Pathological Science: An Update

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From N-rays to cold fusion, scientists have been seeing things that aren't there.

The Nobel chemist Irving Langmuir (1881-1957) used to give a cautionary talk on pathological science, and photocopies of a transcription of his December 13, 1953, colloquium circulated for years before being published in 1989 (Langmuir 1989). In this memorable talk, Langmuir told a number of stories of pathological science and listed the features they have in common. Here I would like to retell two of these stories and add one of my own. These case studies clearly show features common to all pathological science and how such pathology can arise even among competent scientists. With this background, the recent case of cold fusion is seen as a textbook example of pathological science.

One of Langmuir's stories goes back to 1903. Wilhelm Roentgen's discovery of X-rays in 1895 was a major event in science and initiated a burst of new research. While most scientists were content to learn as much as possible about this mysterious new emanation, others wanted the glory of discovering emanations of their own. So perhaps it wasn't surprising that in 1903, Prosper René Blondlot, a distinguished member of the French Academy of Sciences, announced the discovery of N-rays which he had produced by heating a wire inside an iron tube.

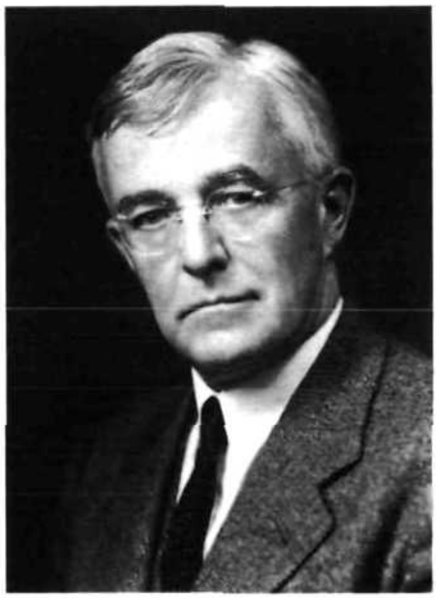
These rays didn't pass through the iron, but did pass through an aluminum window in the iron. They were detected by looking at a very faintly illuminated screen in an otherwise dark room. If the N-rays were there, the screen became more visible; of course a great deal of skill was needed for this because the screen was just on the edge of visibility. Under these conditions, he discovered that many different things give off N-rays, including people. Then he discovered negative N-rays, which decreased the visibility of the screen. He published many papers on the subject, and so did many others, confirming a multitude of unusual properties for these rays.

Among them was the fact that they could be broken up into a spectrum by passing them through a large aluminum prism. In 1904, the American physicist R. W. Wood visited Blondlot and found him measuring, to a tenth of a millimeter, the position of the N-rays as they came through an aluminum prism. "How is that possible," Wood asked, "when the original beam is coming from a slit two millimeters wide?"

"That's one of the fascinating things about the N-rays," Blondlot replied. "They don't follow the ordinary laws of science." So Wood, the room being very dark, removed the aluminum prism that was bending the N-rays onto Blondlot's screen and put it in his pocket. Blondlot, unaware of this, continued getting the same results. Wood published a report of this incident, in *Nature*, which put an end to N-rays.

In 1934, Langmuir himself visited the parapsychologist J. B. Rhine at Duke University and pointed out that Rhine's work had all the characteristic symptoms of pathological science. Rhine thought it would be great if Langmuir published this. "I'd have more graduates," he told Langmuir. "We ought to have more graduate students. This thing is so important that we should have more people realize its importance. This should be one of the biggest departments in the university."

Rhine had begun his studies in extrasensory perception at Duke University in 1930. Most of these were done with cards showing one of the five ESP symbols: a circle, a cross, wavy lines, a rectangle, and a star. Usually a deck was used that had five cards of each kind, 25 in all. The deck would be shuffled and cut, and the subject would call the cards in the order they were picked from the top of the deck. Since there are five different cards, there is a one-in-five



Nobel laureate Irving Langmuir, who often warned of pathological science (photo courtesy of GE Research and Development Center).

chance of a correct call. Results are usually reported as the number of correct calls out of 25. If there is no extrasensory perception, the average of many scores would be 5, although individual variations of plus or minus 3 aren't unlikely.

Langmuir spent the whole day with Rhine, who was in a philosophical mood. "People don't like these experiments," he said. "They don't like me. Sometimes, to spite me, they made their scores purposely low [less than 5]. . . . I took [these low results] and sealed them in envelopes and I put a code number on the outside, and I didn't trust anybody to know that code. Nobody."

Langmuir thought this interesting. "You said that you had published a summary of all your data and that the average was 7. Now you are saying you have additional data that, if added to your published data, would bring the average to 5. Will you do this?"

"Of course not," he said. "That would be dishonest. The low scores are just as significant as the high ones, aren't they? They proved that there is something there just as much, and therefore it wouldn't be fair [to combine negative and positive data]."

Rhine felt justified in withholding low scores from his average because he believed the low scorers were deliberately (or paranormally) producing their low scores. Such self-deception is a common human failing, not restricted to occultists. Mainstream scientists sometimes delude themselves as well, as the following case shows.

The A_2 is an elementary particle created in experiments in which pi mesons from a high-energy accelerator collide with the proton nuclei of ordinary hydrogen. The direction and energy of the recoil proton after each collision is measured by a complex array of electronic detectors. The data from the detectors are stored on magnetic tape for later computer analysis. If a particle is created in some of the collisions, it appears as a bump in a plot of the analyzed data. All such bumps, technically called resonances, have similar shapes, which follow from fundamental theory. But in 1967, a group at the European Center for Nuclear Research (CERN) in Geneva found that the resonance of the A_2 had an anomalous dip in the center (Figure 1); the resonance was split (Chikovani et al. 1967).

This was a startling discovery, contrary to all experience. As seen in Figure 1, the CERN dip has only a few points in it, each with a probable error that is one-quarter the size of the dip. Still, the probability of this being a statistical fluctuation is less than 0.1 percent. Immediately, theorists started churning out papers to explain the greatest anomaly since parity violation.

The position in energy of the A_2 dip had been so well determined by the CERN equipment that the experimental group from the Northeastern University Physics Department planned to use it to check an experiment they were doing at the Brookhaven National Laboratory on Long Island, New York. To their great disappointment, they didn't see the dip. A dip is something that could easily be missed if there were a problem with the experiment, but something that was unlikely to be created by a problem. Thus the Northeastern group first thought the problem was theirs, not CERN's. But after repeated checks of their equipment revealed no problems, and repeated experiments continued to show no dip, they announced their results at a stormy meeting of the American Physical Society in 1971 (Bowen et al. 1971).

A spokesman for the CERN experiment vigorously defended its result, claiming that the *fact* that CERN saw the dip *proved* that the CERN experiment had better resolution. To which Bernard Gottschalk, speaking for Northeastern, replied, "Seeing spots before your eyes doesn't mean you have better vision." And so, amid cheers and catcalls, the physicists argued their cases. Within a few months, Northeastern's results were confirmed by other groups, and the dip was never seen or heard of again.

This leaves the question of how a group of distinguished scientists, using the best equipment in the world,

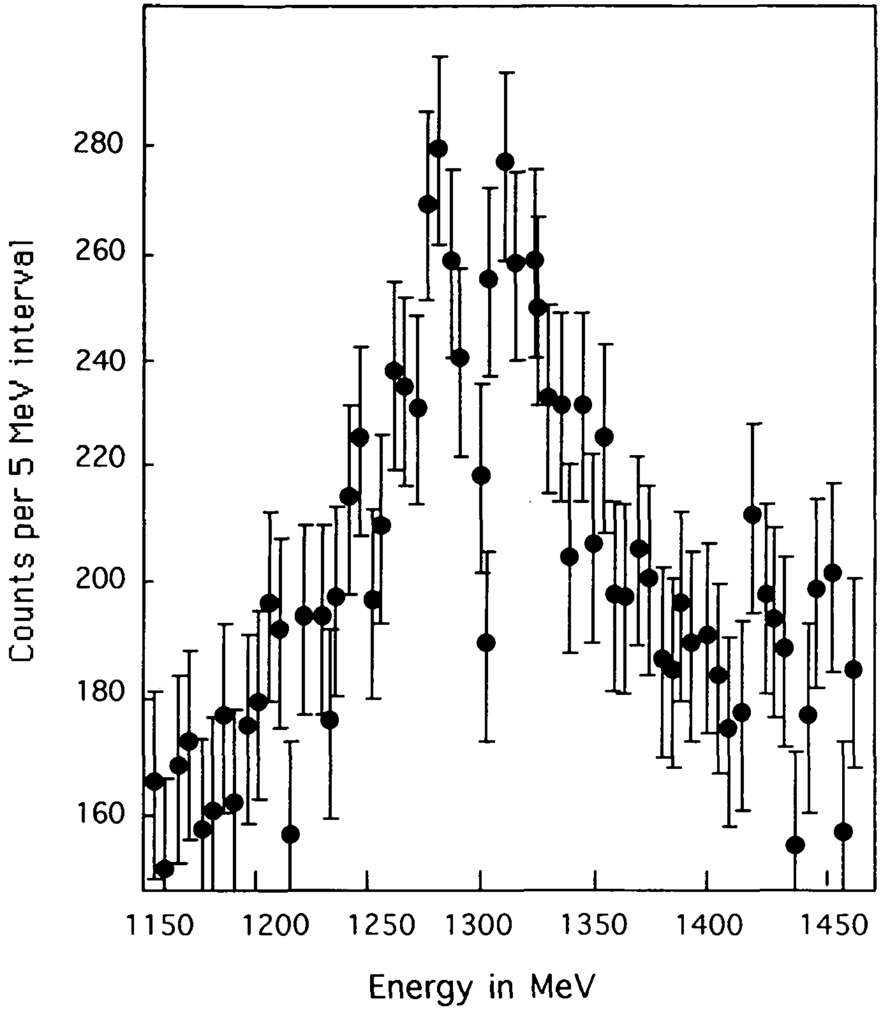


Figure 1. The A_2 resonance as reported by the experimental group from CERN (Chikovani et al. 1967). The anomalous (and erroneous) dip in the middle of the resonance was caused by a bias in the way the data were selected.

could see something that wasn't there. The CERN group, it seems, did exactly what Rhine had done—discarded data that didn't show what it wanted to find. As one CERN scientist so ingenuously explained to Gottschalk: "We broke the data into batches. Whenever we found a batch with no dip, we looked very carefully for something wrong and we always found something." Since they didn't look so carefully for trouble when a batch showed a dip, and since there is always something wrong in every run of a highly complex experiment, they managed to boost an initially insignificant glitch into pathological science.

This story is worth telling because we are dealing here, not with a few benighted occultists, but with a large team of highly trained scientists working with a mountain of electronic equipment and computers. But in the end, both Rhine and the CERN group were fishing in the noise for inexplicable phenomena.

Langmuir found that the cases of pathological science he had studied shared certain characteristics. They are generally claims, based on a weak or marginal effect, of fantastic phenomena contrary to all experience. There are conflicting reports from independent investigators. Reasonable explanations of the data, based on known science, are rejected. Interest rises rapidly for a time, then gradually fades away.

Nothing follows this description more closely than the case of cold fusion. [For a previous discussion, see Milton Rothman's "Cold Fusion: A Case History in 'Wishful Science'?" *SI*, Winter 1990, pp. 161-170.] It was at a press conference on March 23, 1989, that Stanley Pons, a chemist at the University of Utah, and Martin Fleischmann, a chemist from the University of Southampton, England, first announced that they had obtained a controlled fusion reaction in a small electrolytic cell. This claim is certainly contrary to all experience or understanding of both nuclear and solid-state physics. Furthermore, Fleischmann and Pons didn't detect the lethal dose of neutrons that should have accompanied the amount of fusion they reported. Their claim was doubly fantastic. A miracle squared.

Immediately after the press conference, a group of physicists at Brigham Young University, headed by Steven Jones, made a similar announcement. The two groups had been working independently, but became aware of each other's work some months earlier. At a meeting that involved the presidents of these two Utah universities, an agreement was reached that on March 23, 1989, each group would submit a paper on its work to *Nature*. Pons and Fleischmann also announced their results at a press conference on the same day, which wasn't part of the agreement.

Nature accepted the Jones paper (Jones 1989), but not the Fleischmann and Pons paper. This didn't delay publication, however, because Fleischmann and Pons had already had their cold-fusion paper accepted by the *Journal of Electroanalytical Chemistry* (Fleischmann, Pons, and Hawkins 1989).

How inconsistent were the Utah results with previous experience? Fusion reactions—nuclear reactions in which two light nuclei combine to form a heavier one—have been intensely studied for more than 50 years. They are the basis of the hydrogen bomb, of the energy production of the sun, and of research efforts to produce controlled fusion on earth. In the Utah experiments, the reaction involved the fusing of two deuterium (hydrogen-2) nuclei, which generally produces either a tritium (hydrogen-3) nucleus and a proton or a helium-3 nucleus and a neutron. The fusion of two deuterium nuclei into a helium-4 nucleus and a gamma ray occurs less than one percent of the time.

A deuterium nucleus consists of one proton and one neutron; it is an isotope of the hydrogen nucleus, which is just a single proton. Normally the deuterium nucleus has an electron encircling it, in which case it is an atom of deuterium (heavy hydrogen). Deuterium, like hydrogen, is a gas at room temperature; in this gas the deuterium atoms are combined in pairs, forming a molecule.

The difficulty of achieving fusion comes from the fact that two deuterium nuclei repel each other electrically, because each is positively charged. They normally don't get close enough to interact. In a deuterium molecule, the electrons overcome this repulsion to a large extent, and the two nuclei in the molecule are 7-millionths of a millimeter apart. This is still too far apart for them to fuse, but Jones had previously achieved fusion by replacing one of the electrons in a deuterium molecule with a negatively charged muon (Rafelski and Jones 1987). The muon is 200 times heavier than an electron and brings the two deuterons 200 times closer together. At this separation, fusion occurs at a measurable rate, though not enough energy is released to pay for the cost of creating the muons.

It thus is clear that cold fusion is possible if two deuterium atoms can be squeezed together closer than they are in a deuterium molecule. It is also well known that many metals, including palladium, absorb hydrogen. Therefore, isn't it reasonable to suppose that, if deuterium were forcibly incorporated into palladium using an electrical current, deuterium atoms could be squeezed together close enough for their nuclei to fuse?

No, it isn't. The palladium atoms are themselves three times farther apart than are the two deuterium atoms in a deuterium molecule. The palladium is able to absorb deuterium molecules *because* the spacing between the palladium atoms is larger than the diameter of the deuterium molecule. No squeezing is involved. In fact, the deuterium molecule breaks apart inside the palladium, and its two deuterium atoms end up being farther apart in the palladium than they were in the free deuterium molecule.

Furthermore, Fleischmann and Pons claimed their fusion reaction generated a large quantity of heat. A simple calculation shows that if the heat they claimed were due to fusion, there would have been enough neutrons generated to have killed the experimenters. They took the absence of the neutrons as the discovery of a new type of nuclear reaction.

Scientists weren't immediately aware of all this when the announcement was made at a press conference. So when reporters asked scientists for their assessment of the Utah experiments, there were mixed responses. Philip Morrison said, "Based on the information I have, I feel it's a very good case." He said his confidence in the reality of the reaction was "high, but not conclusive." The Nobel physicist Sheldon Glashow said, "I don't believe a word of it" (Chandler 1989b). The amusing comment of Kim Molvig—"I am willing to be open-minded, but it's really inconceivable that there is anything there" (Pool 1989)—probably reflects the ambiguous use we often make of "open-minded." Most alarming were the comments from scientists who put extraordinary confidence in Fleischmann and Pons: "I'd be extremely surprised if they've done anything stupid. They have a very good track record in electrochemistry. I am pretty excited about this" (Chandler 1989a). In fact, stupidity, or human error, or self-delusion in science, is far less surprising than is the radical overthrow of well-established doctrine.

A number of confirming experiments were reported soon after cold fusion was announced, followed by a deluge of nonconfirming experiments. At one point, we had the peculiar situation that physicists largely rejected the cold-fusion claims, while many chemists accepted them. In July 1989, a U.S. Department of Energy panel stated that there wasn't sufficient evidence of cold fusion to warrant government funding. Still, Fleischmann and Pons—together with the remnants of their followers—carried on for another year, supported by the largesse of the State of Utah.

Jones and his group did detect a slight excess of neutrons above background radiation, but mostly in the first hour of the experiment. Some other groups also reported excess neutrons, but many groups didn't see any more neutrons than are usually present in background radiation. So this is clearly a marginal effect. The heat claimed by Pons and Fleischmann—though not in principle marginal—in fact resulted from heavily processing data obtained by incorrect procedures. They never actually generated the heat they talked about.

But for the layperson, unfamiliar with the science involved, the most characteristic sign of pathology was the language used. Fleischmann and Pons's insistence that "it is inconceivable that this [heat] could be due to anything but nuclear processes" isn't the language of science. It is the language of minds fixed on their own strongly held beliefs and unwilling to listen to the justified skepticism of others.

There are many lessons from this episode. First, scientists themselves are often poor judges of the scientific process. Many took Fleischmann and Pons's incredible conclusions about their own work at face value, before even reading their papers.

Second, scientific research is very difficult. Anything that can go wrong will go wrong. Fleischmann and Pons forgot to stir their cell while measuring its temperature, totally invalidating their measurements. Working in secrecy and isolation, even experienced scientists will be hindered by the lack of guidance and criticism of others.

Third, science isn't dependent on the honesty or wisdom of scientists. It is a collective enterprise that seeks to obtain the broadest possible consensus among its practitioners (Ziman 1968). It will survive Fleischmann and Pons, but only after the wasteful expenditure of hundreds of man-years of work and at least one death (Dye 1992).

Real discoveries of phenomena contrary to all previous scientific experience are very rare, while fraud, fakery, foolishness, and error resulting from overenthusiasm and delusion are all too common. Thus, Glashow's closed-minded "I don't believe a word of it" is going to be correct far more often than not. As Langmuir said about earlier nonexistent phenomena:

> These are cases where there is no dishonesty involved, but where people are tricked into false results by a lack of understanding about what human beings can do to themselves in the way of being led astray by subjective effects, wishful thinking, or threshold interactions. These are examples of pathological science. These are things that attracted a great deal of attention. Usually hundreds of papers have been published upon them. . . .
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> [But] the critics can't reproduce the effects. Only the supporters could do that. In the end, nothing was salvaged. Why should there be? There isn't anything there. There never was. (Langmuir 1989)

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