BRIGHAM YOUNG
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THE GLORY OF GOD
IS INTELLIGENCE

August 2, 1991

Prof. John O'M. Bockris
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College Station, TX 77843

Dear Prof. Bockris,

Thank you for your letter. I will respond to four major issues that you raise, that now confront the community: 1) The possibility of nuclear-energy transfer to a metal lattice; 2) The work by Miles et al. on heat and helium; 3) Some views on tritium production; and 4) Detection of energetic nuclear particles vis-à-vis "excess heat" claims.

1) Possible nuclear-energy transfer to a metal lattice

Prof. Preparata and I have exchanged some correspondence on this issue. Rather than repeat myself too much, I enclose a recent letter to Giuliano which your letter suggests you have not seen. In support of my arguments and those of Guy Larsen (a graduate student) given there, I also send the first few pages of Samuel Wong's text "Introductory Nuclear Physics." He discusses the general problem of (virtual) energy transfer in times limited by the Heisenberg uncertainty principle and the speed of light. Certainly the same considerations apply here since it is argued that the nuclear energy is transferred <u>quickly</u> to the lattice <u>without</u> formation of (observable) gammas or energetic emitted particles. Please notice that for <u>virtual</u> energy (E) transfer as applies here, Wong uses

$$t \leq \hbar/E;$$

notice the direction of the inequality. Then, travelling at the speed of light, the energy can be transmitted only a distance:

$$r \approx ct \leq \hbar c/E.$$

Now, E for nuclear reactions is of the order of MeV, while hc=197 MeV-fm. Dividing, we find that nuclear energy, to remain virtual and thus transfer undetected to the lattice, can only travel a distance of about 0.002 Angstroms:

$$r \approx 197 \text{ MeV-fm}/ 1 \text{ Mev} \approx 200 \text{ fm} = 0.002 \text{ Å}.$$

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Samuel Wong, Introductory Nuclear Physics, Prentice Hall (N.J.), 1990, pp. 1-3.

Chapter 1

INTRODUCTION

Nuclear physics is the study of the structure of nuclei and the interaction between nucleons. The basic building blocks of all nuclei are protons and neutrons, two different aspects of the same particle, the nucleon. The fact that a large variety of nuclei are constructed out of nucleons makes the subject an interesting one. The diversity of observed phenomena is the result of both the fundamental interactions operating between subatomic particles and the basic symmetries governing their behavior. It is therefore appropriate to start a study of nuclear physics with a review of the fundamental interactions in nature. This will also serve to relate the study of the atomic nucleus to the overall pursuit of physics.

§1-1 FUNDAMENTAL INTERACTIONS

The dominant force acting between nucleons is an aspect of the strong interaction. In addition, both electromagnetic and weak interactions also play an important role in determining the properties of nuclei. These three interactions: strong, electromagnetic, and weak together with gravitation interaction, form the four fundamental interactions in nature.

The modern view of force between particles is based on field theoretical ideas. A particle feels the presence of another one through the exchange of one or more field quanta, little "bundles" of energy. For example, two charged particles feel the presence of each other by exchanging photons between them. The field quanta are necessarily *bosons*, particles governed by Bose-Einstein statistics in quantum mechanics, so that they may be absorbed and emitted by the interacting particles without being constrained by the Pauli exclusion principle. The energy E associated with a field quantum is related to the range of the interaction it carries. This can be seen from the Heisenberg uncertainty principle. When a quantum of energy E is emitted, the state of the particle that emits the quantum is changed by the process. If the field quantum exists only for a time $t \leq \hbar/E$, where $\hbar$ is the Planck's constant divided by 2π, we need not be concerned with energy conservation. Furthermore, since the particle exists only for a short time, it cannot be observed directly. For this reason the field quantum is called a *virtual* particle. In contrast, a real particle has a definite energy and the amount can be measured in the laboratory.

For the purpose of estimating the range of a force, we may consider the field quanta to be travelling essentially at the speed of light c. The distance a quantum can travel, and hence its range, is therefore $r_0 \approx ct$, where t is the amount of time the field quantum existed. From the uncertainty principle $Et \leq \hbar$, we have the relation

$$r_0 \approx ct \approx \frac{\hbar c}{E}$$

If the field quantum has a non-zero rest mass m, it must have an amount of energy no less than its rest mass energy mc^2 and the range of the interaction it carries is limited to a distance

$$r_0 = \frac{\hbar}{mc} \tag{1-1}$$

Since $\hbar c \simeq 197$ MeV-fm (1 MeV $= 10^6$ eV and 1 fm $= 10^{-15}$ m), a range of 2 fm is obtained for a particle with rest mass energy mc^2 of the order of 100 MeV.

This distance is very small compared to lattice-spacings so that nuclear energy cannot be dumped unobserved on the lattice, to form "heat" without energetic particle emissions. The numbers simply do not work out by three orders of magnitude or so. Once again we see the crucial need to be quantitative in this business.

This argument is similar to that given in my letter to Giuliano, just a different approach (and a factor of 2π in $\hbar$).

Your letter mentions a putative parallel between lattice-heating ideas and the Mossbauer effect. I have formed a table to show that the two notions are in fact not analogous:

Mossbauer Effect	Lattice-heating notion
Of order: 1-10 KeV energy	~1-10 MeV energy
Excited nucleus lifetime 10^{-7}s	Excited He nucleus 10^{-22}s
Negligible energy transfer to lattice (essentially momentum transfer only)	Enormous ENERGY transfer to lattice/collective "super-radiant" state of electrons implied (MeV's of energy! How can momentum be conserved?
Approx. 1% of gammas experience Mossbauer effect	Non-observance of sufficient energetic particles by orders of magnitude requires approx. 100% of nuclear reactions to transmit energy to lattice.

The point that energy transfer to the lattice is essentially zero during a Mossbauer transition is a most significant difference with respect "super-radiance" and similar models. Momentum must be conserved. Momentum conservation along with total energy conservation constrains that most of the energy must go to the lighter particle. In the case of an interaction with the lattice, the lattice is orders of magnitude more massive than the emitted particle -- this would also hold in the case of putative helium or tritium nucleus formation by a nuclear process with "lattice heating". Thus, the (MUCH) lighter nuclear product, say an alpha particle, must carry off nearly all the energy. This is the case in the Mossbauer effect where a gamma recoils against a massive lattice. But then the "heat" does NOT go to the lattice, but rather to the synthesized nucleus. (One could argue that energy goes to electrons or even a collective state of electrons (how massive?), but again these would share MeV's of energy, implying Bremsstrahlung, etc.) You see, momentum conservation implies that the nuclear product must be fast-moving: the "nuclear debris" must carry most of the energy in any interaction with the lattice. And such an energetic nuclear product will

cause secondary interactions, X-rays, etc. --which are absent in the amounts needed (by many orders of magnitude) to allow heat to be commensurate with nuclear debris.

These arguments dealing with momentum versus energy transfer in the two dissimilar cases (Mossbauer and "lattice-heating") are quite independent of the arguments above regarding constraints imposed by Heisenberg uncertainty and the speed of light.

Notions of slow nuclear debris along with lattice heating, or supposed parallels between the Mossbauer effect and lattice-heating notions, as you have made along with others, are seen to be out of line with fundamental principles. Let's move on.

2) On Heat and Helium: Miles et al. Revisited

Mel Miles came to BYU on July 23, 1991, and gave a most interesting colloquium on his heat and helium measurements. The room was occupied by physicists, chemists, and calorimetrists from BYU and the University of Utah. He was kind enough to answer probing questions; his presentation lasted nearly three hours.

We found out a number of worrisome details about the China Lake experiments.

I asked Dr. Miles if any helium measurements had been thrown out. He stated that they had thrown out two such measurements. In one case, heat was seen in a D_2O cell but no helium was detected. (This indicates a potential non-correlation between heat and helium.) The data point was thrown out, he said, because the electrolyte was found to be low so that the heat could have been caused by this. In a related case, heat and helium were both seen, but the measurements were again thrown out since the electrolyte was found to be low. There seems to be an inconsistency here: if the excess heat in the two instances was indeed due to low electrolyte, then the finding of helium in the second case implies helium was present when it should not have been. He cannot have it both ways. Taken together, these rejected measurements cast doubt on claims of 1-to-1 correlations of heat and helium.

Dr. Miles also said that two control flasks out of eleven containing boil-off from liquid nitrogen showed measurable helium-4, one a small amount and one much larger (about 8×10^{11} He-4 atoms in about 100 ml of N_2). He ascribed THIS helium to contamination due to air leaks which occurred during air shipment. In general, observations of helium in controls casts doubt on a nuclear origin for helium seen in D_2O cell gasses. He said that the levels of helium seen in samples extracted from D_2O cells were less than normal levels in air, so contamination is of concern quantitatively.

Based on my own experience of hydrogen-isotope permeation in muon-catalyzed fusion experiments, I predict that experiments will show that Miles is incorrect in claiming that the presence of deuterium inside a glass vessel prevents helium permeation into the vessel. We shall see.

Eight flasks were used for shipping to Texas for helium measurements. Unfortunately, the flasks were not identified so he could not answer whether there was a correlation between helium seen and particular flasks. Most helium levels were taken as 10^{14} (high), or 10^{13} (medium) or 10^{12} (low) helium atoms per 500 ml gas; one would like to see measurements better quantified than this.

Several scientists in the audience reacted that Dr. Miles et al. should take more measurements on light water cells: only 2-3 weeks total were spent on controls compared with several MONTHS on D_2O cells. We strongly encouraged him to run H_2O and D_2O cells simultaneously, taking heat and helium measurements of both equally, and making many helium tests when heat was NOT found as well as when heat was found.

The China Lake team used dental film to look for X-rays. Unfortunately, dental film is sensitive to mechanical pressures -- a student demonstrated this here (we also thought we had something for a short time) before we abandoned this low-tech approach. I understand that various chemicals can also give a false reading, although Miles no doubt took pains to avoid this. Hopefully, Miles et al. will get a state-of-the-art detector, less sensitive to systematic errors. The community agreed on the need for such detectors at the Provo meeting in 1990, so as to have reliable data and make solid progress.

Let us turn from helium-4 to heat studies at China Lake. Dr. Miles showed impressive data on excess power in a D_2O cells -- up to about 25%. I asked to see a similar plot for H_2O cells; he did not have an overhead transparency for the H_2O controls, but showed me a plot afterwards (see Figure 6 attached).

Please notice that fluctuations in this plot are up to 20% ! Oh, my: his calorimeter is not stable. Fluctuations in the light water cells are about as large as his "signal" in the heavy water cells. These data are published in J. Electro. Chem. 296: 241 (1990). He said that recent H_2O data showed smaller fluctuations, but he did not have any of these plots to show.

Compare the above with D_2O cell-data given in the same article (Figures 5 and 7 attached). True, the data points here are essentially all above the no-excess-power line. But perhaps there is an offset in the calibration. Dr. Lee Hansen found out via questioning that calibrations were done with a resistor in place of the palladium rod; but then bubbling is absent during calibrations (this difference may not be offset by stirring). Also, calibrations were done before and after long runs, NOT

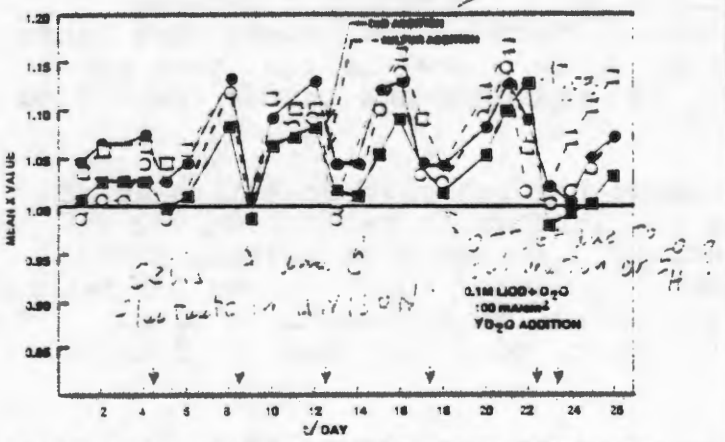

Fig. 5. Second cold fusion study using palladium rod (Johnson Matthey, $d = 0.635$ cm). Cell A (– – –), cell B (———). T_1 (○), T_2 (□), T_3 (●), and T_4 (■).

instability was noted that was likely due to a mixing of the air in the glass thermistor tube. A portion of the thermistor tube extended above the calorimetric cell and was subjected to cooling by the room air. In later experiments, the thermistor tubes were made flush with the cell top resulting in more uniform temperatures and X values within the same cell. All H_2O measurements were made within an average daily room temperature range of 19.9 to 22.8°C with a mean of 21.5 ± 0.7°C. Only a weak relationship between the daily X values and the room temperature could be established in this study with a slope less than 0.02°C^{-1} and a correlation coefficient less than 0.3.

A third cold fusion study using the same palladium rod cathodes in fresh LiOD + D_2O solutions is shown in Fig. 7. Excellent agreement between the two thermistors in each cell was realized in this study where the thermistor tubes were flush with the cell top. This experiment shows a nearly consistent excess enthalpy production with only a few days yielding near unity for the heat ratio. However the daily mean X values are noticeably less than those for the first study with a freshly prepared palladium cathode (Figs. 3 and 4). The daily average room temperature ranged from 22.4–24.5°C in this study with a mean of 23.4 ± 0.5°C.

Tritium measurements on the LiOD + D_2O solutions following the first two experiments using the palladium rods were negative. The two LiOD + D_2O solutions gave 22.99 ± 2.63 cpm and 22.18 ± 1.06 cpm versus 19.34 ± 0.57 cpm for a

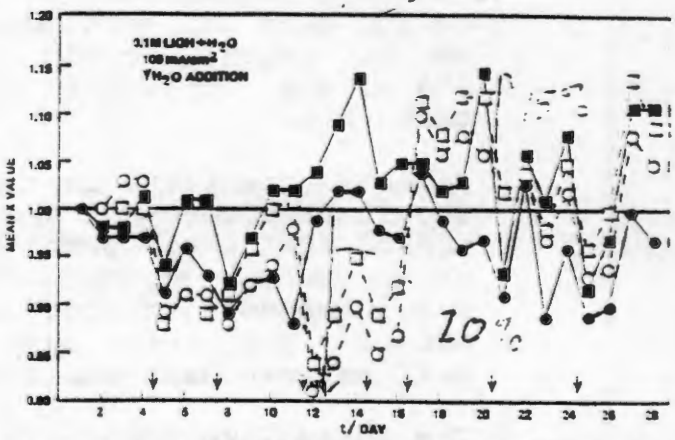

Fig. 6. Studies in water using palladium rod (Johnson Matthey, $d = 0.635$ cm). Symbols for cell A, cell B, T_1, T_2, T_∞, and T_5 same as in Fig. 5.

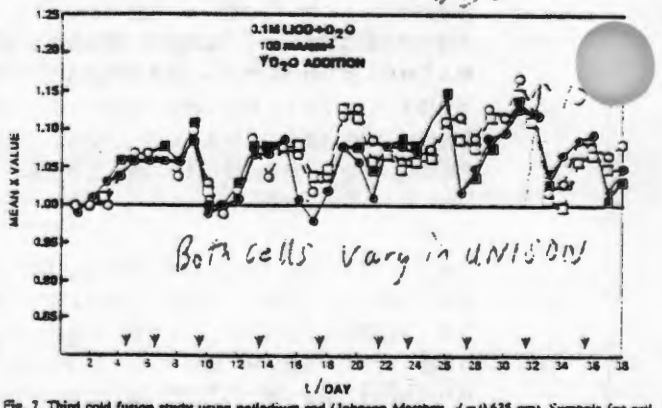

Fig. 7. Third cold fusion study using palladium rod (Johnson Matthey, $d = 0.635$ cm). Symbols for cell A, cell B, T_1, T_2, T_∞, and T_5 same as in Fig. 5.

during. Miles suggested that fluctuations were 2-3%. But Hansen noted that calibration-constant fluctuations were larger than that: 0.138 to 0.145, 0.132 to 0.138, 0.133 to 0.137, 0.135 to 0.141 (shown in table 3 of J. Electro. Chem. 296 (1990) 241).

Hansen learned that the China Lake calorimeter was NOT state of the art: only two temperature probes were used, and much of the heat flow was not through these sensors; the thermistor set higher in the cell tended to have higher readings (poor stirring? inconsistent insulation?) although sometimes the lower thermistor had a higher temperature (Miles in his paper explains: "thermal inversions were occasionally occurring" (p. 245 and 249 in J. El. Chem. 296:241) But why?); vermiculite was used for much of the insulation (unusual); the insulation was not well sealed against atmospheric moisture; the cells were NOT closed systems; the assumed thermal neutral potential (taken as constant 1.53 V for D_2O, 1.48 V for H_2O) actually depends on temperature and pressure. Much better, closed calorimeters are available and should be considered by Miles we think.

Notice in the D_2O plots shown above (Figs. 5 and 7) that the excess power goes up in down in the two separate cells at about the SAME TIMES. Why should the excess heat fluctuate in separate cells in unison like this? One suspects an environmental influence, not nuclear physics.

In a paper, Miles mentioned that "some unexplained excess heat effects were observed" in light water cells (JEC 304:1991, p. 276). During his lecture, he retracted this, saying that he now had an explanation for the heat reported in light water cells. (One hopes he will publish details.)

Who else sees both heat and helium-4? (Note: No tritium or helium-3 were detected by Miles et al.) Liebert and Liau of Hawaii have measurements of heat and helium-4, but the helium-4 is too small to correlate quantitatively with the heat by a factor of about 100 million. I know of no other quantitative claims.

3) Tritium claims.

Claims of detectable (i.e., large) amounts of tritium production without sufficient secondary neutrons (from d-t reactions), X-rays from Bremsstrahlung/lattice excitations, etc. have the same problem as alpha-particle production without secondaries (point 1 above). Nuclear reactions are of order of MeV and energetic nuclear debris must create secondaries, readily detectable if tritium is produced by nuclear processes in quantities claimed.

If one wishes to argue that the lattice picks up the energy instead of the triton, he or she should review point 1 above. Momentum must be conserved.

It is true that we at BYU claimed long ago a possible connection between helium-3 and tritium emanations from volcanos and other hot spots with possible fusion reactions within the earth (and other planets). Indeed, this hypothesis was key to our beginning "cold fusion" experiments at BYU in May, 1986 -- entirely independent of Fleischmann and Pons. U.S. Dept. of Energy funding to BYU for cold fusion studies, which began in May 1986, is still providing funding, and includes studies of volcanic tritium. Reports of tritium detected in magmatic waters were reported at the BYU conference (American Institute of Physics Proceedings No. 228). So, yes, I continue to think that tritium is or may be produced by natural fusion reactions, but at very low rates. Considering the scale of the earth compared with laboratory vessels, one might expect to be able to measure tritium in volcanic gasses, as we claimed in our original work (see J. Physics G: Nuclear Phys. 12: 213-221 (1986), Nature 338: 737-740 (1989), and J. Fusion Energy 9: 199-208 (1990) for details).

Let me add that I take the claims of tritium production of Srinivasan, Claytor, etc. as highly suggestive. But until careful workers such as these see evidences of nuclear energies attached to tritons (such as X-or gamma rays or 14-MeV neutrons), the case is just not definitive for nuclear reactions. Contamination or false readings remain as possible explanations.

A case in point: we received from the NCFI before its demise samples of deuterided titanium that they said contained tritium by the NCFI techniques. A 0.02g sample gave a 'tritium' activity of about 0.11 μCi. We examined these samples carefully with a sensitive germanium detector here. We found no tritium whatsoever. Nothing. I was frankly surprised, so I had the samples run again. Counts at the 1 σ level were repeated, and the statistical significance dropped lower with longer times in each case. Sorry, no tritium. (We have been using this and scintillation counting for months, without finding significant evidences of tritium from our own laboratory experiments. We tested that the germanium-detector technique works well by observing X-rays from a purchased titanium sample containing tritium.)

In the AIP Proceedings of the Provo meeting, you will find another instance of a false tritium reading that had to be corrected (p. 551). Still, several good researchers continue to claim tritium production -- but they do not have correlated and commensurate X-rays nor gammas nor 14 MeV neutrons. The community must bring forth nuclear-type evidences for putative nuclear processes!

Then I will worry about the truly frightening prospect of tritium production by simple means. We have more than enough tritium for its few peaceful applications. The major market for tritium is for hydrogen bombs, an industry that is happily on the decline.

But get this technology into the hands of certain tyrants... a terrifying spectre.

4) Is the "excess heat" due to nuclear reactions?

I am looking at serious studies of energetic particle (including triton) production by groups at Colorado School of Mines, NRL in Washington, at Texas A&M, in Japan, in China and in the Soviet Union. Indeed, we have reported preliminary evidence for low-level charged-particle production in our own charged-particle spectrometer (see Provo workshop proceedings, AIP Number 228, pp. 397-418.) A particle CARRYING a few MeV is clearly a signature for nuclear processes. On the other hand, tritium or helium in a vessel without associated secondary radiations could very well be due to error or contamination.

And again, one must be quantitative: charged particle production rates are roughly in line with neutron emission rates. These rates differ from what is needed for heat production by ten or so orders of magnitude, and thus REFUTE claims of a connection of heat and nuclear products.

I was bemused by the effort in Eugene Mallove's book Fire from Ice to connect the low-level nuclear observations with excess heat claims. He says that the Provo conference ironically proved the "cold fusion" connection to excess heat, or at least provided (I quote) "STRONG PRESUMPTIVE EVIDENCE that the basis for the excess power may be nuclear." (My capitals; see pages 251-253.) This is no proof at all. "Presumptive" is contradictory to "strong evidence."

If the nuclear products -- I must reiterate an insistence on nuclear energy attached to nuclear debris -- are NOT COMMENSURATE with "heat" measured calorimetrically, then there is NO CONNECTION between the two. $E = \Delta mc^2$ tells us this: we come back to fundamental physics. (E in this instance becomes heat whereas Δm reflects unavoidable nuclear products. Note the equal sign: excess heat and nuclear debris must be commensurate.)

It is terribly misleading to claim that a handful of nuclear products implies that the "excess heat" is due to fusion. John, we must be QUANTITATIVE. Here, the discrepancy between "excess heat" and nuclear (energetic) particles is roughly a factor of a trillion. One reason that I stayed away from the Lake Como meeting was to make a statement about the enormous gap between nuclear measurements and heat measurements. Let us stop this confusion.

Even Fritz Will, director of the University of Utah's Cold Fusion Institute before its demise, acknowledged the distinction:

"Will remains convinced, with others, that the nuclear effects in cold fusion are new scientific phenomena that have been proved 'in all likelihood.' He is much less confident about the excess

heat results and a possible connection with the nuclear effects. 'One has to work harder and harder in order to find whether or not there is a relationship,' he says. On the other hand, he completely discards the idea that the excess power has a chemical origin. He thinks that there could conceivably be some kind of mechanical energy storage and release mechanism at work in microcracks within the palladium." (Mallove, Fire from Ice, p. 259).

I conclude with a figure suggested by Nobel laureate Robert Schrieffer which endeavors to make the distinction clear (attached).

Best Regards,

Steven E. Jones

Steven E. Jones
Professor of Physics

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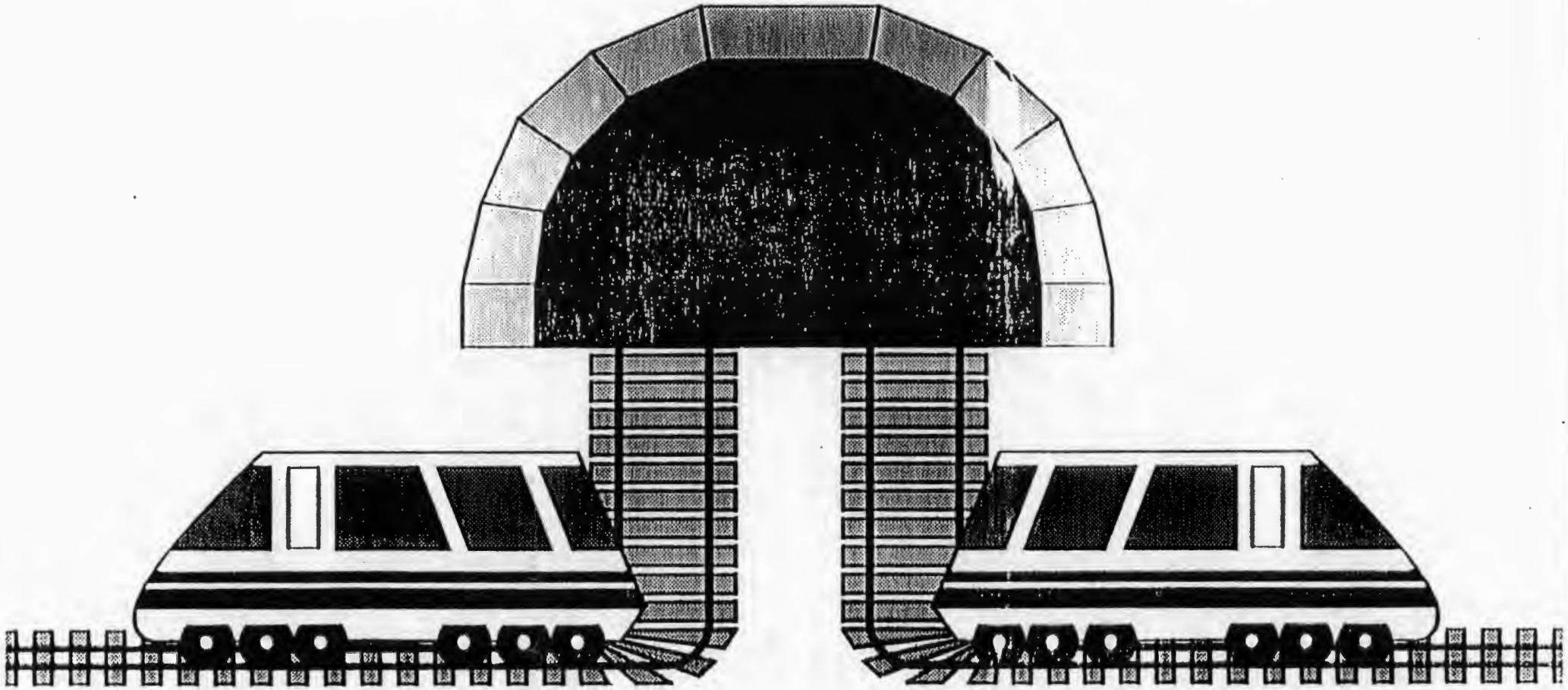
COLD FUSION CONFUSION



"Excess Heat"

(M. Fleischmann & B.S. Pons)

"Nuclear Particles"

(BYU, since 1985 paper
DOE-funded since May 1986)