if there is any proof that such spiking has occurred, and the first character answers, "No, we have no proof that used moderator heavy water is actually present in commercial heavy water, just circumstantial evidence such as the ultra-high tritium-to-deuterium isotope ratios. . . ."

Why this hypothesis is so troublesome to Storms and others is puzzling to me. The possible confusion of beta from tritium versus beta from ^{210}Pb is associated with radon decomposition, not heavy water spiking. Storms's wife, C. Talcott-Storms, is quoted in my book on her checklist to make sure that tritium and ^{210}Pb are not confused. I think Storms misinterpreted my concern about making sure that a beta emission is from tritium and not ^{210}Pb. This is a general concern, not linked to zirconium and high T/D ratios. Storms criticizes my comment that Russia is now selling palladium from reactor fission products. He says he inquired as to whether the palladium producers could include Russian palladium, and they answered, he says, "this possibility does not exist." When I questioned the palladium producers if their palladium could include Russian palladium, they told me that they buy palladium from many sources and could not guarantee "no Russian palladium." The problem with fission fragment palladium is not the tritium, as somehow Storms has interpreted my concern, but possible radioactive species within the palladium. On the other hand, palladium that has been contaminated with tritium during a previous use can be a problem because it can dissolve in molecular form within the electrolyte as well as escaping as TH or TD evolved gas.

Because I anticipated blasts from the pro- and anti-cold-fusion scientists in equal numbers but received the "excess heat" on my book only from the "pro" crowd, I wondered if I had been as evenhanded and objective as I thought I had been. Luckily, I received a very supportive "your book is objective" comment from an ardent pro-cold-fusion scientist, Professor Bockris. Perhaps to be sure that I was not "intellectually dishonest and should be censured by any competent scientist" (as my friend Storms describes me), I need to receive a blast from the anti-cold-fusion establishment!

Nathan J. Hoffman

ONTEC
12 Hutsoref 54
Beersheva, Israel 84112

January 31, 1996

COMMENTS ON "CALORIMETRY, EXCESS HEAT, AND FARADAY EFFICIENCY IN Ni-H_2O ELECTROLYTIC CELLS"

Concerning Ref. 1, which follows our paper[2] in *Fusion Technology*, Vol. 28, No. 4, we have several independent reports from excellent researchers documenting thermal energy release from our light water electrolytic cells; consequently, we are somewhat irritated by the Bose report in the confusion that it causes because our referenced experimental protocol was clearly not followed. Why?

The Bose scientists report that they did not produce energy in their light water electrolytic cells. They reference Mills' first paper[3] on electrolytic cell studies but surprisingly do not reference the second paper[4] even though Ref. 4 was published 5 months before the indicated submission date of Ref. 1.

Unfortunately, the Bose researchers did not follow either the published protocols from Refs. 3 and 4 or Ref. 5, which they also cite—all of which describe how to ensure the proper nickel surface necessary for the electrolytic cells to produce the catalyzed transitions to fractional-state hydrogen atoms and the consequent thermal energy release from those transitions. Everyone knows how important surface preparation is in catalytic chemistry. Shkedi et al. were doomed from the start. If they had contacted us, as they did others (see Refs. 12 and 14 on p. 1730 of Ref. 1), we would have faxed them the proper protocol.

Reference 4 specifically states (p. 109, right column, first full paragraph), "As usual in electrochemistry, measures were taken to avoid impurities in the system, especially organic substances," and further states later in the paragraph, "The nickel cathode was removed from the container with rubber gloves, and cut and folded in such a way that no organic substances were transferred to the nickel surface."

The Bose scientists may indeed have acquired some information from Ref. 4 because they used (Ref. 1, p. 1722, left column, third full paragraph) ". . . cold drawn nickel wire. . . ." See Ref. 4, p. 109, left column, last full paragraph. But, instead of following our protocol, they prepared the nickel electrode surface by cleaning it with not one, but two different organic solvents, and then proceeded to anneal the electrodes at 1100°C. Such a treatment would have left trace organic residue on the cathode and changed the "cold drawn" surface to an annealed surface. For these *two* reasons, we would have predicted the null event.

Having assembled a nonworking (nonenergy-producing) electrolytic cell, it is no surprise that the Bose scientists also failed to detect hydrino atoms: Hydrino atoms must be made before they can be detected. I would suggest a reading of Ref. 2. Fractional quantum energy level $n = \frac{1}{2}$ hydrogen atoms are clearly demonstrated in Figs. 4 and 6 through 9. We have studied several hundred spectra (and samples) since the data reported, and I believe that the *direct evidence* of their existence speaks for itself.

W. R. Good II

Hydrocatalysis Power Corporation
Great Valley Corporate Center
41 Great Valley Parkway
Malvern, Pennsylvania 19355

November 27, 1995

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RESPONSE TO "COMMENTS ON 'CALORIMETRY, EXCESS HEAT, AND FARADAY EFFICIENCY IN Ni-H_2O ELECTROLYTIC CELLS' "

In reply to Ref. 1 regarding Refs. 2 and 3, the following comments may help Mr. Good and other readers to better understand the significance of the results obtained by the Bose® group.

The notion that the Bose research in light water cells did not produce "excess heat" is simply a mistake (assuming the conventional definition of excess heat, based on the assumption that the Faraday efficiency is unity). The conclusion of the Bose research was not that "excess heat" has not been produced in the light water cells. On the contrary, "excess heat" was produced and measured in all the light water open cells. The results are reported in Tables I through IV in the columns labeled "Apparent Excess Heat (%)" in Ref. 2. In this regard the results agree with the findings of many others who reported positive "excess heat" in light water cells. Again, this is all subject to the assumption that the Faraday efficiency is unity.

The cold-drawn wire cathodes in those cells were indeed cleaned with acetone and methanol and baked at 1100°C. However, what organic residue can survive 1100°C for 2 h as postulated by Mr. Good? The Fibrex® cathodes, on the other hand, were not cleaned or treated in any way. The same Fibrex material also produced "excess heat" for other researchers. Cleanliness of handling was even better than that suggested by Mr. Good. Not even gloves were permitted to touch the cathodes—only specially cleaned stainless steel tools and platinum wire. In either case, both types of cathodes produced the same level of "excess heat." Cell W16E7 (Table III of Ref. 2) was also intentionally contaminated with an organic material. Yet, even the organic contamination could not cause the "excess heat" to disappear.

The difference between the Bose research and all other published research in the field is that once "excess heat" was found, Shkedi et al. did not pause to celebrate but continued the research to identify the source of the "excess heat." To everyone's surprise, including ours, the source of the "excess heat" was identified as unaccounted internal recombination of hydrogen and oxygen. In other words, the common assumption that underlies almost every "successful" light water experiment, i.e., that the Faraday efficiency is unity, was proven to be wrong.

When the "excess heat" data were analyzed, taking into account the actual Faraday efficiency measured in real time, all "excess heat" disappeared, and the energy balance turned out to be exactly zero. The data, the methodology, and the analysis are all presented in Ref. 2. The key point is that "excess heat" has been produced, but its source is shown to be neglected conventional chemistry. Hence, the use of the terms "excess heat" or apparent excess heat rather than simply excess heat.

By contrast, the high "excess heat" ratios claimed to be produced by Mills and Good are predicated on the assumption stated in Ref. 3, on p. 1699 following Eq. (7): "The net faraday efficiency of gas evolution is assumed to be unity." This is quite a heavy assumption for such a controversial topic.

Apart from many possible errors in determining input power under pulsed current conditions, the erroneous effects of neglected Faraday efficiency are further amplified by the use of pulsed current with a small duty cycle. While input power is assumed to be applied only during the "on" time of the pulse, internal recombination takes place 100% of the time. Thus, the error introduced by neglecting the Faraday efficiency is multiplied by a factor approximately equal to the inverse of the duty cycle.

The need to accurately account for the actual Faraday efficiency in real time is extremely critical. As shown by Eq. (10) in Ref. 2, the apparent excess heat ratio, ignoring the Faraday efficiency, is given by

$$\textit{apparent excess heat}\ (\%) = 100 \cdot \frac{E_{out} - \int (V - 1.481) I\,dt}{\int (V - 1.481) I\,dt} \ .$$

Because of the singularity near an electrolysis voltage of 1.481 V, a small favorable error in determining the actual Faraday efficiency can give rise to an erroneous "excess heat" ratio of near infinity, while in reality it is zero!

The objective of the Bose research and publication was not to duplicate, prove, or disprove any other work. The objective was to introduce a new standard of experimental methodology and calorimetric accuracy into the field of "cold fusion." All of us in this universe would be very grateful if anyone could demonstrate long-term real excess heat, free from errors related to Faraday efficiency or duty cycle, or any other error sources.

We have yet to see an excess heat demonstration that lasts continuously for many months in a calorimeter that meets or exceeds the accuracy and stability demonstrated by the Bose calorimeters. The challenge presented at the conclusion of Ref. 2 is still open. ". . . all reports claiming the observation of excess heat should be accompanied by simultaneous measurements of the actual Faraday efficiency."

Will anyone pick up the glove?

Z. Shkedi

Bose Corporation
The Mountain
Framingham, Massachusetts 01701-9168

January 18, 1996

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