

The Alternate View

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LENR PART 1

Now and then, the subject of "cold fusion" comes up in the *Analog* online forum, and I, as the resident expert, wind up entering discussions on the topic. Unfortunately, what soon happens is that I start getting questions—usually pretty good ones—that I can't possibly answer in the amount of time I'm willing to spend hanging out on the Internet, let alone the fraction of that time I have to devote to the forum.

Sometimes those questions are as broad as "How does it work?" I don't know the answer to that—if I did, I'd be on my way to Sweden for the Nobel Prize. The best I can do is to inform the questioner that much good work has been done since 1989. Indeed, most of the objections to the "cold fusion" methodology have long since been answered. Also, scientists both regularly obtain reproducible "cold fusion" results in this country, and elsewhere (see Gene Mallove's fact article in the July/August 1997 *Analog*). I also tell the questioner where he can go to find out more for himself.

One common reply to the above answer is an insistence on the part of the questioner that, if all I said was *really* true, then he'd already have heard about it. From my point of view, there's no reason for me to answer that assertion (at least, not for free). I know what I've said *is* true, and I can't help it if others have not been informed. The best way for them to find out why they are uninformed is to go and look in those places I'd already suggested that they go and look.

But I do have this column, and I do get paid for writing it, and there is this new "cold fusion" site starting up on the Web, so now is as good a time as any to once again discuss "cold fusion."

Even the non-astute reader must have noticed by now that I keep putting the words "cold fusion" in quotations. The reason for this is simple—I don't know whether or not that which is called "cold fusion" actually *is* fusion, or if it's something else. I can tell you that excess heat has been measured reliably (that is, heat in excess of that expected from any chemical source given the particular experiment in question), and I can also tell you that nuclear transmutations have been seen and measured (but more about that in Part II). Indeed, the preferred term for "cold fusion" work is fast becoming LENR, short for Low Energy Nuclear Reactions.

But first, I've long been itching to explain *why* I've never bought into the accepted view that "cold fusion" (hence, LENR) is all a bunch of hooey. I will finish out Part I with my relevant intellectual history.

I attended a small liberal-arts school called Calvin College. Though the physics department there is small, nevertheless it is of high quality. One of the distinct advantages of studying physics there is that undergraduates often get to do graduate-level work since all of the professors are Ph.D.s with their own ongoing projects, and they need to get their cheap labor from somewhere.

In the summer of 1986, just before I went off to teach high school for a couple of years, I was privileged to work for Dr. John Van Zytveld, measuring the thermoelectric powers of high-purity rare earth elements in the liquid phase. Space prohibits my going into a discussion of exactly what a "thermoelectric power" is, so suffice to say it's just one of many electrical properties that a metallic element can have. This particular property is temperature-dependent, and also phase-dependent, since the numbers (measured in microvolts per degree Celsius, by the way) change quite strikingly when you transition from the solid to the liquid phase.

How does one go about making these measurements? The rare earth elements I used (praseodymium, neodymium, and samarium that summer) came highly purified to begin with from the supplier, but not pure enough for our purposes. You see, the rare earths tend to accumulate hydrogen inside their lattices,

and I had to get that hydrogen out. The way I did that was to bake it out under high vacuum and above the melting point.

This is how I went about preparing a suitable sample upon which to perform measurements. I'd take the lump of, say, praseodymium, and insert it into the glove box which was then evacuated and back-filled with argon (most rare earths are highly reactive with air, so I had to work in a non-reactive environment). Once in the glove box, I'd spend most of several days cutting the sample up into little chips, which I dropped into a tantalum tube. This tube had been sealed and welded with a tantalum plug on the bottom, and would be treated the same way on the top once I filled it with chips. Once the tantalum tube was filled and welded closed, it was suspended vertically in a custom-built furnace and brought up above the melting point of praseodymium (just under 1000 C), all the time being kept under vacuum. The tantalum tube is porous to hydrogen at those temperatures, so the hydrogen would get pumped out, and in the meantime, the chips would melt and form a nice three-inch-long cylinder in the bottom of the tantalum tube. This melt-and-bake-out part of the procedure lasted about three days.

Once the tube had cooled down and was out of the furnace, back into the glove box it would go, so I could remove the praseodymium from the tantalum tube. To do that, with a hacksaw I would make a spiral cut down the bottom four inches of the tantalum tube and then, with pliers, peel the tantalum off from the praseodymium sample.

This took another few days.

Once the sample was out of the tube, suitable holes had to be drilled into it (with a manual drill kept in the glove box) to receive the thermocouple probes used to take the actual measurements. After that, it would be time to remove a large part of my test rig (which had been baked out and cleaned in another complicated ritual I won't explain) from its holder and place it also into the glove box so that the praseodymium sample could be emplaced within it.

This last was an extremely tedious process, since the unit from the rig into which I had to slide the sample was almost as long as the glove box was wide, further cramping the limited space I had to work with. This part of the job could again take hours, and maybe even a day, and then there was the reattaching of the unit to the rig, retesting the rig to make sure of its integrity, another baking-out process to eliminate contamination, and so on.

At this point I should also explain that what I was doing that summer was simply the culmination of years of earlier work that went into making my job as easy as it was. A lot of failures were responsible for giving me the test rig I inherited, and the sample preparation and testing procedure I followed.

The first sample of praseodymium I took measurements on required me to nurse the rig along for about 35 hours straight. You couldn't start the experiment and then stop it when you got tired. I had to heat the sample, take some measurements, heat it more, take more measurements, finally bring the sample up to the solid-solid phase transition (the crystalline structure changes from one ordering to another), get measurements there, and then push it through the melting point. This was critical, since earlier attempts had failed at this point. Liquid rare earths are highly reactive—previously, an alumina (a tough ceramic) containment vessel was used to hold the sample, but the praseodymium ate it up once it became a liquid. For my run, I used a BeO (a tougher ceramic) vessel.

To shorten a very long story, I was probably the first person in the history of the world to know what the thermoelectric power of pure praseodymium in the liquid phase is. This was because our lab knew *exactly* how to purge rare earth elements of hydrogen contamination, and the tricks that *had to be used* to obtain a successful measurement.

That particular summer was spectacular in terms of successful results. I measured the thermoelectric power of both Pr and Nd, and then got started on samarium. Ah, samarium—Kooistra's Bane. The first

samarium run went well, right up until it melted, at which point the samarium ate up my BeO vessel, my thermocouples, my stainless steel holder, and was on the way to eating up the furnace before I pulled the plug. Liquid samarium is *really* reactive stuff.

So ended my research that summer.

Two years later, having finished my stint as a high school teacher, I returned to the lab and began work on gadolinium. This metal melted at a higher temperature than the previous ones, and the rig had to be reengineered to reach those temperatures. This was demanding from a design standpoint, in that we were really pushing our rig to the limit of what it could do. That summer was solid frustration. I didn't get in a single run on gadolinium, and it wasn't until the following spring, in 1989, that I was finally able to take any measurements. Even then, the unit failed when I got to the melting phase. We thought we were pushing to the limits of the technology, but we had pushed beyond.

It was on March 23 of 1989 that the news of "cold fusion" hit. There was tremendous excitement in the department, and some instant rushes to judgment on the negative side. (The "positive side" was made up of those of us who wanted to be open-minded.)

In the larger world, within weeks of the announcement came both supportive positive findings and a lot of negative findings. And then within months, "cold fusion" was written off as a mistake, and even as "the fiasco of the century."

Oh, give me a break!

Those few months affected my understanding of how science works more than anything before or since. I knew how hard it could be to perform a good experiment even when you'd had months to prepare it, even when the best expert in the world was your mentor, and even when many of the tricks had been worked out long before you came on the scene. And now some scientists from a few "prestigious labs" were trying to tell me that they, having grabbed the first lump of palladium that was at hand, had tried to replicate the "cold fusion" experiment and failed, so therefore, Pons and Fleischmann hadn't done it either.

Bullshit.

It may have been true that Pons and Fleischmann hadn't done what they'd actually thought they'd done. And it was likely that many—perhaps all—of the hasty positive replications were false positives. But I myself was intimately familiar with how one did a particular kind of complicated—though entirely mundane—physics experiment, and I don't care if you brought in a pack of masters from Super-genius University: they would *not* have been able to replicate my experiment in a few weeks time.

It is true that only a few years before, high-temperature superconductors had been fabricated, and it was amazingly simple for others to replicate these, given the recipe. But this was the exception, not the rule. LENR is more akin to trying to fashion *wires* out of high temperature superconductors, and that is not at all easy.

So, LENR experiments—hard to do? You betcha. But in Part II, we'll see what some well-trained and experienced scientists have accomplished, and I'll tell you of a new website where you can follow the action.