idence for nuclear products. The American newspapers and journals refused to report the evidence. Their science reporters did not demand some response from the physics community: you demanded a nuclear product, you demanded helium, and now you have it, what do you say? The ghetto's wall of silence was strictly maintained.

Failed Experiments

What ought to be made of the many failed attempts to generate excess heat energy? These experiments create no excess heat, no neutrons, nor any other nuclear product. Many scientists take pains that their experiments are designed according to the best information available. They are carried out exactly as reported in their published papers so far as is known. Did such experiments demonstrate that there is no "cold fusion" phenomena of interest? Do those failed experiments invalidate the remaining claims of Fleischmann and Pons? How is their significance to be weighed?

A large fraction of the unsuccessful experiments were run by scientists who also had run successful experiments. These failed experiments were reported by scientists who had measured anomalous power with their own hands. They had developed confidence in their techniques, and they accepted anomalous power generation by some of their cells as a real phenomenon. They concluded that there was some additional agent (a variable or parameter) in the failed experiment that was not under control. The failed experiment may be of some value to the experimenter who can review its design.

Many experimenters never saw a positive result, even after intense effort. These included some of the most prestigious institutions such as MIT, Yale (at Brookhaven*), Caltech, and Harwell (England). Does the caliber of scientists and resources that such institutions can bring to bear on a task imply that their failed results are the correct results?

In his *The American Scholar* article, David Goodstein is clearly speaking to the orthodox scientist only, not to the cold fusion scientist. He invokes the failed experiment syndrome by raising the specter of Sir Karl R. Popper, the late Austrian philosopher of science. Goodstein speaks about the significance of the failed experiment.

> Science in the twentieth century has been much influenced by the ideas of Karl Popper, the Austrian philosopher. Popper argues that a scientific idea can never be proven true, because no matter how

* The Brookhaven National Laboratory on Long Island in New York is a large nuclear research facility that works cooperatively with many eastern universities.

> many observations seem to agree with it, it may still be wrong. On the other hand, a single contrary experiment can prove a theory forever false. Therefore, science advances only by demonstrating that theories are false, so that they must be replaced by better ones. The proponents of Cold Fusion took exactly the opposite tack: many experiments, including their own, failed to yield the expected results. These were irrelevant, they argued, incompletely done, or lacking some crucial (perhaps unknown) ingredient needed to make the thing work. Instead, all positive results, the appearance of excess heat, or a few neutrons, proved the phenomenon was real. This anti-Popperian flavor of Cold Fusion played no small role in its downfall . . .[18]

Popper assumes, as one might expect of a philosopher, that the experiment is defined with infinitely detailed rigor. Otherwise, the ". . . single contrary experiment . . ." becomes merely a *different* experiment, proving nothing.

To be more precise, Popper's argument is about experiments disproving theories, not other experiments. Moreover, the claims of cold fusion are concerned with experimental observation, not theory. Orthodox nuclear theory is of concern here because it apparently does not provide an energy source for the anomalous power claimed. If the power measurements are right, then presumably that theory must suffer some degree of amendment. This argument fits the Popper shoe to the physicist's foot, so to speak. One, and only one, cold fusion experiment that went contrary to a theory of orthodox physics would be sufficient to prove that theory false forever.

The surface-catalyzed electrochemical reaction is a complicated one. No assurance is available that only one type of experiment is involved in the "cold fusion" episode. In fact, the plethora of results implies that a variety of experiments are involved. This is why some produce heat and others do not. Which reaction type is active at any moment depends upon the precise condition of the cathode surface or the presence of particular impurities in the electrolyte or the palladium.

It is not possible, of course, to prove the Fleischmann and Pons effect wrong by performing the experiment and getting a failed result. The cold fusion experiment has not leant itself to Popper's kind of analysis. Unfortunately, progress will have to be made without his help.

The counting of failed experiments was not useful if the relationship of the experiment's input to output was effected by some threshold effect such as loading. Below the threshold value (0.85–0.90), the experiment's output was always zero. Above the threshold value, it was possible for the experiment to succeed.[19]

In Figure 8.1, the average value of curve (A) may or may not have a useful

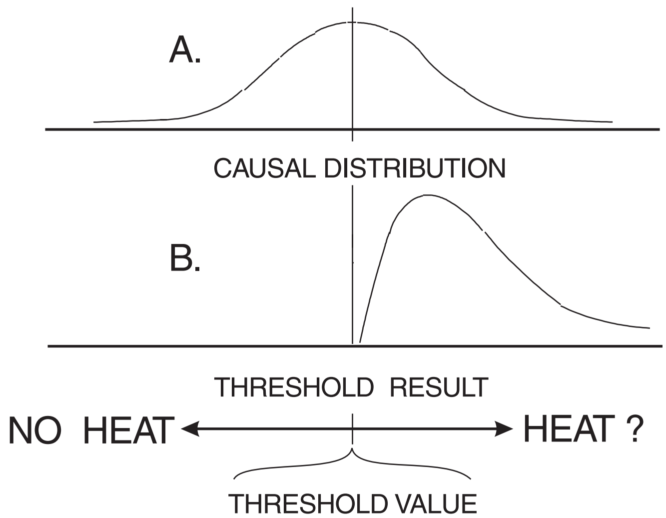

FIGURE 8.1 Illustration of the threshold effect on an otherwise smooth probability distribution curve.

meaning. If the experiment responded with a distribution like that of curve (B), then the average value was misleading and its use will invite error. In the case of curve B, the statistical distribution of failures and successes is not helpful. One example of this appeared in the summer of 1991, when McKubre suggested that the loading of deuterium into the palladium must exceed 0.90 (D/Pd ratio) value or the experiment will always fail.

A constructive way of looking at the failed cold fusion cells is as follows. Imagine you have a lake before you, and you want to know if there are fish in it. You send out one hundred expert fishermen to fish. When they return, ninety-five of the fishermen have caught no fish, and five fishermen are each holding up a fish. Now your question can be answered.

The political answer to the question is to take a vote, as the APS did for its third press conference on May 2, 1989, at Baltimore. Clearly, if you do that, the "no fish" have it: a ninety-five to five vote says that the lake does not contain fish. Or, one can use scientific reasoning and argue that the five fish caught indicate that the lake does contain fish. It may also be argued, of course, that the five fish were the result of fraud or incompetence, e.g., that the five caught actually were smuggled in tackle boxes or that they were eels, not fish. In any event, the claim that fish were caught must remain at the center of the argument. Counting the ninety-five empty returns is of no help. Similarly, counting the failed cold fusion experiments is of no diagnostic value.

SUMMATION
The Place of Failed Experiments

A threshold effect means that an indefinitely large number of failed experiments could be expected if the experiment were operated at values below that threshold.

Above the threshold, the statistical distributions of materials variety were not known. They might have included additional threshold effects. It could not be assumed that those statistics were smooth valued; they might have been piecewise continuous, or discontinuous.

There was one important conclusion that could be drawn from the discussion of the failed experiments. The count of the number of failed experiments carried no diagnostic value.

There is profit in discussing why some experiments failed. The presence of certain experimental impurities may have had a positive effect, whereas other impurities were definitely inhibiting. During 1989, palladium was obtained largely from supplies on distributors' shelves. It was not manufactured with cold fusion experiments in mind. Some of it worked, fortunately, or Fleischmann and Pons would have seen nothing and then would have abandoned their research. Much of it did not work, but for reasons that were not understood. Five years later, there were companies that specialized in supplying palladium more or less suitable for cold fusion research.

Since the metallurgy of palladium seemed to be so important, I asked Fleischmann why he did not establish a palladium metallurgical operation as a part of his operation in France. He responded:

> I can understand the wish to [establish a metallurgical facility], but in the first phase of the work you are better off [working] with the experts [who are employed at the palladium vendor's facility] . . . People think that making palladium is easy; it is very difficult to make palladium satisfactorily. You have to control the oxygen partial pressure, the annealing history, the drawing history, the swaging history, the rolling history. [At the beginning] you don't know what you want to do. We have focused in on the rods, but maybe you want to use wire? Maybe go on to [using] mesh? [At one point] we were working with palladium-cerium electrodes, and those damn things had to be made with electron-beam furnaces. Holy Moses, you could be there forever.
>
> You can spend the money. You are doing it in the end [through the vendor], but it is premature [to try to do it in-house].[20]

There was the question of purity. The semiconductor industry finally required silicon that was 99.999999999% pure. Experimenters in cold fusion by 1995 had available purity of 99.98% relative to some specific impurities. No one knows if improving the absolute purity will improve the experiment's replication.

One scientist looked at the list of impurities (that is provided with each delivery of palladium from the vendors). He calculated that the heat could have come from any one of several impurities if they were consumed in some as yet unknown nuclear reaction. In that scenario, the performance of a successful cell would depend on the presence of that impurity.*

There is nothing about these difficulties that is foreign to science, to chemistry, or to the specialty of electrochemistry. Good laboratory practice requires the exact preparation of each item that goes into the cell in excruciating detail. The cold fusion cell is no exception.

A Threshold

What about those 1989 claims that Fleischmann and Pons forgot to stir the pot? Their two seminal papers of July 1990 and 1992 reported uniform temperatures to ± 0.01C measured within their cells. R. H. Wilson agreed that mechanical stirring was not necessary in the Fleischmann and Pons cell (see Chapter 9, page 117).

The loading of deuterium into the palladium cathode (D/Pd) achieved at Caltech was ". . . 0.77, 0.79 and 0.80 . . ."[21] These values are shown in Figure 8.2 by the (A) arrow.† Published in 1995,[22] this graph depicts the propensity of a palladium cathode to generate excess heat as the loading ratio increases from 0.2 to 0.8 D/Pd. This writer's vertical lines show the region where Lewis operated his cells. To generate excess heat, the tracing must be in the positive region. If Lewis had built a thousand cells that only loaded to this extent, none would have generated excess heat.

It would be a mistake to assume that the D/Pd ratio was the only threshold to effect experimental results. There was a distinct onset of excess heat reports when the current through a cathode exceeded a certain value. Below that value, the phenomenon was not observed. This threshold was not as sharply

* Courtesy of David J. Nagel.

† Fleischmann referred to this figure as, "The variation of the relative partial molar enthalpy of hydrogen in palladium as a function of the charging ratio." Those who take a special interest in this graph should move immediately to the referenced paper, as I have taken some liberties to simplify the figure and its explanation.

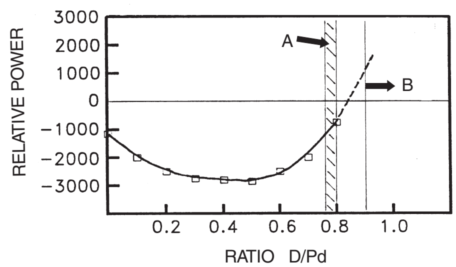

FIGURE 8.2 Fleischmann reported that as the palladium cathode stored more deuterium, its ability to generate anomalous power changed from negative to positive.

defined as was the loading threshold. It was shown by some experimenters to occur at about 100 milliamperes per cm^2 of cathode current.[23]

The most damning fact about cold fusion research in the eyes of its critics was the lack of repeatability of the Fleischmann and Pons cell: there were so many failed experiments. The failure, however, was not so much in the failed cells as in the obtuse conclusions drawn from them. A study of the first years of the cold fusion saga might persuade a serene observer that the effect of a threshold in an experiment was something new for science.