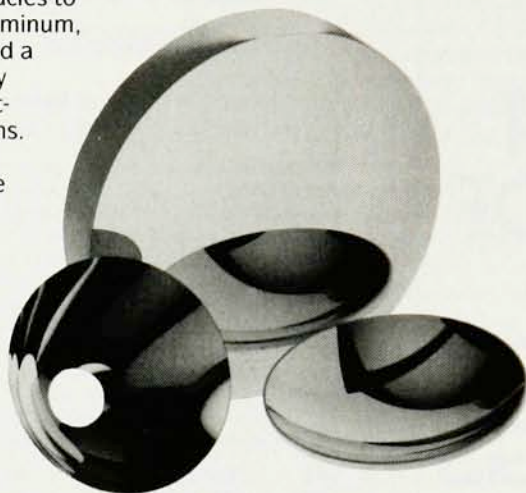
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ROBERT JONES
Emporia State University
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4/89

By the time this letter appears, I trust that it will have dawned on the physics world that the energy found by Stanley Pons and Martin Fleischmann is the energy of the crystal structure of palladium, or other reaction metal, given up when the process of hydrogen embrittlement destroys the ordered structure of the metal.

Using the same logic I have seen verbalized, I can very easily create a system having an energy gain of millions. All I have to do is supply a few joules to an explosive squib attached to a ton of TNT. The thermal energy I get out is a lot more than the electrical energy I put in.

YALE JAY LUBKIN
Owings, Maryland

5/89

There is a certain similarity between the attempt to develop flight capability and the effort to attain energy from nuclear fusion.

In the case of flight, humans began by trying to imitate nature, in particular the flight of birds. After hundreds of years of such efforts, success was attained by embarking upon a different road.

In the case of the fusion process, we are again trying to imitate nature, in particular the processes in the interior of the Sun. And despite predictions made more than 20 years ago, we still have not succeeded.

Even if the recent reports of success in attaining energy from fusion turn out to remain unsubstantiated, they can be helpful in getting us to look for solutions other than the one of following nature. Just as the departure from this last tactic in the case of flight led to success, so departure in the case of fusion may enable us to attain more energy from fusion than we put into it.

SAUL BIRNBAUM
Bronx, New York

4/89

No Jingoism at High- T_c Meeting: Graham

Although Irwin Goodwin is an unusually careful and thorough science journalist, his news story in the July issue (page 47) perpetuates a myth concerning the July 1987 Federal Conference on Commercial Applications of High Temperature Supercon-