Case Studies in Pathological Science

How the loss of objectivity led to false conclusions in studies of polywater, infinite dilution and cold fusion

Denis L. Rousseau

Scientists are often viewed as "committed to truth, unbiased by emotion, open to new ideas, and professionally and personally unselfish," according to Michael J. Mahoney, an American author and psychologist. Similar sentiments have given rise to a widespread image of the archetypal scientist—someone painstakingly obtaining objective data, testing every side of a question and disregarding personal interests. Like other archetypes, however, this flawlessly competent and dispassionate scientist does not exist. Even scientists may lose objectivity in the pursuit of truth. John Locke, the 17th-century English empiricist, recognized this possibility when he wrote: "Error is not a fault of our knowledge, but a mistake of our judgment giving assent to that which is not true.... It is in man's power to content himself with the proofs he has, if they favor the opinion that suits with his inclinations or interest, and so stop from further research."

Errors in science created by a loss of objectivity consistently exhibit a similar set of characteristics. Irving Langmuir, the late Nobel prize–winning chemist from General Electric, generated a formal model of this syndrome and called it *pathological science*. He described six "symptoms" of this "disease." I have condensed Langmuir's six symptoms into two characteristics and added a third, which I believe is the most important.

The first characteristic of pathological science is that the effect being studied is often at the limits of detectability or has a very low statistical significance. Thus it can be difficult to do experiments that reliably test the effect. In some instances, subjective visual observations replace objective instrumental measurements; in other cases, only sophisticated analyses can reveal a statistically significant effect. If the effect is at the edge of detectability and is measured by visual observation, unconscious personal bias may affect the results.

Because the effect is so weak or of such low statistical significance, there may be no consistent relationship between the magnitude of the effect and the causative agent. Increasing the strength of the causative agent may not increase the size of the effect. This is usually attributed to an incomplete understanding of all of the variables that control the effect. Once the investigator has become convinced that something new and important has been discovered, the fact that all of the parameters involved in its development are not under control is viewed as having little consequence at the early stages of the "discovery."

The second characteristic is a readiness to disregard prevailing ideas and theories. Of course, if the effect that has been discovered is not real, it may not fit into the established theoretical framework. Proponents of the effect might therefore concoct fantastic theories to account for the new phenomenon. Some of these theories violate a multitude of established physical principles, whereas others only mildly distort fundamental ideas. When confronted with the dilemmas that the new theories create, their proponents either ignore the criticisms or offer ad hoc excuses to dismiss the criticisms. By putting forth a new theory, the investigator becomes still more deeply committed to the new discovery because, with both a remarkable experimental observation and a revolutionary theory, major international prizes may be waiting over the horizon.

To avoid these pitfalls, scientists must conceive and carry out a critical series of experiments. Ideally, the experiments give a definitive answer—either the effect is real or it is not. But the third identifying trait of pathological science is that the investigator finds it nearly impossible to do such experiments. The results could be devastating. To avoid confronting the truth, the investigator selects experiments that do nothing, except perhaps add another significant figure to the result or measure a variant of the phenomenon. The investigator never finds the time to complete the critical measurement that could bring down the whole house of cards.

What happens if someone else does a critical experiment that reveals a fatal flaw in the so-called discovery? The experiment is not accepted. Proponents of the effect claim that methodological mistakes, contamination or a missing key ingredient caused the negative result. No matter how carefully the experiments are performed or how many attempts are made, there is always some excuse for rejecting a negative outcome.

This description of science gone bad is not a portrait of deliberately fraudulent behavior. Pathological science arises from self-delusion—cases where scientists believe they are acting in a methodical, scientific manner but instead have lost their objectivity. The practitioners of pathological science believe that their findings simply cannot be wrong. But any idea can be wrong, any observation can be misinterpreted.

There are many examples of nonobjective science. In contrast, deliberately fraudulent work is rare. It is self-delusion and the associated sloppiness that spawn most errors in science. Occasion-

Denis L. Rousseau is a Distinguished Member of the Technical Staff at AT&T Bell Laboratories. He received a Ph.D. in physical chemistry from Princeton University. After two years as a research associate in the Physics Department at the University of Southern California, he joined Bell Labs in 1969. He is in the Biological Computation Research Department, where his work is centered on biophysical studies of proteins. Address: AT&T Bell Laboratories, 600 Mountain Ave., Murray Hill, NJ 07974.

ally, the putative discovery is so important that it gets a great deal of attention and stimulates a large part of the scientific community to move in a new direction. The three examples discussed here are such cases. I tell these stories not to ridicule those who turned out to be in error but rather to warn of a danger to which anyone in the scientific community could be vulnerable.

Capillary Conjuring

In the 1960s and early 1970s reports of a new form of water called polywater astounded the scientific community. N. N. Fedyakin of the Kostrama Polytechnical Institute in the U.S.S.R. reported that water condensed in a capillary tube is different from normal water. Fedyakin joined with Boris V. Derjaguin of the Institute of Physical Chemistry of the U.S.S.R. Academy of Sciences to describe the characteristics of this new water. They found that polywater froze into a glass-like material at –50 degrees Celsius and boiled at about 300 degrees Celsius. Polywater was more dense and viscous than normal water. Fedyakin proposed that polywater had a new and unknown structure.

Polywater is made by placing freshly drawn capillary tubes in an atmosphere that is nearly saturated with water. Through temperature control, the vapor pressure of the water surrounding the capillary is held slightly below saturation to deter normal condensation of water in the tube. After a few days, a condensate forms inside the capillary tube. Normal water is removed from the condensate through evaporation, leaving only the thick polywater.

Polywater received little attention outside the U.S.S.R. until Derjaguin presented his findings at international meetings in the late 1960s. His reports enticed Ellis R. Lippincott of the University of Maryland and Robert R. Stromberg of the National Bureau of Standards to enter the polywater arena. Lippincott, Stromberg and their colleagues applied infrared spectroscopy to the new substance. This spectrum reveals the geometry of a molecule and the energy of its bonds. Polywater yielded a surprising spectrum, entirely different from that of normal water. The spectroscopic results were interpreted as evidence for a polymeric structure (hence the name polywater) with water molecules arranged in a network of hexagonal units. Subsequently, numerous theories on the structure of polywater filled the literature.

Figure 1. Cold-fusion chamber generated more excitement than energy. The surprising reports of successful cold-fusion experiments in 1989 can be interpreted as an instance of pathological science. Pathological science can be defined by three general conditions: the effect is nearly undetectable or statistically irreproducible; accepted theories are disregarded; and crucial experiments are neglected. B. Stanley Pons, Martin Fleischmann and Steven Jones entered the realm of pathological science in the pursuit of cold fusion. They believed that their experiments revealed a way to extract vast amounts of energy from a simple apparatus at low cost. The economic potential and theoretical excitement surrounding cold fusion forced the debate into the public media. While many publications and news programs touted cold fusion as the greatest discovery since fire, many scientists remained skeptical. This is a demonstration chamber in which the positive electrode is copper, rather than platinum as used in the experimental device. (Photograph courtesy of the University of Utah.)

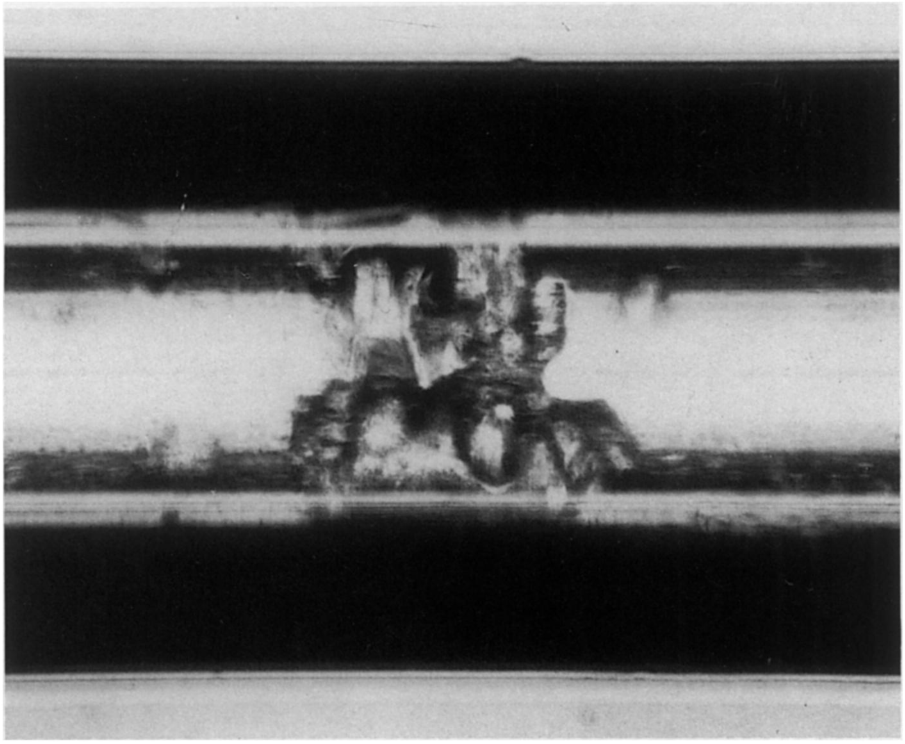


Figure 2. Polywater, allegedly a polymeric form of water, was the subject of a pathological-science episode in the 1960s. Polywater was discovered by investigators in the U.S.S.R., who described it as a substance with the consistency of petroleum jelly. They reported that it forms in capillary tubes, like the one shown here, and has properties different from those of normal water. Polywater freezes at –50 degrees Celsius and boils near 300 degrees Celsius. And, like petroleum jelly, polywater is denser and more viscous than normal water. (From Rousseau and Porto 1970. *Science* 167:1715–1719. Copyright 1970 by the AAAS.)

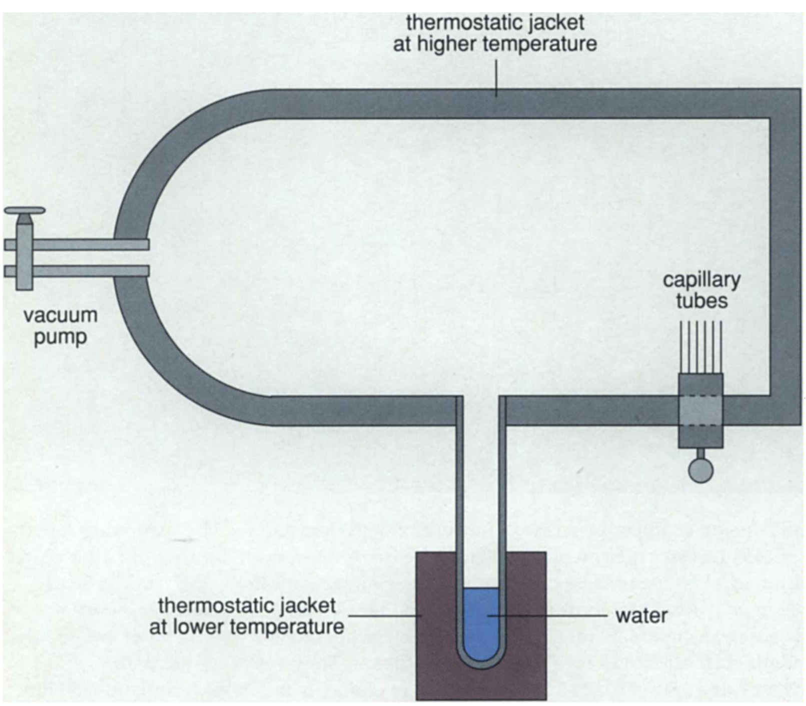


Figure 3. Polywater condenses inside freshly drawn capillary tubes. Water evaporated from a reservoir connected to the chamber nearly saturates the atmosphere with water vapor. In a few days, material condenses in the capillaries. Any normal water in the condensate evaporates. The remaining substance is polywater.

I began studying polywater as an associate of Sergio Porto of the University of Southern California. Porto reasoned that the environment holds many capillary-size pores; polywater could be formed naturally in these pores. This thought quickly swept us into speculation. Could polywater alter biological processes? We wondered if polywater could extend longevity, possibly being the long-awaited "fountain of youth."

The first experiments we planned on polywater were Raman-scattering measurements. This technique measures vibrational modes of the molecule, similar to an infrared spectrum. To obtain a Raman spectrum, a sample is irradiated by a laser, and the spectrum of the scattered light reveals the vibrational energies. As soon as we directed our laser on polywater, it turned into a black char! This was no polymer of water but more likely a carbonaceous material. We quickly abandoned our grandiose plans for exploiting polywater's immortal qualities.

By that time, many scientists had accepted polywater as real, even without a thorough chemical analysis. In a preliminary analysis of polywater, Porto and I found contamination by sodium. Years before, Derjaguin had given 25 samples of polywater to V. L. Talrose of the Institute of Physical Chemistry for mass spectroscopy. Talrose found substantial organic contamination—lipids and phospholipids in quantities comparable to the mass of the polywater. Still, Derjaguin argued that only those 25 samples were contaminated. The results of the analysis appeared in an obscure journal.

After my initial discoveries with Porto, I left for Bell Telephone Laboratories in the summer of 1969. At the time of my arrival, there was great excitement about polywater. Some of the managers of the laboratory invited me to a meeting to discuss an interesting question. Dielectric losses were increasing in some of the transatlantic telephone cables. Could it be, they wondered, that polywater had seeped into the cables and changed their properties? William Slichter, director of chemical research, quickly introduced me to the analytical chemists and gave the analysis of my samples the highest priority.

With the help of these chemists, I discovered many impurities in polywater—the specimens were up to 60 percent sodium, 15 percent chlorine and 15 percent sulfate, none of which appears

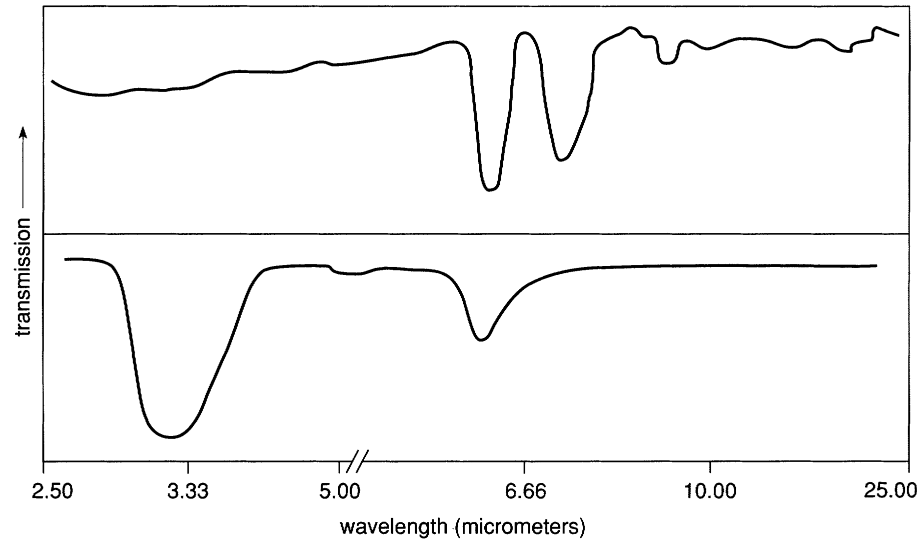


Figure 4. Polywater *(upper graph)* and normal water *(lower graph)* produce different infrared spectra. The geometry of a molecule dictates its infrared spectrum; materials with different structures have different spectra. Polywater's unusual infrared spectrum suggested an unusual structure, and this provided the strongest evidence that polywater was a new kind of water. (Data from the author.)

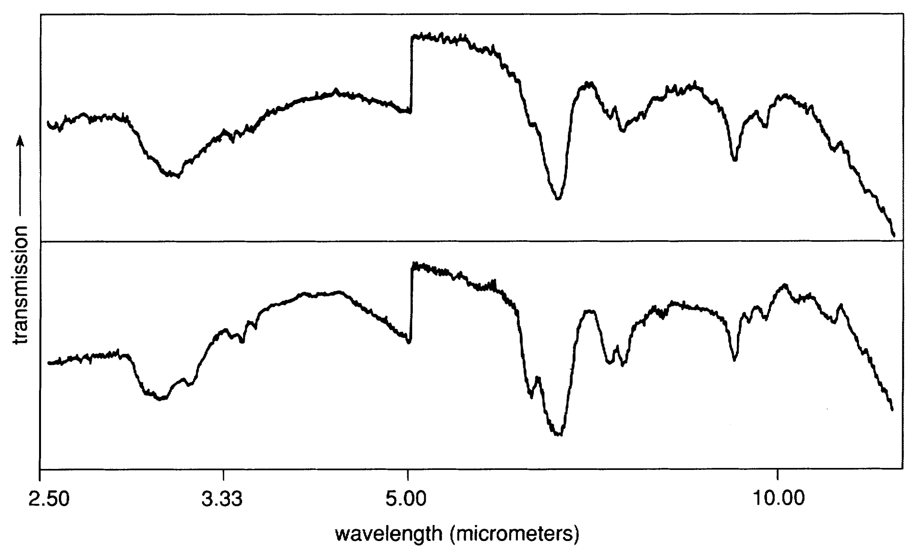


Figure 5. Polywater *(upper graph)* and human sweat *(lower graph)* have similar infrared spectra. A chemical analysis of polywater shows significant levels of lipids and phospholipids, common bio-organic substances. The similarity between the spectrum of polywater and the spectra of other bio-organic molecules, such as those in sweat, reveals that polywater is not water at all but a product of organic contamination in the capillary tubes. (From Rousseau 1971a.)

in normal water. The proponents of polywater accepted that *my* samples were contaminated. They claimed that their samples, however, were not. It became clear that a high concentration of contaminants would not be the silver stake in the heart of polywater.

The proponents argued that the unique infrared spectrum of polywater proved that it was a novel form of water. This spectrum allegedly originated from vibrational modes involving oxygen and hydrogen atoms. Accordingly, polywater prepared from heavy water (D_2O)—in which hydrogen is replaced by deuterium (a heavier isotope of hydrogen)—should produce different vibrations because of the different mass. I prepared polywater from heavy water and found that the infrared spectrum was identical to the spectrum of polywater prepared from normal water.

Determined to understand polywater's infrared spectrum, I turned to my athletic passion, handball. After a lively game, I returned to the laboratory with my sweaty T-shirt and wrung the perspiration into a flask. When I placed the sweat in an infrared spectrometer, the spectrum looked strikingly similar to that of polywater. The implication was obvious: that the contamination of polywater resulted from the condensation of bio-organic matter on the surface of the freshly drawn capillary tubes. With the publication of this discovery, nearly all research on polywater stopped.

Even without the evidence of chemical contamination, the proponents of polywater might have paused over a more fundamental, thermodynamic, dilemma—polywater was too easy to make. At about the same time Fedyakin reported his first observations of polywater, Kurt Vonnegut published his novel *Cat's Cradle*, in which a new form of water called ice-nine is discovered. Ice-nine has properties remarkably similar to those attributed to polywater. Vonnegut, however, saw the inescapable thermodynamic conclusion. At the end of the novel, all of the water in the world becomes ice-nine. Ironically, by some accounts the idea of ice-nine was originally suggested to Vonnegut by Irving Langmuir.

The polywater episode illustrates the loss of objectivity that can accompany the quest for great new discoveries. The quantities of polywater available were so small that many useful experiments could not be done. Many theories were put forward to describe the structure of polywater without even considering the thermodynamic difficulty of accounting for its very existence. Finally, definitive experiments showing high levels of contamination were done but not accepted, until overwhelming evidence showed that a new polymer of water had not been discovered.

In reflecting on the polywater saga, I am struck by the similarities between it and the more recent reports of infinite dilution and cold fusion. These discoveries, too, created great excitement in the scientific community.

Finite Illusions and the Inquisition

The physical principle of infinite dilution is simple. A biologically active solution is diluted so many times that no active molecules can be present, but the solution continues to produce a biological effect. This curious notion is the ba-

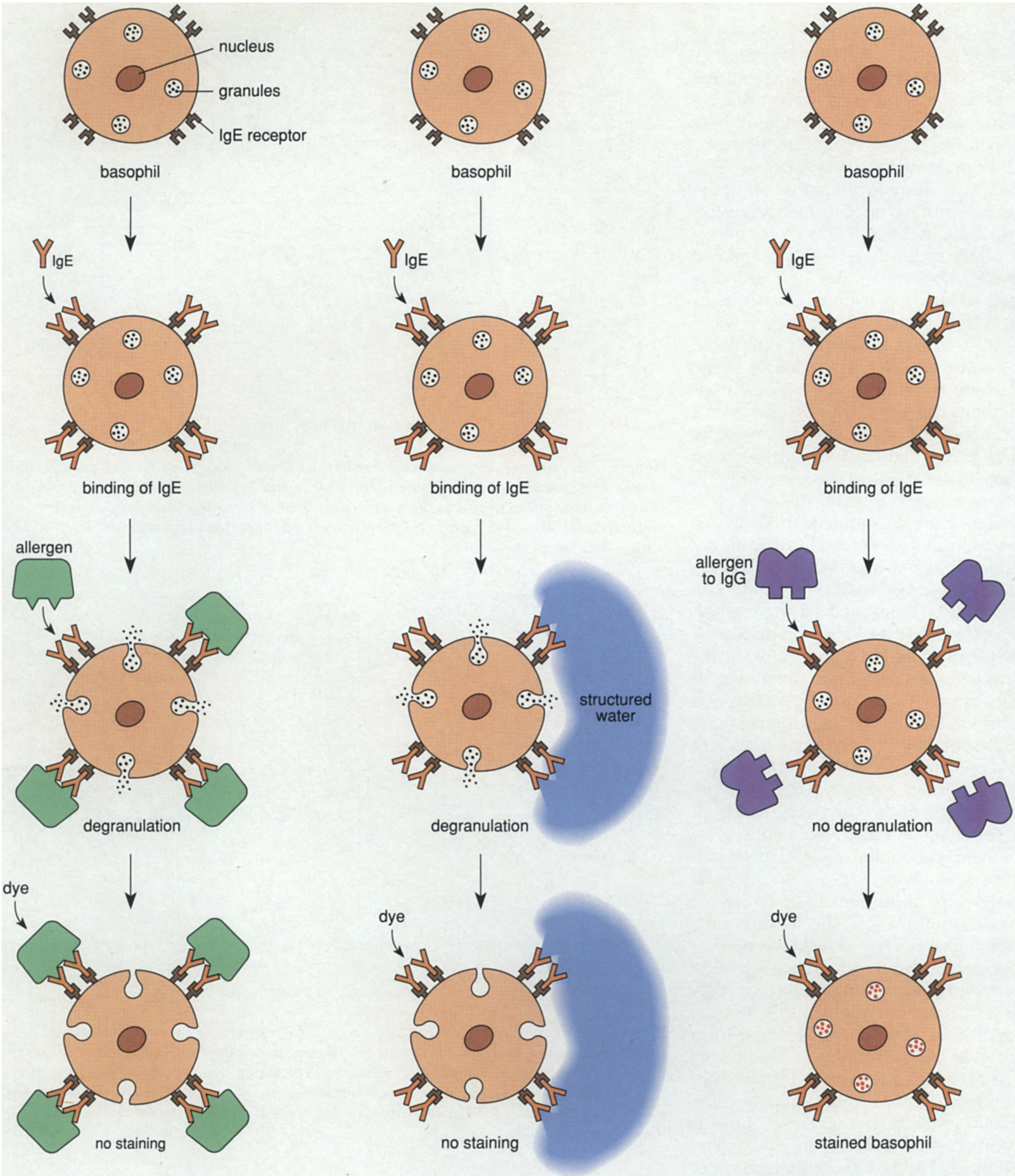


Figure 6. Infinite dilution, a technique associated with homeopathic medicine, had a brief appearance in the scientific literature in 1988. A group of French investigators reported that infinitely diluted allergens affect basophils, a type of white blood cell. In a standard experiment *(left)*, immunoglobulin E (IgE) binds to receptors on the surface of a basophil. When an allergen is added, it binds to the IgE and causes the basophil to release granules through exocytosis. After degranulation, toluidine blue (a dye) fails to stain basophils that lack granules. Jacques Benveniste of the University of Paris and his collaborators claimed that an infinitely diluted allergen (a solution containing essentially no molecules of the allergen) also induced degranulation of the basophils *(center)*. Benveniste and his co-workers suggested that the water in the infinitely diluted solution carried a "template" of the allergen which attached to the IgE on the basophil's surface. As a control experiment *(right)*, the allergen that binds to IgE was replaced by an allergen to IgG (a different immunoglobulin, not present in the experimental preparation). The allergen to IgG fails to induce degranulation, and so toluidine blue stains the basophils red. The French workers reasoned that if there were more red basophils in the control experiment than in the experiment that used the infinitely diluted allergen, then the infinitely diluted allergen to IgE had induced degranulation.

sis of homeopathic medicine—the belief that symptoms can be alleviated by a medication even when it is given in vanishingly small doses.

In 1988 Jacques Benveniste of the University of Paris and his collaborators reported a biological effect from an infinitely diluted solution. The controversy began when *Nature* published their paper after a two-year delay and followed it with a note that expressed reservations about the validity of the phenomenon. During the two-year delay, the editors of *Nature* had insisted that Benveniste have his experiments repeated by independent laboratories.

Benveniste's experiments involved human basophils, one type of white blood cell. Basophils hold many cytoplasmic granules that contain histamine and other substances that induce allergic reactions. High-affinity receptors for immunoglobulin E (IgE)—a class of antibodies that mediate some allergic reactions—cover the membrane of a basophil. When the complex formed by an allergen and an IgE molecule binds to one of these receptors, the basophil is induced to release granules via exocytosis, a process called degranulation. The dye toluidine blue stains intact basophils red; degranulated basophils do not absorb the dye, because it is the granules that become stained. Therefore, the degree of degranulation can be monitored by counting the number of red basophils after adding an allergen and IgE and comparing the results with a control sample in which degranulation has not occurred.

Benveniste and his colleagues wanted to measure the level of degranulation as the allergen was serially diluted. They diluted the allergen tenfold and then tested it on the basophils. Then, they took the diluted allergen and diluted it tenfold again. After performing this process of progressive dilution as many as 120 times, the experimenters still observed degranulation of the basophils. Benveniste and his colleagues estimated that this final dilution contained only 10^{-107} molecules of the allergen. The allergen was infinitely diluted—the probability was negligible that even one allergen molecule was in the solution applied to the basophils. With no allergen present, what caused degranulation? Benveniste and his colleagues proposed that the water acted as a template for the allergen and thereby carried the information even in the absence of the allergen.

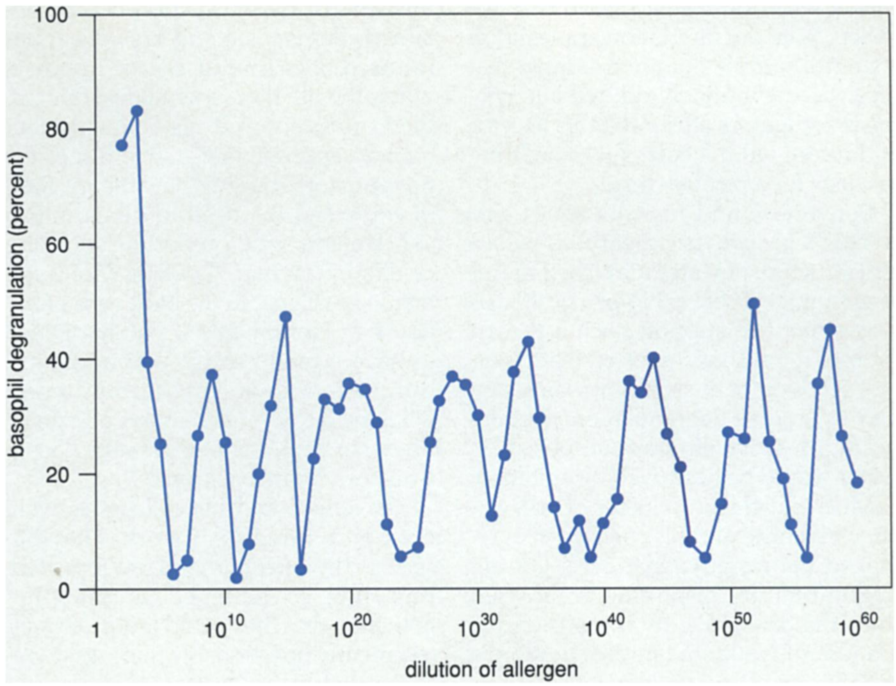


Figure 7. Degranulation varies periodically as the allergen is progressively diluted. After the first two dilutions, the allergen causes about 80 percent of the basophils to degranulate. With further dilutions of the allergen, the percentage of degranulation oscillates at around 20 percent. According to Benveniste and his collaborators, this periodicity is consistent and replicable. Later experiments reported considerable variation. (From Davenas et al. 1988.)

The level of degranulation varied in an odd manner with further dilutions of the allergen. A plot of the percentage of degranulation in basophils versus the logarithm of the dilution was roughly periodic. According to Benveniste, the periodicity was consistent and reproducible.

Benveniste's entire story failed to convince John Maddox, the editor of *Nature*. After publishing the original paper, Maddox created an investigative committee composed of himself, James Randi (a professional magician) and Walter Stewart (an experienced fraud investigator). After spending three weeks in Benveniste's laboratory, the committee discovered a few interesting facts about the experiments. The periodicity of degranulation was not consistent and varied from sample to sample; dye-marked basophils were difficult to count because basophils make up only 1 in 100 white blood cells; experiments simply failed to work for as long as several months in some cases; and one investigator, Elizabeth Davenas, was the best at making experiments *work*.

During the committee's time in Benveniste's laboratory, they saw some degranulation even when the allergen was highly diluted. But the committee wanted more proof. Finally, an elaborate series of double-blind experiments was initiated. These tests ensured that none of the investigators doing the cell counting knew which cells had received IgE and allergen and which ones had not. Indeed, Randi instituted an absurd procedure—wrapping the identifying code in aluminum foil, placing it in a sealed envelope and finally taping it to the ceiling. These experiments produced no degranulation when the allergen was highly diluted.

Maddox, Randi and Stewart concluded that unintentional bias had influenced the measurements. Analysis showed that duplicate readings of the same samples agreed more closely than statistically expected, except in the double-blind experiments. Therefore, the committee reported that the original experiments were poorly controlled and that no effort had been made to exclude systematic error or observer bias. There was no degranulation from the infinitely diluted allergen.

The infinite-dilution experiments had all of the characteristics of pathological science. The effect was weak and independent of the causative agent, the allergen. It was extremely difficult to count basophils and, yet, the experiments relied on visual measurements. When more degranulation appeared in a control sample than in a sample that received the infinitely diluted allergen, the investigators attributed this to error and recounted the basophils—building in a bias for a positive result.

Benveniste and his colleagues also created a bizarre new theory—a persistent structure of water mimicked the allergen in its absence. No physical basis was offered to support such a theory. Indeed, the authors reported that vigorous agitation of the solution was necessary to observe degranulation. Certainly, any putative structure imposed on water would be destroyed by agitation.

Much like the proponents of polywater, Benveniste and his colleagues overlooked the negative evidence. During degranulation, histamine is released from the basophils. By measuring the amount of histamine in the solution, a measure of degranulation can be obtained. Benveniste and his colleagues ignored this line of study because, when the allergen was highly diluted, they could not find histamine! Rather than accepting this negative result, the investigators sought a new theory. Randi expressed the need to do definitive experiments when reporting amazing results by saying: "Look, if I told you that I kept a goat in the back yard of my house in Florida, and if you happened to have a man nearby, you might ask him to look over my garden fence, when he'd say, 'That man keeps a goat.' But what would you do if I said, 'I keep a unicorn in my back yard'?"

Benveniste and his colleagues were not doing fraudulent work. They observed the effects that they reported. But they so believed in the phenomenon that they could ignore or reinterpret any questionable findings. In replying to the Maddox committee, Benveniste wrote, "It may be that all of us are wrong in good faith. This is no crime but science as usual and only the future knows." But, self-delusion is not science as usual.

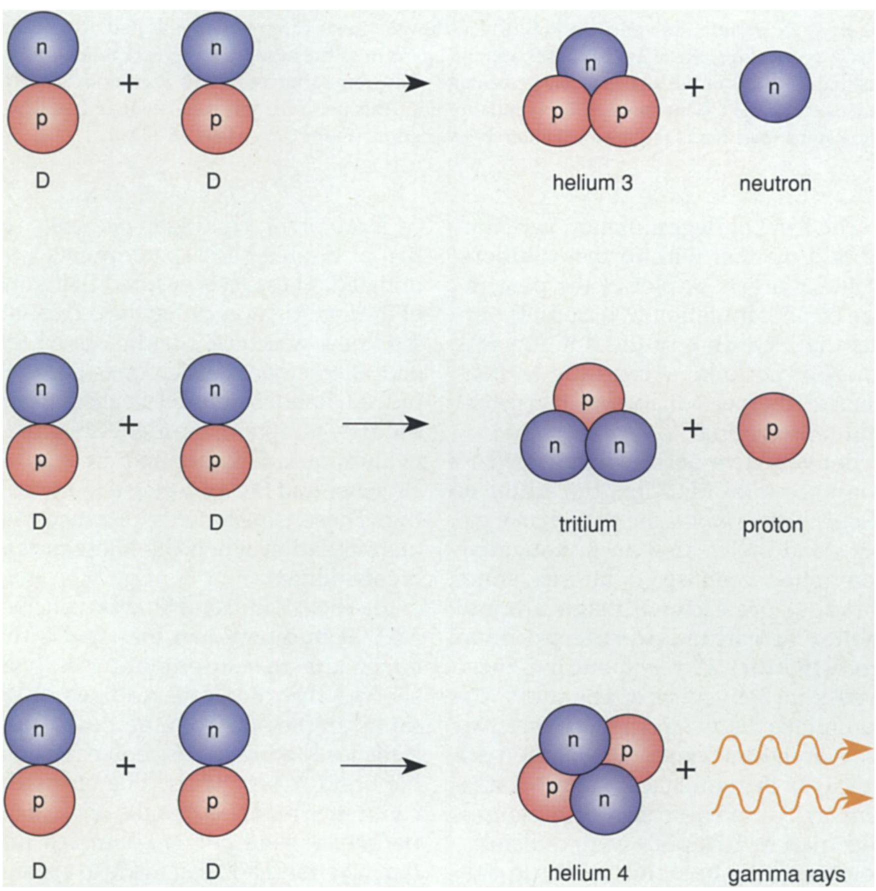


Figure 8. Fusion of deuterons to form larger nuclei is the process that Pons, Fleischmann and Jones believed they had observed in their cold-fusion experiments. A deuteron is a nucleus of deuterium, or hydrogen 2; each deuteron *(D)* consists of one proton *(p)* and one neutron *(n)*. When a pair of deuterons fuse, two outcomes are equally likely. The constituent particles can be rearranged to form a nucleus of helium 3, with the release of a neutron *(top)*. Alternatively, the particles can form a nucleus of tritium, or hydrogen 3, with the release of a proton *(middle)*. In a third reaction path *(bottom)* the product is helium 4, with only gamma rays being emitted to carry off excess energy, but this reaction is about 10 million times less likely than the other two.

Cold Confusion

A torrid controversy began in 1989 when two research groups in Utah discovered that they were independently working toward the same goal—cold nuclear fusion. Electrochemists B. Stanley Pons of the University of Utah and Martin Fleischmann of the University of Southampton in England directed one group, and physicist Steven Jones of Brigham Young University directed the other group. Cold fusion offered the potential for an inexpensive, inexhaustible and clean source of energy. Some scientists even called it the greatest discovery since fire. And yet the experiments and apparatus appeared so ordinary that they could be duplicated in your kitchen sink.

The principle of fusion is simple. It is the joining or *fusing* of two light nuclei (usually with a mass number below eight) to make one larger nucleus; energy is released in the process. One specific example of fusion is the joining of two deuterons. A deuteron is the nucleus of deuterium and consists of one proton and one neutron. If two deuterons are combined, they can produce an isotope of helium, namely helium 3 (with two protons and one neutron), plus an extra neutron. Less energy is required to hold together the two protons and one neutron of helium 3 than to hold together the two nuclei of the deuterons. The extra energy is released, producing the power of fusion. But a substantial problem, electrostatic repulsion, eliminates fusion as a present source of energy. Deuterons have a positive electric charge, and so they repel each other. To overcome this repulsion the deuterons must be heated to about 100 million degrees Celsius, about 10,000 times hotter than the surface of the sun. At this temperature deuterons collide violently enough to overcome the electrostatic repulsion. Cold fusion, however, requires only room temperature and no other extraordinary conditions.

Under thermonuclear-fusion conditions, two other fusion reactions can result from the combination of two deuterons. Two deuterons can combine to produce tritium (another isotope of hydrogen, with one proton and two neutrons) and a proton. Whether two deuterons combine to form helium 3 plus a neutron or tritium plus a proton is simply a matter of chance; both reactions are equally likely. Two deuterons can also fuse to form a nucleus of helium 4, the common isotope (with two protons and two neutrons) plus gamma rays. This result is about 10 million times less likely than the production of helium 3 or tritium.

Pons and Fleischmann collaborated on the study of complex processes in electrochemical cells for many years. They thought that, under the right conditions, the electrical forces acting at an electrode could squeeze atoms together and cause the nuclei to fuse. In 1985 they constructed an electrochemical chamber based on their ideas. During one experiment, Pons and Fleischmann claimed that an electrode became so hot that it melted right through the lab bench. Cold fusion had begun! Rather than risking premature release of their results, which would be necessary to get government funding, Pons and Fleischmann claimed to have spent $100,000 in personal funds for the next level of research.

Jones had also wondered about cold fusion for many years, but from a different perspective. He wanted to simulate conditions proposed to exist inside the earth. (One model suggests that the earth generates its heat through nuclear fusion.) He primarily monitored his experiments for neutron emissions, not heat.

The basic experiments were similar for the two groups. A simple electrochemical cell was constructed. The positive electrode (anode) was platinum and the negative electrode (cathode) was palladium, although Jones tried other metals. Palladium has long been known to absorb deuterium. Pons and Fleischmann filled their cell with a salt solution of lithium deuteroxide (LiOD) in heavy water. Jones used a solution, a Mother Earth soup, consisting of a mysterious mixture of salts—some concentrations were listed simply as "a very small amount"—also in heavy water. Both groups relied on a similar theory. When a current is passed into the cell, the heavy water splits into deuteroxyl ions (OD^-) that move to the anode and deuterons that are absorbed into the cathode. Some of the deuterons would be tightly packed into the lattice of the palladium electrode, where they might fuse.

Regardless of the experimental similarities, the two groups obtained radically different results. Pons, Fleischmann and Marvin Hawkins, a graduate student in the Department of Chemistry at the University of Utah, ran the cell for extended periods of time to load the palladium electrode with deuterium before detecting any production of heat, a possible indication of fusion. In the best case, they claimed that the thermal output exceeded the energy input by four and a half times. Jones detected no heat; but, he claimed that fusion began as soon as one hour into the experiment and that it decreased after eight hours. Jones detected neutron emissions that were a million times smaller than those estimated by Pons, Fleischmann and Hawkins. However, the neutron emission that Jones reported was about 40 orders of magnitude greater than the predicted background.

In September 1988, two years after building his first fusion cell, Jones—unaware of the work being done by Pons and Fleischmann—received a research proposal to review for the Department of Energy. The proposal came from Pons and Fleischmann and concerned cold fusion via an electrochemical cell. Jones wanted more information to assess the proposal. In January 1989 the two groups met, and there was no indication that they would collaborate. Furthermore, Jones revealed that he had hastily submitted an abstract on cold fusion to the American Physical Society to be presented at their meeting in May. Although Pons and Fleischmann wanted more data before publishing, they also wanted the prestige of being the first to publish results on cold fusion.

The two groups failed to resolve the dilemma of when and how the results should be published. Finally, the two universities attempted to arbitrate between the research groups. At a meeting on March 6, both groups agreed to mail their papers simultaneously to *Nature* on March 24, even agreeing to meet at the Federal Express office. Pons, Fleischmann and Hawkins, however, welshed on the deal. They mailed a preliminary paper to the *Journal of Electroanalytical Chemistry* on March 11 and held a news conference on March 23 to announce their findings. Having heard about the press conference, Jones sent his paper by telefax to *Nature* one day early.

The *Journal of Electroanalytical Chemistry* pushed their schedule ahead to publish the article by Pons, Fleischmann and Hawkins as quickly as possible. And in this race to the press, accuracy suffered. After publishing the original article, the journal published two pages of errata—19 changes in all. One change rectified "the inadvertent omission" of Marvin Hawkins as a coauthor.

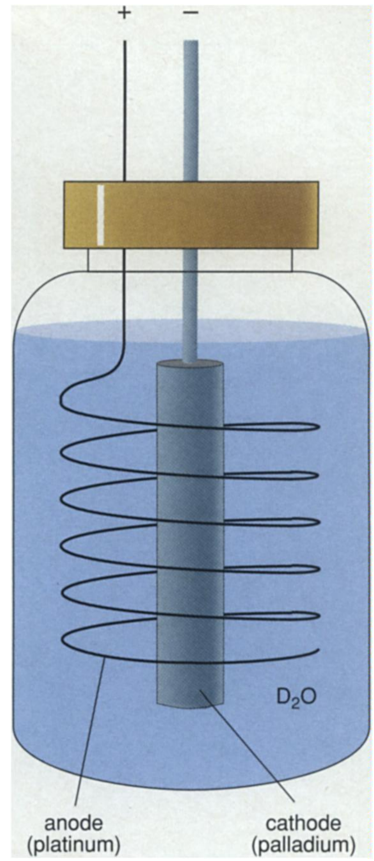
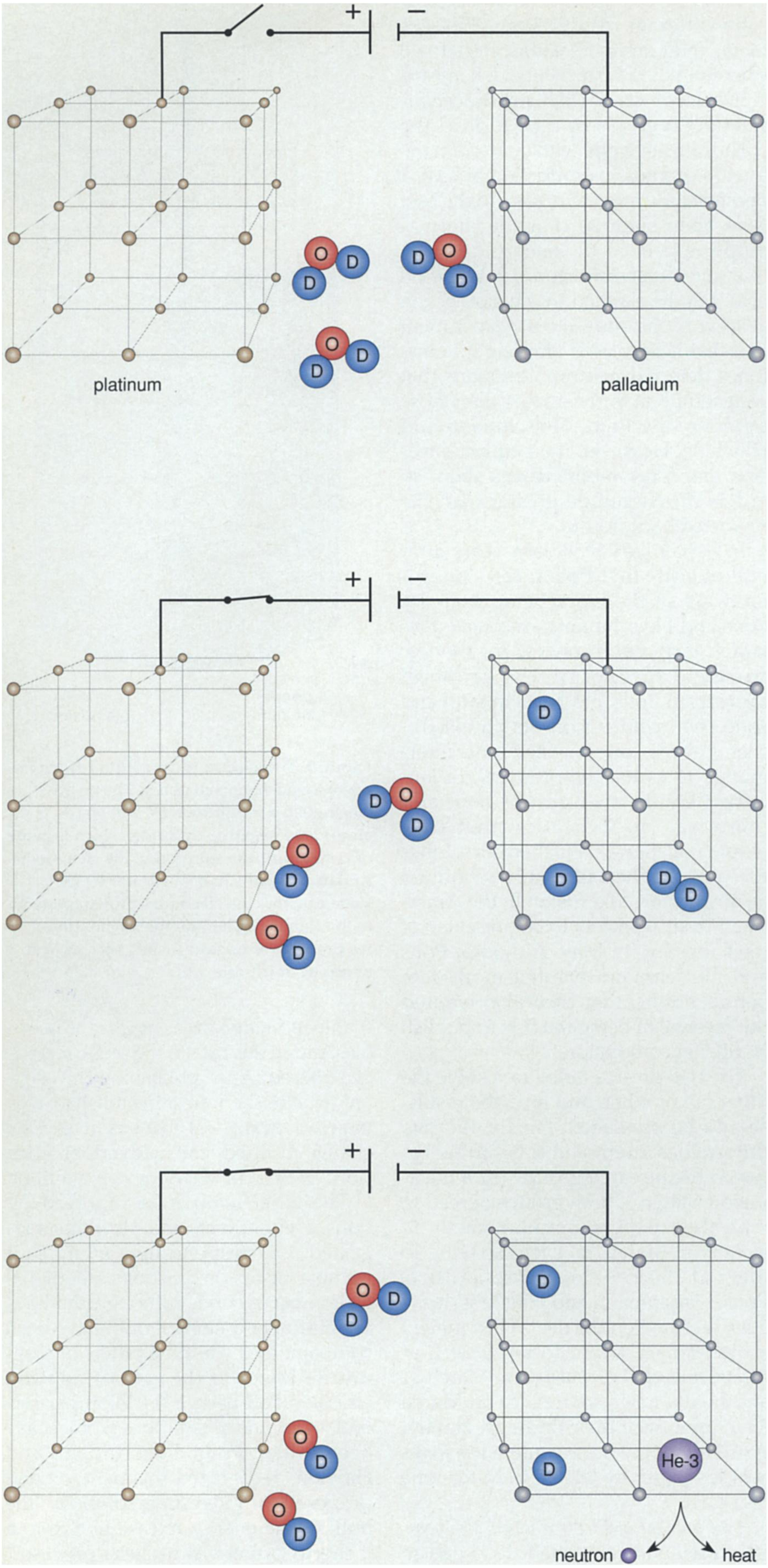


Figure 9. Two electrodes in a liquid-filled jar make a cold-fusion chamber. The positive electrode is a platinum wire that wraps around the negative electrode, which is made of palladium. The solution in the chamber is a mixture of salts in heavy water (D_2O). Voltage across the electrodes provides energy to the chamber. The chamber is monitored for products of nuclear fusion, such as heat, neutrons or tritium.

The significant heat reported by Pons, Fleischmann and Hawkins suggested a correspondingly significant number of fusion reactions, about 100 trillion per second. Although the level of neutron emissions should have been comparable, it was nine orders of magnitude lower. If the rate of neutron emission had been 100 trillion per second, everyone in the room would have been killed. Pons, Fleischmann and Hawkins recognized this paradox and proposed an interesting solution: "the bulk of the energy release is due to a hitherto unknown nuclear process." When other laboratories tried to repli-



cate the production of heat, however, it was never consistently found.

Then other investigators wondered if *any* neutrons had been emitted. Pons, Fleischmann and Hawkins based their claim of neutron emission on the gamma-ray emission spectrum. They proposed that this emission spectrum arose from neutrons being captured by the surrounding water bath. Careful analysis, however, showed that the reported spectrum was a factor of two more narrow than the resolution of their detector. Furthermore, the spectral line associated with neutron emission was not at the expected energy level. Many other laboratories repeated these experiments and found a definitive answer: No neutrons beyond the background level could be detected from electrochemical cells. These findings discredited the work of Pons, Fleischmann and Hawkins as well as that of Jones, whose only evidence for fusion was neutron emission.

Still, Pons, Fleischmann and Hawkins reported finding tritium, a potential product from fusion. Other laboratories also claimed to have found tritium at relatively high levels. This was the strongest evidence for cold fusion. But by June 1990 tritium was shown to be a contaminant in the palladium electrode. There was no evidence for the production of fusion products.

Cold fusion was doomed from the start when a race to be first took precedence over the desire to be right. Most measurements reporting nuclear effects from cold fusion were barely above the background noise, and extended periods of failed experiments afflicted even Pons's laboratory. The proponents of cold fusion attributed the failure to several causes: differences in the materials, the size of the electrodes, impurities in the electrodes, and low current density. The list goes on.

Nuclear reactions, however, are very well understood. Any theory offered to

Figure 10. Close packing of deuterons within the atomic matrix of metallic palladium is the hypothetical mechanism of cold fusion. Within the apparatus molecules of heavy water surround the platinum and palladium electrodes *(top)*. When voltage is applied to the electrodes, molecules of heavy water split into deuteroxyl ions that move to the positive electrode and deuterons that move to the negative electrode *(middle)*. Deuterons accumulate in the spaces between the palladium atoms of the negative electrode. If two deuterons are tightly packed in the palladium electrode, they may overcome their electrostatic repulsion and fuse *(bottom)*.

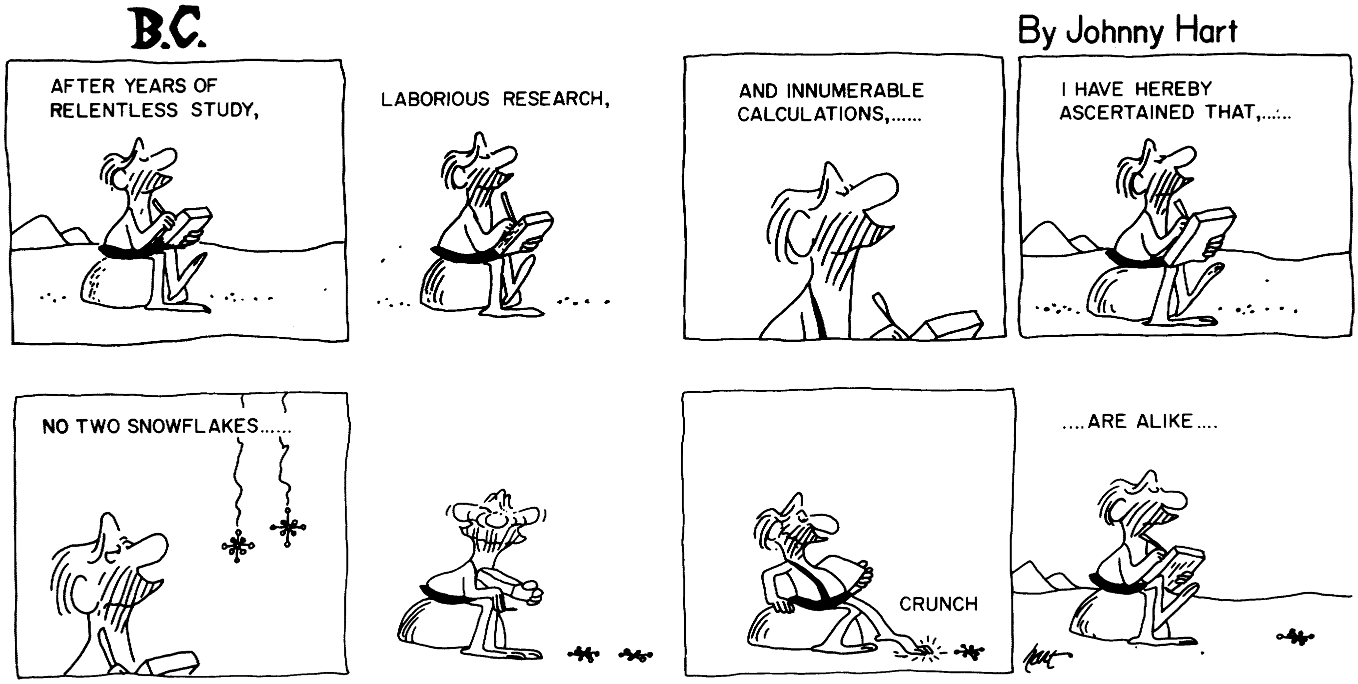


Figure 11. Cartoon by Johnny Hart portrays extremely pathological science. Here, the scientist more than overlooks negative evidence; he destroys it. (Reprinted with permission from Johnny Hart and Creator's Syndicate, Incorporated.)

account for the reported observations must postulate new nuclear processes that only occur in the palladium electrodes. Indeed, Edward Teller has proposed that there may be an undiscovered neutral particle involved!

The investigators of cold fusion also ignored definitive experiments. Pons, Fleischmann and Hawkins only examined their work in cells containing D_2O. An obvious control replaces D_2O with H_2O, which would prevent any nuclear reactions. Likewise, no one showed that fusion products are formed at the same time as the heat. Michael L. Salamon of the physics department at the University of Utah and an army of colleagues spent more than five weeks in Pons's laboratory attempting these measurements. No fusion products were detected when heat was produced, or when it was not. Salamon later said that they did not see a "peep" or an "iota" of conventional fusion products. Pons, however, rejected this negative result and said, "Maybe they should have been searching harder for nuclear particles instead of peeps and iotas."

There are many examples of scientific projects in which objectivity was lost. Self-delusion in scientific research was recognized centuries ago, is evident today and will no doubt continue into the future. Polywater, infinite dilution and cold fusion received a great deal of attention simply because they were scientifically and technologically very important. In each of these examples, the investigators could have avoided the trap of nonobjectivity by doing the definitive experiments—those experiments that give a decisive answer. Definitive experiments existed for polywater, infinite dilution and cold fusion; but those experiments were either not done or not accepted when they were done. The ability to define, carry out and accept definitive experiments is the responsibility of every scientist, a responsibility that must be fulfilled at all costs.

Bibliography

Cohen, James S., and John D. Davies. 1989. The cold fusion family. *Nature* 338:705–707.

Davenas, E., F. Beauvais, J. Amara, M. Oberbaum, B. Robinzon, A. Miadonna, A. Tedeschi, B. Pomeranz, P. Fortner, P. Belon, J. Sanite-Laudy, B. Poitevin and Jacques Benveniste. 1988. Human basophil degranulation triggered by very dilute antiserum against IgE. *Nature* 330:816–818.

Derjaguin, Boris V., and N. V. Churaev. 1973. Nature of "anomalous water." *Nature* 244:430–431.

Fleischmann, Martin, and Stanley Pons. 1989. Electrochemically induced nuclear fusion of deuterium. *Journal of Electroanalytical Chemistry* 261:301–308.

Hall, Robert N. 1989. Pathological science. *Physics Today* 42:36–48.

Jones, Steven E., E. Paul Palmer, J. Burt Czirr, Daniel L. Decker, Gary L. Jensen,James M. Thorne, Stuart F. Taylor and Johann Rafelski. 1989. Observation of cold nuclear fusion in condensed matter. *Nature* 338:737–740.

Lippincott, Ellis R., Robert R. Stromberg, Warren H. Grant and Gerald L. Cessac. 1969. Polywater. Vibrational spectra indicate unique stable polymeric structure. *Science* 164:1482–1487.

Maddox, John, James Randi and Walter W. Stewart. 1988. "High-dilution" experiments a delusion. *Nature* 334:287–290.

Rousseau, Denis L. 1971a. An alternative explanation for polywater. *Journal of Colloid and Interface Science* 36(4):434–442.

Rousseau, Denis L. 1971b. "Polywater" and sweat: similarities between the infrared spectra. *Science* 171:170–172.

Rousseau, Denis L., and Sergio P. S. Porto. 1970. Polywater: polymer or artifact? *Science* 167:1715–1719.

Salamon, Michael H., M. E. Wrenn, H. E. Bergeson, K. C. Crawford, W. H. Delaney, C. L. Henderson, Y. Q. Li, J. A. Rusho, G. M. Sandquist and S.M. Seltzer. Limits on the emission of neutrons, gamma-rays, electrons and protons from Pons/Fleischmann electrolytic cells. *Nature* 344:401–405.

Schriber, James E., Michael A. Butler, D. S. Ginley and Ronald I. Ewing. 1989. Search for cold fusion in high-pressure D_2-loaded titanium and palladium metal and deuteride. *Fusion Technology* 16:397–400.

Williams, D. E., D. J. S. Findlay, D. H. Craston, M. R. Sené, M. Bailey, S. Croft, B. W. Hooton, C. P. Jones, A. R. J. Kucernak, J. A. Mason and R. I. Taylor. Upper bounds on 'cold fusion' in electrolytic cells. *Nature* 342:375–384.