EXCESS HEAT &

Why Cold Fusion Research Prevailed

SECOND EDITION

Charles G. Beaudette

Foreword by
Sir Arthur C. Clarke, CBE
Author of *2001: A Space Odyssey*

Introduction by David J. Nagel, Ph.D.
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This book is dedicated
to my wife

Kate

without whom
it would not have been possible.

An investigative report prepared for the general reader to explain how the most extraordinary claim made in the basic sciences during the twentieth century was mistakenly dismissed through errors of scientific protocol.

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Foreword

In March 1989, two respected chemists, Drs. Martin Fleischmann and Stanley Pons, claimed to have achieved nuclear fusion at room temperature in certain metals saturated with deuterium, the heavy isotope of hydrogen. Under these conditions, they reported that they were obtaining more energy than they had put into the system.

Naturally, this claim caused a worldwide sensation, and many laboratories tried to repeat the experiment. Almost all reported failure, and Pons and Fleischmann were laughed out of court. That was the last anyone heard of them for several years.

From the mid-1990s however, there was an underground movement of scientists who believed that these claims should be looked into more seriously, and started experiments of their own—often in defiance of their employers. There have now been several international conferences on so-called cold fusion—derided by skeptics as congregations of deluded disciples worshiping a false religion. Some of their criticism is very valid: if Drs Fleischmann and Pons had indeed produced nuclear fusion, they should have been dead! For where are the neutrons and gamma-rays and tritium and helium—the lethal "ashes" such a reaction should produce? Well, later experiments claim to have detected them, but in quantities far too small to account for the energy liberated. A theoretical basis for cold fusion is therefore still a mystery—as was the energy produced by radioactivity and uranium fission—when they were first discovered. I am tempted to say, "It's not fusion as we know it, Jim." Lukewarm fission perhaps?

To complicate matters still further, there are several reports of excess ("over unity") energy that apparently can have nothing to do with nuclear reactions. Some involve systems of magnets, which appear suspiciously like the "perpetual motion" devices that have obsessed generations of inventors. More convincing are machines—several now being manufactured on a commercial scale—that depend upon liquids under extreme conditions, where it is known that the phenomenon of micro-cavitation can produce million-degree bubbles.

Whatever the final verdict on this whole affair—and despite all claims to the contrary the jury is still out—it is almost certainly the biggest scandal in the history of science. Charles Beaudette, an MIT graduate with thirty years of engineering experience, has done a remarkable job in untangling and documenting the whole story of cold fusion. *Excess Heat* is not only a superb record of an extraordinary episode, but is also highly entertaining. The author does not hesitate to apportion blame where it is deserved—and there is enough to go around to satisfy everyone.

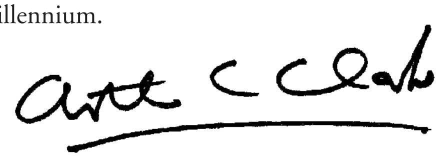
I do not believe any unbiased reader will put down this book without feeling that *something* strange is happening at the fringes of physics. Although skeptics are still fond of intoning "pathological science" like a mantram, the wisest approach must surely be "wait and see."

Perhaps the most disappointing outcome would be if cold fusion turns out to be merely a laboratory curiosity, of some theoretical interest but of no practical importance. But this seems unlikely: anything so novel would indicate a major breakthrough. The energy produced by the first uranium fission experiments was trivial but everyone with any imagination knew what it would lead to.

Of course, the most exciting possibility will be if these anomalous energy results can be scaled up. That could terminate the era of fossil fuels, end worries about pollution, and change the geopolitical structure of the world out of recognition.

In 1973, when OPEC started to multiply the price of oil, I rashly predicted: "The age of cheap power is over—the age of *free* power is still fifty years ahead."

Excess Heat strengthens my hope that this may be not too far from the truth, early in the new millennium.



Arthur C. Clarke
Colombo, Sri Lanka
Op 995
4 January 2000

Introduction

The topic called cold fusion has been dismissed, often derisively, by most scientists and the general population as wrong, a good example of bad science. The terms “pathological science” or “voodoo science” frequently follow mention of the subject. *Excess Heat* deftly makes the case, in fashion reminiscent of a legal brief, for serious attention to the subject. This book concludes that there is no basis now for dismissing cold fusion. Each of the major reasons that are offered for ignoring, or actively opposing, further research are shown to be flawed. The persistent lack of a theoretical explanation and problems with experimental reproducibility are major legitimate concerns, but they are not reasons to dismiss the topic.

The fact that cold fusion is without a satisfactory explanation at present merely ranks it with other topics in science which await understanding. Most of the time, the discoverers of a new scientific effect are able to explain its origin, many times in the initial report. However, the history of science has several famous examples for which decades passed between an observation and its elucidation or between development of an idea and its substantiation. Superconductivity refers to the lossless and persistent circulation of electrical current in some materials at low temperatures. It was discovered experimentally in 1911 by Onnes, but not understood until development of the correct quantum mechanical theory by Bardeen, Cooper and Schrieffer in 1957. Plate tectonics is the name given to the slow (few centimeters per year) relative motion of major sections of the earth’s crust. The idea was put forward in 1912 by Wegner, who was a meteorologist. However, the theory was not generally accepted until the 1960s, when sea floor spreading and earthquake data made clear the existence and motion of crustal plates. Einstein postulated stimulated emission in 1925, but the maser was not demonstrated by Townes until 1954. Other cases could be cited in which many years elapse between a laboratory discovery and its explanation, or between an idea and its validation. The current lack of an explanation, and the decade that has passed since the announcement of cold fusion, are not reasons to ignore it. Many of us wonder how long, indeed, will it take to understand this particular scientific mystery.

The lack of reproducibility in cold fusion experiments leads some people to believe that there is something wrong with the reported results. However, this argument bothers many others. Again, there are instances in science in which an effect was once dismissed, but turned out to be correct, reproducible, controllable, and even useful. Early experiments are often erratic and hard to reproduce for various reasons. Understanding which variables exert important effects and which can be ignored, is difficult in complex situations. This is especially true where the measured effects are near threshold, a common situation in some new fields. Confusion over the critical variables is even germane to the case where the effects seen are large, as is actually the case for many cold fusion experiments. If it were easy to control an experiment and make the associated measurements, it is likely that a discovery would already have been made, or else would have to wait for the appropriate instrumentation to be developed.

Further points about reproducibility in science are relevant. Certainly, recipe-style reproducibility is desirable, even in the absence of partial or full understanding. This has been the case for many years in the area of "high temperature" superconductivity in certain classes of complex ceramic materials. However, some degree of reproducibility can exist even in the absence of a reliable formulation. It is not uncommon for an experiment to "work" only a fraction of the time when the relevant variables are either not recognized or not controlled with sufficient sensitivity. This was the case in the early days of silicon crystal growth for electronic devices. Then, sodium was a critical—but not adequately controlled—impurity, and it often killed the desired semiconductor effects. Impurities at the parts per billion level, rarely analyzed for in cold fusion experiments, might account for the familiar experimental problems in the field. Experiments in many laboratories have shown that proper materials are critical to attainment of unusual cold fusion results. This book shows that there are numerous cases in the published literature on cold fusion where a significant fraction of the experimental trials in a particular laboratory yielded anomalous results.

Reproducibility from laboratory to laboratory is certainly one of the desirable characteristics of a scientific experiment. But, once again, its absence does not render a reported observation wrong. In the case of cold fusion, there have been several attempts to reproduce experiments done first in one laboratory which have indeed yielded large anomalous results in another laboratory. Some have already been reported and others soon will be properly documented. By now, the cold fusion reproducibility question can itself be seriously questioned.

The situation that currently exists can be summarized as follows. A few dozen competent scientists performed some similar and some very different experiments, with proper equipment, careful calibrations, controls and other thoughtful procedures, and observed anomalous effects that are many times the noise floor of their experiments. Details of these experiments and their results are widely available. Prime among them is excess heat, which is sometimes over 100 times anything explicable by normal chemistry. This naturally implicates some nuclear effect. And, indeed, nuclear products have been seen in many experiments. However, they are not the products that are expected from conventional fusion, those reactions that require high energies or temperatures. This departure from accepted physics leads some to conclude that the experiments are wrong. However, many of us take the disparity to indicate that there is something new and not understood in the cold fusion observations. Very important, a correlation between the amount of observed excess heat and the number of nuclear products has been found in a few experiments. The entire body of experimental evidence points to some of the variables in cold fusion experiments that are important, and further, indicates what numerical values must be achieved for these parameters, if anomalous effects are to be observed.

Excess Heat goes further to make the key point that a single observation of an anomalous effect, such as those associated with cold fusion, by itself deserves attention. A stark way to reinforce this point is to recall the 1987 supernova. It is certainly not understood in detail, and it is clearly neither controllable nor reproducible. However, there seems to be no question in the scientific community regarding its reality. Of course, supernovae are broadly consistent with now-accepted physical theories. But consider the famous supernova of 1024. It was visible to a large fraction of humankind almost one millennium before the emergence in the twentieth century of the theories of relativity and quantum mechanics that are necessary to understand the basics of stellar explosions.

The field of cold fusion has been full of procedural and technical mistakes. The original press conference has been described by Fleischmann and Pons themselves as a mistake. The large number of truly bad experiments remains a problem for anyone interested in getting to the core of the situation. The disconnects in communication between those doing cold fusion experiments or following the field in detail on the one hand, and the scientific community and public on the other hand, also complicates the subject. Charles Beaudette has penetrated this thicket of problems and clearly laid out the case for not dismissing cold fusion. His work both undermines the reasons for dismissal of the topic and makes the case for continuing attention to the subject. The book lays the needed foundation for a forward-looking plan to (1) put the experimental situation on a firm basis, (2) arrive at the desired understanding, and (3) exploit the remarkable new effect(s) of cold fusion for the good of humans and their planet.

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December 11, 1999

Preface

The field of study called cold fusion was born *de novo.* It did not emerge from a recognized body of continuing scientific research. It was not an extension of ongoing scholarship. No precursors puzzled the world's scientific laboratories. More dramatically, it threatened the canons of nuclear physics. This birth will prove unique in the annals of science.

Nature guards its secrets with great jealousy. To discover those secrets, the practice of science is in constant contest with nature's elemental powers. Scientific research aims to outwit nature's lock: sometimes forcing a lock, sometimes deciphering a combination, and hoping always to find a castle keep that was left unguarded.

Some of nature's most valuable secrets are the so-called laws of science. These immutable physical laws are expressed as the formulae that govern the behavior of matter and energy regardless of time or place. The formulae are mutable when nature reveals more. Then they are modified accordingly.

Occasional modification of the physical law's formulae is a well recognized part of the process of scientific progress. That kind of change, or the threat of it, usually causes severe turbulence in the world of science. Such is the case at hand.

Change is a difficult burden. In his slim volume *The Ordeal of Change,* the longshoreman philosopher Eric Hoffer contemplated one of its elements:

> Back in 1936 I spent a good part of the year picking peas. I started out early in January in the Imperial Valley and drifted northward, picking peas as they ripened, until I picked the last peas of the season, in June, near Tracy. Then I shifted all the way to Lake County, where for the first time I was going to pick string beans. And I still remember how hesitant I was that first morning as I was about to address myself to the string bean vines. Would I be able to pick string beans? Even the change from peas to string beans had in it elements of fear.

A physics professor, who played a public role in this episode, as we were quietly reminiscing about the spring of 1989, suddenly made a somewhat violent, sweeping gesture with one arm waving it at his wall of books, and declared, "If cold fusion is true, then all of this is wrong." Such was the element of fear in our topic.

Fear of harm was the source of much of the public and private antagonism that marked the subject at its beginning. Revolutions, even nascent ones in science, always hit hard and they hurt. The notion that somehow—if only things were handled better—the deep divisions could have been avoided is not a realistic sentiment.

This book is the story of a journalistic investigation into a field of scientific activity. It is not about the sociology of science or the philosophy of science although, inevitably, there are passages that touch upon those topics. That demarcation is important because there exists a cultural divide between science and the sociology of science, if not the philosophy of science. This book resides within the culture of science. It is a book of and about science.

The field was investigated largely by working with published technical reports of the laboratory research. Hundreds were reviewed and scores were digested in full. Out of that study came an explanation of the substance of the controversy and why the field developed to its continuing level of activity despite events of the first months.

Although the book contains much that was selected from the technical literature, it was especially planned to allow a full comprehension of the story by nontechnical readers. Much of the technical information presented serves to assuage the intellectual demands of those who have considerable scientific background and therefore deserve further argument.

It is organized in the usual rhetorical manner to support the primary argument about its findings. A result is that several important topics are treated in more than one place in the text. Each of the twelve *Summations* brings together its topic in concise format. Because these are not chapter summations but topic summations, they are to be read separately from the chapter text. They are placed generally within the chapter that contains their principal subject matter.

In this episode, there was the sibling rivalry between physics and chemistry. There was more to that than rivalry, however. The disciplines of nuclear physics and electrochemistry had different ways of developing scientific knowledge. The membership of the two disciplines had different temperaments. It is remarkable that two early books about the field were written by nuclear physicists and that no early books were written on the subject by electrochemists or by chemists for that matter. Most important, in some instances the fields of nuclear physics and chemistry used different protocols to define what was or was not within the discourse of science. My hope was that by following those that were well established, fear and divisiveness would be allayed sufficiently to permit a measured evaluation of the field.

One conclusion that followed directly from the investigation emerged as a failure of the skeptics to follow established protocol. In the early years, as reports of well-measured excess heat multiplied, the scientific community failed to undertake an evaluation of the phenomenon in the manner customary with experimental science.

At its tenth anniversary, March 1999, this subject involved a multitude of technologies, publications, and countries. Its story could no longer be contained within one book. It was necessary to be quite selective in choosing what to include if the story was to be manageable. The knowledgeable reader must inevitably be disappointed by my many omissions.

It was on a lark that I attended the fifth international conference on cold fusion in April 1995. As a retired electrical engineer, MIT 1952, I was looking for something new to hold my interest. At the conference, I saw that those in attendance were competent scientists doing serious research. I reached that conclusion simply by noting the quality of their technical presentations, by participating in discussions with them, and by watching them extend sharply pointed criticisms to one another's work. At the very least, the best half of them were so. Many had been honored by their associated institutions. Professional meetings often have their Saturday morning sessions for topics irreverently referred to as nuts and fruits. The cold fusion conferences were no exception to this rule.

Where much of the investigation involved the technical literature, I was pleasantly surprised to find that its best technical papers were up to the standard that I was accustomed to from my days in engineering. The talk of lax peer-review proved to be rumor-mongering. I could find no commentary or analysis of such a lack in the literature. That condition allowed at least a preliminary conclusion that they would provide useful insights into the field.

Several outspoken nuclear physicists played an important role in disparaging the field at its start and their effect was still dominant ten years later. This account necessarily refers to them often, but that should not be seen as a prejudice towards those who practice nuclear physics. My concern is only with those who were loud and strident in their castigation of our subject. Also, my arguments will certainly be seen by many as an apologia for the two chemists who started it all. The abuse that was heaped upon them during the early months, and the ridicule that continues, insures that no reasonable exposition of their surviving claims could be seen differently in the United States.

Two decisions came from that conference and my subsequent overview of the topic: to make a modest financial investment in a firm active in the field and to write a book on the subject (although at the time it was by no means clear what kind of a book it would be). What resulted was an investigation that was undertaken to determine why there was so much confusion in the subject and to find out whether a new science did exist.

The term *cold fusion* predated the cold fusion episode by several decades and came to include a mélange of topics. It became a misleading term in many ways. For that reason, the statements "cold fusion is true," or "cold fusion is false," carried no unambiguous meaning. For example, if one said, "cold fusion is false," did that mean there was no real excess heat? And if so, on what argument was the excess heat data to be dismissed? Literal reference to a cold fusion event required the use of a more specific nomenclature than the phrase "cold fusion," such as deuterium–deuterium fusion. The term *cold fusion* was adopted for this book as the name of the field of study and research simply because most references during that period used the term exclusively. I found no substitute for it that the reader would not have considered prejudicial to the inquiry.

My hope is that the reader will come to see the cold fusion contention laid out in an orderly fashion, much as the writer happened upon his own understanding of it, sometimes fortuitously, in the unfathomable depths of individual comprehension. This is a story of test and contest, of science and politics, challenge and response, integrity and cowardice, of accomplishment and of destruction.

Charles G. Beaudette

Charles G. Beaudette
Cumberland, Maine
January 9, 2000

Preface to the Second Edition

The publication of a second edition provides not only to meet the continuing demand for this book but permits it to include new data as well. A new Chapter 15 reports experiments that generate anomalous power without the presence of applied excitation power. Reorganized Chapters 16–20 present evidence for several nuclear products. These two areas of addition allow the critical reader to make an increasingly substantial evaluation of the field of cold fusion research.

C. G. B.
Cumberland, Maine
January 9, 2002

Part One

ANOMALOUS POWER

CHAPTER ONE

The Significant Claim

The French Academy printed a brief report by Pierre Curie and his collaborator Albert Laborde in 1903 to announce that the newly recognized metal radium was always a little warmer than its surroundings.[1] The metal gave off heat continuously without suffering apparent change. In a later memoir, Marie Curie, Pierre's widow, offered her appraisal.

> More striking still was the discovery of the discharge of heat from radium. Without any alteration of appearance this substance releases each hour a quantity of heat sufficient to melt its own weight of ice. This defied all contemporary scientific experience.[2]

In 1989, a certain chemistry experiment, by its claim to run a little too warm, "defied all contemporary scientific experience." Two reputable chemists at the University of Utah, Salt Lake City, in March of that year, claimed that an electrochemistry experiment generated a large amount of power in the form of an excess of heat, an amount of power that could not be accounted for by science. This phenomenon happened in an experiment consisting of a water solution in a flask with two metal electrodes immersed in it such that when a considerable electric current was made to flow between the electrodes, gas formed on them and bubbled to the surface. They also set forth an hypothesis that the observed energy came from an unrecognized or unknown nuclear process, one that did not emit dangerous radiation.

Evidence for anomalous power emerged from their heat measurements and established a scientific observation not unlike that made by Pierre Curie, whose report was accepted even though the source of the warmth was not known to science and certainly there could be no understanding of it at that early date. The two Utah chemists presented their experimental observation of excess heat to the scientific community that it might be recognized and evaluated in the same way.

These two chemists also claimed achievement of sustained nuclear fusion in their experimental flask. That announcement flew in the face of the world's hot fusion physicists. The scientific community reacted in a frenzied and skeptical manner. Shortly, knowledgeable scientists declared that their measurement of nuclear activity was severely flawed and did so with good reason. The scientists properly dismissed the measurement as a mistake.

That evaluation of the nuclear fusion claim followed proper protocol (formal procedure) in that it was evaluated simply as a measurement. Observational science offers a cosmic supernova (exploding star) or the phenomena of electrical superconductivity (electrical conductivity with zero resistance). These interest science enormously, even if their cause or mechanism is unknown. For example, the 1911 discovery of superconductivity presented a scientific question: How was it possible for a metal to conduct electricity with zero resistance? The claim to have discovered anomalous heat power presented the question: What was a possible origin of the heat power? The first question, about superconductivity, was not answered for forty-six years.

How many years of scientific study must pass before the source of anomalous heat has been determined? The process of validating a thermal measurement is properly held completely separate from its consequent questions. This separation enables the scientific community to do an evaluation in accordance with historically established procedures.

In that manner, conventional protocol calls for the scientific community to accept each well-measured observation as a *stand-alone* datum. Each, after validation, is admitted into science to begin a new field of study. Science will elucidate afterwards, as its *raison d'etre,* the underlying mechanism thus engendering further understanding of matter and energy. Scientists will bend their backs to answer the questions: what causes a supernova, and what enables superconductivity. When the process of answering the causal questions has been completed, something that may take a generation or more, science will have acquired the understanding that was missing at the first observation or discovery. In this way, the routine procedures of science provide for that understanding which is often missing at the moment of discovery.

Over the years 1989–1994, meticulous measurements were made of anomalous power. That was done with a wide variety of experimental arrangements and instrumentation, and it was done in many different laboratories. The measurements continued for a decade and were essentially without scientific challenge. They were reported in more than one hundred full-length technical articles in a number of scientific journals and constituted the field's source of intellectual motivation for the first decade.

Unfortunately, physicists did not generally claim expertise in calorimetry, the measurement of calories of heat energy. Nor did they countenance clever chemists declaring hypotheses about nuclear physics. Their outspoken commentary largely ignored the heat measurements along with the offer of an hypothesis about unknown nuclear processes. They did not acquaint themselves with the laboratory procedures that produced anomalous heat data. These attitudes held firm throughout the first decade, causing a sustained controversy.

The upshot of this conflict was that the scientific community failed to give anomalous heat the evaluation that was its due. Scientists of orthodox views, in the first six years of this episode, produced only four critical reviews of the two chemists' calorimetry work. The first report came in 1989 (N. S. Lewis). It dismissed the Utah claim for anomalous power on grounds of faulty laboratory technique. A second review was produced in 1991 (W. N. Hansen) that strongly supported the claim. It was based on an independent analysis of cell data that was provided by the two chemists. An extensive review completed in 1992 (R. H. Wilson) was highly critical though not conclusive. But it did recognize the existence of anomalous power, which carried the implication that the Lewis dismissal was mistaken. A fourth review was produced in 1994 (D. R. O. Morrison) which was itself unsatisfactory. It was rebutted strongly to the point of dismissal and correctly in my view. No defense was offered against the rebuttal. During those first six years, the community of orthodox scientists produced no report of a flaw in the heat measurements that was subsequently sustained by other reports.

The community of scientists at large never saw or knew about this minimalist critique of the claim. It was buried in the avalanche of skepticism that issued forth in the first three months. This skepticism was buttressed by the failure of the two chemists' nuclear measurements, the lack of a theoretical understanding of how their claim could work, a mistaken concern with the number of failed experiments, a wholly unrealistic expectation of the time and resource the evaluation would need, and the substantial *ad hominem* attacks on them. However, their original claim of measurement of the anomalous power remained unscathed during all of this furor. A decade later, it was not generally realized that this claim remained essentially unevaluated by the scientific community. Confusion necessarily arose when the skeptics refused *without argument* to recognize the heat measurement and its corresponding hypothesis of a nuclear source. As a consequence, the story of the excess heat phenomenon has never been told.

A few basic notions about the atom are needed if the components used in cold fusion experiments are to be recognized. The atom's center is the nucleus, a tiny object relative to the atom, that may hold two kinds of objects, the proton with a positive electric charge and the neutron with no charge. Hydrogen gas is the lightest element with one proton in its nucleus and one electron orbiting about it. It has three forms (isotopes) each of sufficient importance to have its own name. Hydrogen (H), the most common type, has no neutrons, deuterium (D) has one neutron, and tritium (T) has two neutrons. Because most of the atom's weight is in the nucleus, deuterium with its two particles has twice the weight of hydrogen. When water consists of deuterium instead of hydrogen, as in D_2O, it is about 10 percent heavier than ordinary water and is referred to as heavy water.

The two Utah chemists were Martin Fleischmann, electrochemist and Fellow of the Royal Society, and Stanley Pons, Chairman of the Chemistry Department at the University of Utah. By March 1989, they had been experimenting with the generation of anomalous (unaccountable) heat power for about five years.

Their experiment in its most general form is familiar to chemistry students. The cell, as the apparatus is called, is tightly configured. The glass flask itself has a Dewar, double walled (thermos), construction with a hard vacuum between the walls. Its content consists of heavy water (D_2O) with lithium dissolved in it to form an electrolyte (an electrically conductive solution) that fills the flask up to its neck. Inside the flask, immersed and centered near the bottom, is the cathode electrode, a palladium metal rod. Wrapped against the inside wall of the flask is the anode electrode, a platinum wire. The flask is usually submerged to its neck in a cooler bath of temperature controlled (plain) water for heat measuring purposes.

To operate the cell, a direct current is passed between the two electrodes from an external power supply. The electric current causes the water to break down into its constituent parts. Oxygen gas bubbles off at the anode (+) and deuterium gas bubbles off from the cathode (−). Some of the deuterium atoms enter directly into the body of the palladium. The temperature of the cell's liquid electrolyte, and the voltage across the two electrodes are the two measurements that tell an experimenter what the cell is doing. Because the electrolyte is slowly bubbling away, it has to be replenished at regular intervals.

Anomalous Power

Figure 1.1 is an advantageous starting point for an introduction to anomalous power.[3] The illustration is taken from an informal article Fleischmann wrote for an electrochemical society journal. In it he shows *qualitative* evidence for the existence of anomalous power. The drawing has two tracings, (the central

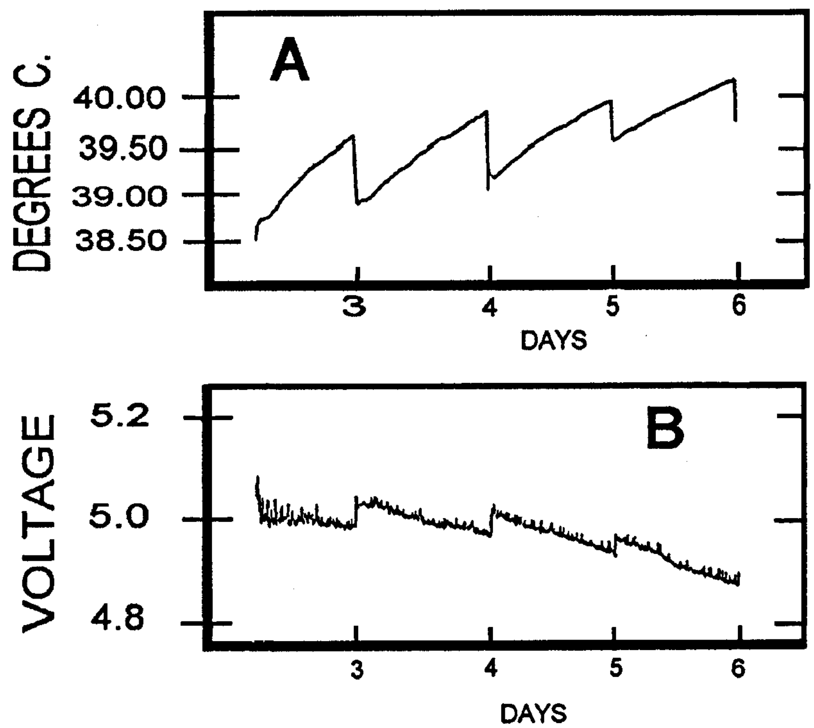

FIGURE 1.1 Fleischmann offers a qualitative display of excess heat power. When the cell voltage decreases, the cell input power decreases, but the cell temperature continues to increase.

lines in the graphs): A is a sequence of cell electrolyte temperature measurements and B is a sequence of cell electrode voltage measurements.

Tracing A shows temperature in degrees Celsius (Centigrade) as marked on the vertical axis. It is shown for one cell operating during days three through six, after the electrical current from an external power supply was turned on. The temperature climbs continuously during each 24 hour interval. A precipitous temperature drop occurs when the cell's liquid level is replenished. The temperature also increases from one day to the next. After replenishment on day three, the temperature is just under 39.00 degrees.* At the end of the subsequent 24 hour period, the temperature has climbed up to 39.75C. At replenishment, it drops to 39.20C and starts to rise again.

* The temperature and voltage numbers come from the experiment's database that was used to draw the tracings in Figure 1.1.

Tracing B shows electrical voltage as plotted on the vertical axis. The voltage across the cell electrodes decreases during each of these daily intervals and also decreases from day to day. At day three, the potential starts at about 5.08 volts, and decreases to 4.98 in 24 hours. With replenishment, the voltage jumps to 5.05 volts, and begins to descend again. Since the cell operates with a constant current from its power supply, the voltage decrease means a decrease of power delivered into the cell.*

After each addition of water, the cell ought to achieve an equilibrium temperature in ten hours, which would result in the temperature and voltage traces leveling into horizontal lines until the next electrolyte addition. How is it possible for the temperature to get hotter while the electrical input power is reduced? The experiment displays no attainment of equilibrium.

Fleischmann states, "The conclusion that there is excess enthalpy [heat power] generation is inescapable and we note that this conclusion is independent of any method of calibration which may be adopted to put the study on a quantitative basis."[4] The data demonstrate *qualitatively* that there is within the cell a hidden source of additional energy that causes the temperature to rise even as the input power decreases.

It is possible at this point to see how some scientists came to the conclusion that within the cell there was a source of anomalous heat generation that was unrecognized or unknown to science. Their source of motivation during the first ten years was to confirm and explore this now well-measured, anomalous-heat observation. The esteemed hot fusion physicist Franco Scaramuzzi states from his own laboratory experience that, "It is my conviction that some of the phenomena known with the name of CF [cold fusion] are real, in particular, the production of excess heat and its nuclear origin."[5]

Evaluation of a measurement (observation) claim proceeds in ways that might at first seem strange, or at least counter-intuitive. The protocols of science† require that the scientific community evaluate a significant measurement claim. If the two chemists' claim is sustained, then the community will be obliged to study that phenomenon until an understanding of it is achieved, no matter how long that might take.

* For the experiment of Figure 1.1, details include a solution of lithium sulfate (Li_2SO_4) in heavy water, the cell current was 0.4 amperes, the Faradaic efficiency was virtually 100% (there was no significant amount of recombination), and the coefficient of heat transfer from the cell (using a Dewar flask with a hard vacuum) was independent of time. The rate of power generation at the end of each day was reported as 45, 66, 86, and 115 milliwatts for days 3 through 6 respectively. (These calculations allow for the energy used in separating the water molecule into the two gases that then leave the cell.)

Fleischmann's cells usually have a relaxation time of about ninety minutes (with nine hours allowed to realize equilibrium), a silvered top/neck area to mask liquid level changes, and 95% radiant cooling (5% conduction cooling) to the water bath.

† Protocol means an explicit step-by-step procedure. A doctor follows the appropriate protocol in the treatment of a patient for a disease.

The Orthodox Response

Orthodox scientists by and large did not look at the heat data. Instead they demanded that the chemists show evidence of the nuclear products produced by a nuclear reaction. And, furthermore, the quantity of those products must be concomitant with the amount of energy measured. In demanding this, they had in mind those nuclear products generated by known fusion reactions.[*,6] Such products were relatively easy to find if they existed, because they were highly energetic and well known, e.g., neutrons and gamma-rays.

These scientists, however, in their public statements, refused to recognize the existence of the hypothesis about unknown nuclear reactions. They did so on grounds that there was no evidence that a new nuclear reaction had been discovered. They did not consider the measurement of large amounts of unaccountable energy to be such evidence. Lacking nuclear products, they assumed the heat measurements to be completely wrong even though they knew of no procedural error in the experiment. A few years later that attitude continued unabated even after there was available corroborated evidence that helium was the nuclear product.

In 1989, the two chemists were unable to provide evidence of nuclear products. However, finding the nuclear products of an unknown process could be a prodigious undertaking, a consideration the orthodox scientists would not allow.

Immediately then, with this orthodox response, confusion ensued. Experimenters were measuring what appeared to be a new source of energy, and critics were demanding evidence of conventional nuclear fusion reactions. The debate was sterile because the two sides were talking past each other.

The laboratory work of those engaged in the new field called cold fusion research was ignored without reason. The orthodox physicists argued that there was an unidentified error in the experimental procedure that, if corrected, would reduce the measured anomalous power to zero. With that, there would be no aspect of the experiment that was of interest to science thus bringing the cold fusion episode to an end. This response was not based upon an analysis of the experiment or its calorimetry. They saw no reason to enter the laboratory and participate in the experimental work and point out the suspected source of error. Their confidence was based on nuclear theory. To put it quite simply, they assumed and asserted that the nuclear behavior of matter could not offer-up energy under the conditions of the experiment. Their posi-

* The journal *Nature* stated, ". . . the Utah group requires that there should be 10^{12} [1,000,000,000,000] fusion reactions a second . . . to account for the rate at which heat is produced."

tion was that theory takes precedence over data. This error of scientific protocol provided the basis for their rejection of the excess heat claim.*

To demand nuclear products proved to be a serious distraction, because it directed attention to a lack, a lack of any known source for the power claimed. It incurred what might be called a "broken-wing" strategy: the successful excess-heat experimenter was asked, where are the nuclear products? In the manner of a mother bird feigning a broken wing, it distracted the inquirer from the claim. If a similar protocol had been imposed upon Pierre Curie in 1903, he would have gone unacknowledged as the discoverer of radium's self-heating phenomenon. A proper adherence to established procedure obliged the orthodox scientists to be concerned with the validity of the heat measurements.

At the announcement, the two Utah scientists pointed out that the quantity of evident nuclear products from their experiment was smaller by a factor of one billion than would ordinarily be expected for the amount of heat measured. The excess of heat defied all contemporary scientific experience: it had no recognized source. They put forth an hypothesis of a source, or cause, in accordance with normal procedure: they hypothesized that the heat came from an unknown nuclear process. This was done that the hypothesis might be explored by those who enjoyed the appropriate training and facilities for such work.

Almost all parties on both sides of the controversy recognized that if the heat came from a nuclear process, then there must be some elemental changes at the nuclear level (nuclear ash) that resulted from the process. These changes, orthodoxy said, would constitute proof of the heat claims. However, any such nuclear process was inaccessible to both parties because it was either unrecognized or unknown. To find an unknown process one might have to measure the quantity of every atomic isotope† before and after an experiment, an impossible demand. Asking two chemists for their nuclear products under these circumstances was demagoguery.

The principal skeptics were nuclear physicists. Their criticisms and references to nuclear matters at every point in the episode keep the nuclear aspect of the field always present. But this story at its beginning, nevertheless, is about the claims and corroboration of excess heat.‡

At the same time, there was no reason for cold fusion scientists to post-

* This phenomenon of anomalous (unaccountable) power is widely referred to as excess heat (flow). I use the term excess heat from time to time for stylistic variation. In either case, the description is of the flow of energy, that is, of power. Power is the flow of energy. Although both are of interest, power is the prime interest. Chapter 4 offers a further discussion of their relevant importance starting on page 51.

† The core of the atom is the nucleus with its two kinds of constituents: the proton with a positive electrical charge and the neutron with no charge. Isotopes are atoms with the same number of protons and different numbers of neutrons in the nucleus. The three kinds of hydrogen—-hydrogen, deuterium, and tritium—-are isotopes of hydrogen.

pone exploration of possible sources of the measured power. Nor was there any reason not to begin work on finding such a source if an individual scientist or laboratory was so inclined. Separation of the heat and nuclear issues mattered greatly, but only concerning the need to follow a valid evaluation protocol. Finding laboratory evidence for nuclear activity in the Fleischmann and Pons cell was an important research objective. It was properly undertaken by some scientists from the beginning.

It should be possible for the reader to follow this narrative free of anguish over the fact that, during the first decade, the origin of the anomalous energy was unknown. The exploration of that unknown reaction was not ignored. It was correctly left to the future obligations of the scientific community. In adopting that method, my account insists that conventional methodology be followed to see if the claim of discovery of anomalous heat power is a well-measured, scientific observation.*

The literature does identify a source of error in heat measurements (calorimetry) called recombination. Recombination goes with electrolytic chemistry as smoke goes with fire; it has no particular relationship to cold fusion research. Recombination is the propensity under limited circumstances of the hydrogen and oxygen gas, that bubbles from the anode and cathode, to come together and re-form water molecules. The re-forming of water releases energy that might mistakenly appear as a part of the measured anomalous power. Given the magnitude of the controversy, one would have expected to hear this argument shouted from the housetops. It was only heard as a last resort, and even then in subdued tones. The issue can be eliminated by using a closed cell, in which all the gasses are deliberately recombined back into water. Chapter 14 includes an outstanding example of such a cell starting on page 190. Also, control experiments are subject to the same recombination effects and they do not demonstrate anomalous power.

My Epiphany

Immediately upon starting this investigation, I surfaced a credo for the cold fusion episode as formulated by one of the field's most ardent skeptics. It

‡ This approach may irritate those nuclear physicists who strongly prefer to dismiss the seemingly amorphous and unquestionably difficult matter of heat measurement. They would greatly prefer that we move straightaway to the more tangible business of counting particles. I offer my sincere apologies to them. Only by anchoring this book in the area where there is abundant data and substantial analysis available can the confusion be dissipated.

* An "observation," as used in this story, does not refer to looking or seeing in the usual sense. It means to record scientific data, even though the oversight of an experiment by the scientist continues to be important. This recording or storing of data is done by instruments in most cases.

showed that during the first four years (to closure of his manuscript), this outspoken physicist had not examined the laboratory processes and procedures upon which professional activity in cold fusion studies was founded and motivated. That fact was not remedied in subsequent years. Furthermore, the same lack was true for other outspoken skeptics of cold fusion research.

In the immediate aftermath of the announcement many laboratories jumped into the activity of trying to replicate Fleischmann and Pons's experiment. In the course of several months, four prestigious laboratories reportedly failed to replicate the anomalous power claim. This topic, that of the many, many *failed* experiments, is explored in Chapter 8, starting on page 106, and found to offer no guidance for our investigation. A related topic, that of the *reproducibility* of the experiment, which involves exploring the variety of scientific methodologies, especially the particularly narrow methodology adhered to in the discipline of nuclear physics (with its strict criterion), is developed and analyzed in Chapters 10 through 13. The reproducibility of the two chemists's experiment compares favorably with that of newly tried experiments in other disciplines.

Another consideration often raised by orthodox critics is whether the electrolytic cell itself constitutes a useful energy-generating device. In the experiment, which lasts three months, the total energy balance of the cell may be positive or negative. This book is about science and scientific methodology. Its proper interest is in the ability of the Fleischmann and Pons cell to give a sliver of insight into the workings of nature. Does a new science exist in their experiment? If so, then a *later* interest will explore whether there is a useful technology that can be derived from that science. In turn, from that technology would come a useful energy generating device if such is possible. But to get there one usually has to travel a long and rocky road. A comparison of the total amount of energy put into the cell with the total amount that comes out is of only passing interest. The electrolytic cell may well prove itself overall to be an energy inefficient instrument for these scientific studies.

Circumstances

The field of cold fusion studies was surprisingly vital after ten years, despite its quick and categorical dismissal in 1989. At the Massachusetts Institute of Technology, Cambridge, Massachusetts, a tenured professor continued in his attempt to explain a nuclear source for the anomalous power. He was not working alone. Over 150 scientists working in the field participated in the eighth International Conference on Cold Fusion (ICCF-8), May 2000, in Lerici, Italy. A Wall Street financier displayed a thick dossier that was within a few weeks of being current on developments in the field. Three small enterprises in the United States had raised more than a million dollars each to finance new product development during the previous two years. A few major American corporations had carefully watched these developments and invested token amounts of venture capital with the fledgling companies. Research continued on cold fusion topics in academic and national laboratories in America, Japan, China, Italy, India, Russia, Belarus, and France. More than 1,000 full-length technical reports of cold fusion research had been published, and they continued to be published at the rate of approximately fifty a year. The twentieth century had not previously experienced a dichotomy in the basic sciences as great as that which existed in the 1990s between orthodox science and the heterodox research called cold fusion.

It is much easier to describe orthodoxy than heresy when writing about this controversy. The orthodox argument in many instances can often be set forth in one sentence. Simply because it is orthodox, the writer can expect immediate recognition and appreciation of the argument. The task of presenting an heretical position often proves laborious. It needs more space, as there is much to be explained and in considerable detail if it is to be equally persuasive. The reader should not assume that the orthodox presentation is lax simply because of the smaller amount of space devoted to it.

It is well recognized that there is not just one scientific method or protocol. Rather there are a variety of methods whereby science elucidates the physical world. My investigation repeatedly came upon striking differences of protocol within the contentions that had racked the field. The differences were not simply between physics and chemistry. There were significant differences of method between the protocols of nuclear physics and the rest of science, including other disciplines in physics. Those differences were at the heart of the disagreements, and they would have been severe obstacles to mutual understanding even if there had been strictly rational behavior on all sides.

In particular, this book is concerned with the methodologies of nuclear physics and those of surface chemistry.* To help put them into perspective, examples are taken from solid-state (condensed matter) physics, geology, astronomy, and biology as well. One of the methodologies that is universally respected asserts that discovery claims are corroborated by replication in an independent laboratory. This rule appears to be acceptable to the various disciplines of interest (except that of nuclear physics). The well recognized rush to be second, to be the one to corroborate an announced discovery, is an expression of this protocol.

By whatever methodology, orthodox science insisted that there was nothing of interest in cold fusion studies, nor was there anything that might be commercially useful. It said so emphatically in 1989, both in professional

* The term *surface chemistry* refers to chemical reactions that take place on the surface interface between a solid electrode and, in this case, a liquid.

meetings and in a government sponsored study of the field, and it continued with that opinion twelve years later. After 1989, physicists, chemists, and the scientific community as a whole paid little attention to further developments. Research reports that would ordinarily be of wide interest were not reported, or were mentioned and dismissed as probably mistaken. Twelve years after its beginning, the status of its research was unknown to the scientific community.

During these years, the several technologies that had become a part of cold fusion research promised a benign environmental impact if they could be exploited. This is an important consideration if their initial use will be in low technology societies, as seems likely. There, a source of energy that does not strip the countryside bare of wood fiber growth, for example, is advantageous.

At the turn of the twentieth century, there was a vigorous concern with the relationship between experiment and theory in the core sciences of chemistry and physics. The general conclusion was that science was best advanced when experiment and theory mutually supported each other in giving direction to research. A half-century later, instrumentation joined them as a third component enabling the advancement of science. To a considerable extent, this principle guides the development of this story. Galileo discovered the moons of Jupiter largely because he knew how to grind the best lenses of his day. This story recognizes the Fleischmann and Pons Dewar flask, with its hard vacuum, silvered upper section, and resting in a water bath, to be a candidate for recognition as a significant advance in calorimetric instrumentation.

Proof

Scientists on all sides desire a final resolution of this matter. The wish for proof is universally enticing.* Consider for a moment that atomic and subatomic particles are intrinsically perfect. Experiments with them were often done in a high-vacuum chamber where the environment was also quite simple and even perfect. This level of perfection in the experimental system enabled the devising of definitive experiments that forced nature to reveal some of her innermost secrets. Nuclear physicists became accustomed to achieving proof. In fact, they demanded much more than proof, as is shown in the strict criterion of Chapter 11, p. 155. Mathematicians working with perfect numbers, perfect geometry, and perfect logic likewise learned to routinely require proof.

Most scientists, however, made progress with mere experimental outcomes, devoid of clear-cut proof. For much of science, proof appeared over time as an overwhelming aggregation of evidence. In this narrative, the question to be answered was whether anomalous power existed in the Fleischmann

* By proof, I mean measurements that to a chemist or physicist are irrefutable.

and Pons experiment. The answer was sought after, even though it may not have been available then through an absolute proof. If no method of proof was accessible at the time, an insistence on proof would serve only to force a false-negative result.* That possibility needed to be limited in the same way that the likelihood of reaching a false-positive conclusion was also deliberately limited. Mere evidence would have to do if the possibility of a false-negative outcome were to be constrained.

I examined the body of research papers on anomalous power. Surprisingly, the presentation of it to the public was uncharted territory considering the books that had been written on the cold fusion episode. The well-charted part consisted of nuclear physics as expressed in several critical books that were devoted almost entirely to that subject. They included no examples of excess heat data, and, astonishingly enough, no bibliography leading to such examples. A principal theme of this narrative is that the several arguments offered against the significance of anomalous power measurements were either unsupported by data, contained mistaken assumptions, or involved a corruption of protocol.

The First Six Years

Within one year the scientific community had stopped following technical developments in the field. During an interview in 1997, I mentioned Dr. Oriani's experimental results to one scientist who participated in the frenzy of 1989. He replied that he was too busy to follow developments in Italy. I then gently mentioned that Professor Richard A. Oriani is a professor emeritus at the University of Minnesota. This same academic scientist went on to talk about cold fusion events as though the events of 1989 had happened just the previous year.

During the second year, the divide between cold fusion research and orthodox science became quite complete in the United States. The *New York Times,* the *Wall Street Journal,* and two of the four broad-audience scientific journals, *Scientific American,* and *Nature* (London) were adopting the position of giving no recognition to its scientific reports. *Science* (published by the American Association for the Advancement of Science) and *Chemical & Engineering News* (published by the American Chemical Society) report snippets, with the latter's reports often accompanied by dismissal or ridicule. As a consequence of this disconnect, most American scientists still viewed cold fusion research as it was described to them during the spring of 1989, when confusion reigned and the subject was dismissed absolutely.

* "False negative" means a negative answer that is at the same time a wrong answer.

The phenomena which scientists referred to as pathological science is "the science of that which is not so." Charlatans get tossed into that bin. The topic unfolds with sufficient detail in Chapter 5, p. 62, to clearly demarcate its position relative to cold fusion studies. The polywater episode of the 1970s is also reviewed there for purposes of comparison.

The nearly six years from March 1989 through the end of 1994 provide the needed perspective. That longer view offered the explanation of why there were some hundred or more scientists working in the field, and why so many full-length technical papers continued to be published. Skeptics asserted that the claims required miracles and that the participants were believers. In general, the field was populated by scientists who had carefully examined the evidence for anomalous power and found it persuasive: there were no "believers." The fact that a number of scientists had found the evidence for anomalous power compelling will be seen to be most reasonable, even if other equally competent scientists had chosen to disagree with them. Furthermore, if the source of the anomalous power is someday registered in physics textbooks, those textbooks will contain no miracles, just as contemporary physics textbooks have in them no miracles.

An Orthodox Article

The scientific community quickly dismissed the claims advanced at the Utah announcement. A prominent physicist, writing five years later, described correctly how orthodox science in America ". . . a mere five weeks after it began . . . cast cold fusion right out of the arena of mainstream science."[7] That was accomplished in the biblical forty days and forty nights after the original disclosure in the *London Financial Times.* At a meeting in Baltimore of the American Physical Society, two scientists declared that the cold fusion experiment did nothing unusual, that on theoretical grounds it could not do anything unusual, and that the two founding chemists were incompetent and possibly delusional. With that event on May 1, 1989, orthodox science in America set its shoulder against cold fusion research. Within the same year, the two chemists were the subject of easy ridicule, and within one more year most sources of further funding for them had disappeared. After Baltimore, any scientist in the American academy who evinced professional interest in pursuing cold fusion science placed his career in some jeopardy. Aspersions about cold fusion as a pathological science continued to have their impact twelve years later and helped greatly to turn science away from its duty to study the claims.

The article about cold fusion studies that was quoted above was published in *The American Scholar,* autumn 1994. It deserves special attention for

SUMMATION

The Settled Contention (c. 1994): Orthodox and Heterodox Positions

1. The orthodox position of the scientific community insisted that cold fusion was not proved: in 1989 there was no such thing as cold fusion in the Fleischmann and Pons experiment. That condition still held six years later.
2. The heterodox position was held by those working in the cold fusion research community. It insisted that claims for the generation of anomalous power in amounts well beyond what was possible through chemistry were well validated by varieties of replication and instrumentation in independent laboratories.
3. a. The orthodox retort to 2 (above) said that each and every experiment, *without exception,* purporting to demonstrate anomalous power was somehow fatally flawed, and that if the flaw were removed, the measured anomalous power would become zero.
   b. Occasionally, the orthodox retort to 2 (above) said that each and every report, *without exception,* purporting to describe anomalous power was somehow poorly peer-reviewed, and that if the report were adequately reviewed, the declared anomalous power would become zero.
   c. Occasionally, the orthodox retort to 2 (above) said that each and every experimenter, *without exception,* purporting to have demonstrated anomalous power was somehow mentally ailing, and that if the ailment were cured, the measured anomalous power would become zero.

Example:
An illustration of statements 1, 2, and 3 rolled together was the statement by Robert L. Park (American Physical Society spokesman) made in September, 1996, "There has been not one iota of progress in seven years."*

4. Statement 1 (above) was conditionally true. It was true only (1) if the term cold fusion was taken literally to mean a nuclear fusion process as recognized in contemporary physics, and only (2) if one overlooked its implied denial of anomalous power measurements.
5. Statement 2 (above) was true. Statement 2 retained conventional scientific protocol by asserting nothing about the energy source, whose identification was properly left to the future obligations of science.
6. Statement 3 (above) was false. Statement 3 was *without* support in the peer-reviewed literature, *without* support in laboratory experience, and *without* theoretical hypothesis.

Example:
In March 1989, the University of Utah announced that two chemists had found a certain electrochemical experiment that ran warmer than could be explained by all contemporary scientific experience.

* Park, Richard L., Private communication, Sept. 26, 1996.

four reasons: for what may be called pride of place, for its provenance, its content, and its timing. Its place is that of the only dispassionate article written by an (orthodox) scientist in the decade after 1990; its provenance derives from the author being a senior member of that institution which contributed much to the early anathematization of the field; its content exemplifies and amplifies some of the principal thrusts that constitute the design of this book; its timing was almost exactly when one might first have perceived that the original measurement of anomalous power had become well validated. The article's purpose, apparently, was to open a dialog on the legitimacy of cold fusion research within the orthodox community. If that was its purpose, the endeavor failed. The article is given further consideration in Chapter 23.

The New Discipline

The pervasive question during the first decade was whether or not a new science or a new physics was born. My answer to that question involved what might be seen at a glance as a substantial digression. The investigation examined anomalous power's *validity* by looking at scientific method or protocol. To appreciate the process of validation required examination of prevailing methodology. There are several sources in the scientific literature from which to obtain whatever wisdom was to be found there. Quotes from well recognized sources were not used simply to ornament chapter headings; they illustrate the considerable variety of application and discipline that exist in scientific methodology.

It has been said that the quality of papers in cold fusion studies was lax. These complaints usually came from those who would like to see nothing printed on the subject. Others of the same ilk wanted only one topic printed: was there deuterium-deuterium fusion or not? The assertion that the papers are of poor quality is not supported by an analysis. After four years of studying the principal papers that pertain to anomalous power and the published criticisms of those papers, I do not perceive any systematic weaknesses in them. The *Journal of Electroanalytical Chemistry* did not suffer for its role as a principal outlet for the Fleischmann and Pons papers. *Fusion Technology,* a prominent publication for cold fusion papers, was castigated by a few for editing them to special rules, though the rules maintained the integrity of the content. Some members of its board of editors say that it ruined the publication, but their arguments were without supportive data.

The most rapid and efficient means of communication was publication of technical reports in the proceedings of the International Conference on Cold Fusion (ICCF) meetings held about every sixteen months. I saw a level of critical commentary directed at presentations that was not unlike what I had seen at conferences on other topics. There were individuals who looked only for evidence of deuterium-deuterium fusion, especially at the early conferences. When that was lacking, they announced to the press that they had seen nothing new to change their conclusion that cold fusion research was pathological science. There were also those with outlandish interpretations of the experiments. These were as politely ignored by the conference as they would be at any professional society meeting.

In all this give and take, the part played by the skeptic is quite separate from that of the critic. The skeptic's perspective was located at a considerable psychological distance from that of the critic. In my text, the two are delineated from one another. The critic is placed in Chapters 5–9 and the skeptic in Chapter 22.

In 1993, four years after the initial press conference in Utah, the book *Bad Science: The Short Life and Weird Times of Cold Fusion,* by Gary Taubes, enjoyed considerable success. It soon became clear, though, that, "*The Short Life* . . ." was not going to be so short. Nine years later, in 2001, that life continued with some considerable vigor. The question must be asked, What is going on there? Answering that question is one of the primary purposes of this book. We continue with an overview of the rubble.

CHAPTER TWO

The Overburden

At one o'clock on Thursday afternoon, March 23, 1989, a press conference convened at the University of Utah in Salt Lake City. Several hundred people assembled there including university trustees, senior faculty, and members of the chemistry department, along with a sizable contingent of the American national press. Dr. Chase N. Peterson, President of the University, James Brophy, Vice-President for Research, and Robert Nesbit, Dean, Faculty of Science and professor of geology, University of Southampton, England were on the dais. At the center was Dr. B. Stanley Pons, research professor of chemistry and Chairman of the Department of Chemistry, University of Utah. Beside him was Dr. Martin Fleischmann, research professor of chemistry, University of Utah, research professor of electrochemistry, University of Southampton, and Fellow of the Royal Society.

A press release was available from the university public relations office. Peterson and Brophy made statements of a general nature pointing out that the experimental claims to be announced will have to be evaluated over the next several months and years by the scientific community.

Dr. Pons was then invited to speak. He explained how he and Dr. Fleischmann developed an electrochemical experiment in which a deuterium-deuterium type of nuclear fusion reaction was sustained at room temperature. Fleischmann followed with his affirmation that a sustained fusion reaction had been achieved, and he held up for viewing the special type of "test tube" they used. It was a double walled, Dewar flask that permitted them to measure the evolving heat.

Questions were invited from the press and they prompted some memorable answers. Fleischmann responded to a question that suggested their experiment was "kitchen chemistry," saying, "It's a pretty big kitchen." To another question, he commented that the neutron particle radiation observed was, "[a factor of] a billion times less than what is experienced with the nuclear reactions of physics. So we have a relatively low rate of production of neutrons."[1] It is of some interest that Pons seemed quite enamored of the specific fusion claim, while Fleischmann appeared sure-footed in going well beyond it to articulate the hypothesis of an unknown nuclear process.

The formal conference was followed by a tour of the small laboratory. The guests saw several cells, each in its Dewar flask. Each cell (flask) was filled nearly to the top with an electrically conductive liquid (electrolyte). Submerged in it were the two metal electrodes of platinum and palladium. Plastic tubes and insulated wires led from the cell tops to an array of electronic equipment supported in steel frames behind the baths. The press conference ended less than two hours after it had begun.

The press release[2] staked out a number of claims. A principal one concerned the source of the energy:

> [They] have . . . created a sustained nuclear fusion reaction . . . This generation of heat continues over long periods, and is so large that it can only be attributed to a nuclear process . . . reactions lead to the generation of neutrons and tritium . . . The device . . . produces an energy output higher than the energy input.

The release offered no specific numbers for heat output or for power gain.

Prior to the announcement, the University of Utah filed several patent applications on the work of Fleischmann and Pons.

Unfortunately, the technical paper that Fleischmann and Pons had completed and that was accepted for publication the previous day was not available for distribution. The omission constituted a breach of protocol, as did their failure to brief their colleagues in the chemistry and physics departments beforehand.

Their paper, in the form of a Preliminary Note, was published in the *Journal of Electroanalytical Chemistry* on April 10.[3] Its length was severely restricted as the purpose of the note format was to obtain rapid publication. It described the results of four kinds of experiments that they had conducted over the previous five years. It apparently was completed with great haste as it had many errors. The authors soon circulated an errata sheet, which the *Journal* published on May 10.*

It was correct to say that, by this experiment, Fleischmann and Pons

* The errors included leaving off the name of Marvin Hawkins as the paper's third author. They somehow managed to include his name in endnote five, where the future, unpublished, full-length article was referenced.

SUMMATION

The Original Claims of March 1989

It is worthwhile to list Fleischmann and Pons's original claims announced in the press release and in the press conference.

1. They had achieved a sustained deuterium-deuterium fusion at room temperature in a bench-top chemical experiment.
2. In tests lasting many hours, more excess heat output was detected than could be accounted for by known chemical properties.
3. Observed neutron radiation was [a factor of] a billion times too small for conventional fusion to be the source of the heat energy.*
4. They had achieved twenty watts of heat generation per cubic centimeter (cm^3) of metal cathode (palladium).
5. A tritium buildup was measured (using a Beckman scintillation counter).
6. A gamma-ray spectrum† was measured emanating from the water bath in which the experimental cell was immersed.

The Preliminary Note contained further details on the extent of their work. Its claims were as follows.

1. They had achieved more than ten watts of continuous heat generation per cubic centimeter of palladium cathode during an experiment lasting for more than 120 hours.‡
2. The accumulated anomalous energy was in excess of one kilowatt-hour for each cubic centimeter (cm^3) of palladium cathode.
3. Neutron emission from the cell was identified by the gamma-rays that the neutrons generated in the water bath.
4. The intensity of radiation was about 40,000 neutron particles per second for one of the cells.
5. Tritium accumulated in the electrolyte to the amount of 100 radioactive counts per milliliter per minute.
6. Since little of the heat energy came from known nuclear reactions, the hypothesis was offered that other nuclear processes were involved.

* Nuclear reactions often emit radiation consisting of neutron particles moving at high velocity.

† High-energy gamma-rays are sometimes emitted from nuclear reactions. The implication of this claim is that the cell is emitting neutrons that interact with the water, thus generating the gamma radiation. The spectrum is the distribution of energies in the radiation.

‡ For comparison purposes, the smallest burner on many electric stoves in the Unites States operates at a maximum (glowing orange) heating level of 1,500 watts. The author estimates the burner element as having approximately 24 cubic centimeters of volume, giving a heat level of 62 watts per cm^3. So the claimed heat (for each cubic centimeter) of cathode rod is about one-sixth that of a stove burner running at full heat.

claimed to be the first to discover anomalous power, continuous cold D-D fusion, and the profound matter of nine orders of magnitude inconsistency between the two.* They resolved that inconsistency by advancing the hypothesis that the heat was the product of an unknown nuclear process.

The claim presented in the Preliminary Note that neutrons had been measured was discarded by the scientific community within a few months. Certainly Fleischmann and Pons were wrong to attempt to defend it as they did. They should have formally disowned that part of the note. They went on to make a new set of measurements during the remainder of 1989 and again claimed the detection of neutrons, but at an exceedingly low level.

Their helium-four† measurements were done in the winter of 1989 as a limited experiment without the controls and documentation required for publication. As a result, it was of benefit only to them, and could not be presented to the scientific community for evaluation. Claims for the evolution of tritium were done quickly in the few weeks prior to the announcement and, also, were not adequately documented for publication. Fleischmann and Pons did no more research to detect tritium. They looked unsuccessfully for helium for several more months before leaving that search to others.

Orthodox Reaction

Reactions to the announcement were of two kinds. The general reaction was one of wonderment within the scientific community. Unconditional disbelief was openly expressed by many scientists, particularly by nuclear physicists. Most other scientists did not offer judgements. Their comments were of the wait and see variety.

Professor H. W. Lewis (a physicist at the University of California at Santa Barbara) immediately published a brief article that insisted "It was against the laws of nature," and "we poor mortals can do nothing about that."[4] Within days, Dr. John A. Wheeler, a prestigious American nuclear theorist, compared the Utah claims with those of a turn of the century French scientist whose claims were found to be entirely the result of self-deception.[5] A physicist at the California Institute of Technology (Caltech), Pasadena, California, said quite simply, "It's bull—."[6] And so it went with a number of physicists expressing

* In the phrase anomalous power, anomalous means that the measured heat power is beyond the current experience of science. Use of the term power indicates that the flow of heat, as well as the heat per se, is of special interest.

† There are two isotopes of helium that are of interest. Helium-three has two protons and only one neutron. Helium-four has two protons and two neutrons in its nucleus. These are written as ^{3}He and ^{4}He respectively. Helium-four is the common type that is present in air in slight quantities and is used to fill balloons.

quick, absolute judgements. Those opinions were mostly based upon a consideration of the precepts of nuclear science, where the release of heat was accompanied by prodigious particle radiation that was lethal at the level of a fraction of a watt of power.

The plasma (hot fusion) physicists naturally dominated the response to the claim of nuclear fusion. Hot fusion occurs when the deuterium and tritium forms of hydrogen were confined at extremely high temperatures. Fusion, as they understood it, occurred only with the availability of highly energetic (hot) particles. The claim of fusion at room temperature was referred to as cold fusion. Use of the word fusion placed the Utah announcement in their area of expertise. The broad-audience scientific journals chose physicists to conduct their response. To a considerable extent, the die was cast during these first days. Eight years later, *Scientific American* magazine was still calling upon fusion physicists to interpret research in the field of cold fusion studies.[7]

During the spring of 1989, response to the published paper included the complaint that no control experiments were performed. John Maddox, editor of *Nature* complained in the April 27, 1989, issue, ". . . the Utah group . . . had [not], before seeking publication, carried out the rudimentary control experiment of running their electrolytic cells with ordinary (light) rather than heavy water."[8] Contrarily, Fleischmann was enticed by the evidence for large amounts of unaccountable power in the form of heat without radiation, rather than by low level fusion. He suggested replacing the palladium electrode with an exhausted one or with a platinum rod for a control experiment, rather than changing the type of water.* As evaluation of the claims began, the first profound misunderstanding was established in this subtle way. This topic warrants more detail in Chapter 9, p. 112, but the confusion between the two applications of control experiments was never resolved.

At MIT, Professor R. D. Petrasso examined Fleischmann and Pons's paper carefully because the displayed spectrum for neutron evidence did not seem to be an allowable shape. He presented his doubts to the science community and Fleischmann and Pons had to back down. This undoing over the nuclear measurements hurt their scientific credibility enormously. The matter is developed more fully in Chapter 8, starting on page 102.

By the summer of 1989, with Fleischmann and Pons openly ridiculed, the many failed heat experiments, and the dismissal of their nuclear measurements, the two chemists were wholly on the defensive. Yet to come was an official government report setting government policy to rule out their work and claims, and a *coup de grâce* in the form of a long lampoon about them in

* Fleischmann says that when *Nature* magazine complained that no control experiments were made, he wrote to them about this particular experiment, but the magazine never published his letter.

the *New York Times Sunday Magazine.* The orthodox scientific establishment assumed with good cause that the episode would disappear in a few months to become a footnote in the annals of science.

But that was not the end. During the summer, laboratories in Menlo Park, California, and Minneapolis, Minnesota, finished anomalous heat experiments that corroborated those of Fleischmann and Pons. More such successful experiments continued to be reported over the ensuing months and years to sustain the most enormous conflict in basic science of the twentieth century.

The Overburden

One result of this large public conflict was an overburden of debris, a terminal moraine from the avalanche of failure and skepticism. This overburden, Figure 2.1, consisted of ten layers of debris piled high upon the evidence for the claim of anomalous power. The order of the ten items moves from the most distant in generality to the most immediate.

The name "cold fusion" stuck to the Fleischmann and Pons experiment from the first instant even though excess heat and the lack of dangerous radiation were the phenomena of social and scientific importance. The term resonated in the public domain because of its familiarity to the public and to science buffs and reporters. "Pathological science," as a second name, also stuck because it answered to the fears of professional scientists. It mattered not at all that it did not follow Langmuir's stated criteria. Finally, in the public domain, superficial evaluation by a government panel nearly buried this emerging new field of study.

At the level of scientific analysis, things did not go much better. Normal scientific protocols were distorted beyond recognition. As a substitute for evaluation of the heat measurements to see if they could withstand examination for procedural error by experts, a cry went up that the heat measurement did not correspond with the nuclear measurements. Twelve years later the scientific community is silent. Those well-made heat measurements have not been examined and reported to the scientific community. Meanwhile, two measurements showing an exceedingly rapid expansion of the universe has set the cosmologist's house on a roar.

It has been argued that the experiment is not sufficiently repeatable, while during these same years the science community proudly displayed the first cloning of a mammal in an experiment that produced one success out of 227 tries and a wait of eighteen months for confirmation in an independent laboratory.

The publication of Fleischmann and Pons's calorimetry in peer-reviewed

Misdirection by the misnomer COLD FUSION

Superficial evaluation by the government

Mistaken label of pathological science

Vitiation of scientific protocol

Mistaken demand for understanding & theory

Mistaken demand for experimental repeatability

Misplaced concerns about failed experiments

Rumors of lax reviews

The stain of *ad hominem* assault

Hasty experimental evaluations

Was the existence of anomalous power
well confirmed by its replication
in independent laboratories?

FIGURE 2.1 Ten items make up the overburden borne by the evidence that supports the existence of anomalous power. The listing progresses from the general to the particular.

journals greatly dismayed the skeptics because peer review was supposed to be the gateway to scientific respectability. They pointedly ignored such presentation and abrogated professional standards by rumor-mongering the notion that the peer review was lax. Never did they bring a proper professional response to these published articles by pointing out evident error in an article which was then submitted for itself to pass peer review and publication.

The combination, at Baltimore, of outrageous personal attack with the false insinuation that the Utah experiment had been reconstructed at Caltech and thereby found lacking in rigorous design, added up to the heaviest part of the overburden. The ten items constitute a partial table of contents for this book. Those were the principal matters that must be understood if the field was to be sorted out and appreciated. Only when the rubble has been moved out can the anomalous power evidence take its proper place.

After the debris was removed and our subject uncovered, anomalous power provided three topics for those in the field to pursue. These three matters were to find a more efficient experiment for scientists to work with, one that was faster to respond and more easily replicable. Next, there was the need to determine the extent of the phenomenon (i.e., did the process occur in biological systems), and finally, there was the intellectual obligation to find the source of the heat energy.

By way of scientific introduction, a few words about the atom are appropriate. The atom is the basic building block of all materials. From high school science, recall that the atom has a tiny core called the nucleus and orbiting electrons at a great distance from that core. The proton has a positive charge, and the orbiting electron has an equal and negative electrical charge. The atom normally contains an equal number of protons and electrons so that it is electrically neutral.

There are about one hundred different atomic types as elements. This account of the cold fusion episode will have an interest in only the first two, hydrogen and helium plus the heavy precious metal palladium. As was mentioned earlier, there are three *isotopic* forms of hydrogen. They are named hydrogen (H), deuterium (D), and tritium (T). A nuclear reaction, called fusion, occurs when two deuterium atoms combine to form a helium atom, and a great deal of energy is released.*

As radium was discovered to be warmer than the ambient temperature, so Fleischmann and Pons's experiment runs warmer than it ought to run. The measurement was difficult—an MIT scientist swore it was more difficult than plasma physics—so our first look was a qualitative one. The key to a successful measurements claim resided in a precise design of the instrument or cell, which was configured not so much to enhance anomalous power production as it was to make possible its precise measurement. The Fleischmann and Pons cell may reasonably be thought of as a newly designed heat measuring instrument or calorimeter.

B. Stanley Pons

The cold fusion episode was an enormous heresy. No matter which way you look at it, a lot of people were confused. What sort of persons were B. Stanley

* This statement is sufficient for the technical depth of this story in matters that are nuclear. In more detail, deuterium-deuterium fusion produces tiny amounts of helium-four (a rare product), and large and equal amounts of helium-three and tritium plus protons.

Pons and Martin Fleischmann to bring this controversy upon science, and America, not to mention themselves?

They first met in 1975 when Pons arrived at the University of Southampton in England, where Fleischmann was head of the Department of Chemistry. Pons had come to evaluate it as a place to reenter academia by working for his doctoral degree after eight years in the world of business management. It was a large department, so Fleischmann did not have much time to give to Pons, and did not work with him while he was there. Fleischmann introduced him to Professor Alan Bewick, who was his teacher, mentor, and thesis advisor.

Pons grew up in the rural town of Valdese, North Carolina. The town had a population of about 4,000 and had been settled at the turn of the twentieth century by Waldensian immigrants from northern Italy. In the United States, the Waldensians were affiliated with the Presbyterian Church. In northern Italy, their sect was seven centuries old. Their communities were mostly located in the Italian Piedmont and in the valleys of the Cottian Alps. The sect originated during the twelfth century as a reformation movement against the Catholic Church. At an early point, eighty of its members were burned at the stake for their heresy. The Waldensians acquired permanent allies with the emergence of the religious reformation movement in the sixteenth century. In the early nineteenth century, Victor Emmanuel II tried to drive them out of Italy, but they found protection under Charles Emmanuel where they survived bloody persecutions and emerged into the modern climate of somewhat greater religious tolerance. This heritage seems to have bequeathed to Pons a fierce tenacity to stand his ground when confronted.

The young Pons graduated from Wake Forest University in 1965. He continued in the academic world with graduate studies at the University of Michigan for two years. Sensibly enough, he left his graduate studies and moved into the family business, Pons Enterprises, a textile manufacturing conglomerate. After eight years of business management, he was restless for a more intellectually demanding pursuit. He decided to return to academia to get an advanced degree in chemistry. By going to an English university, he could avoid the extra step of obtaining a masters degree before starting on his Ph.D. studies. Besides, Southampton was reported to have the best electrochemistry department in Europe if not in the world.

When Pons entered the chemistry building at Southampton, he saw that they were clearing a corridor of people, so that it could be used for an experiment. A crossbow was set up at one end, and a target of straw bales at the other. Attached to the arrow was a narrow tube of glass called a capillary that was heated to a red glow. The purpose of the corridor-long exercise was to make a micro-capillary tube. Firing the bow would cause the glass tube to be stretched narrow and thin before it cooled. Fast stretching was needed, and the crossbow could do just that.[9] With that unconventional initiation into the Southampton practice of "chemistry," Pons was persuaded to stay. Being much older than the other students, and independently wealthy, he was an unusual student.

At Southampton, his thesis work was a mixture of sub-disciplines. He started with some organic electrochemistry, some spectroscopy on electrode surfaces, and went on to do (Fourier) transform spectroscopy, these being methods to measure chemical reaction processes on electrode surfaces. After receiving his doctorate degree in 1978, he went back to Michigan at Ann Arbor and shortly moved on to a position in Edmonton, Canada. In 1983, he moved again, this time to the University of Utah where he achieved the title of full professor in 1986, and department head in 1988.

Martin Fleischmann

Martin Fleischmann spent much of his professional life at the center of scientific controversies of his own making. He was accomplished in the practice of surface catalyzed electrochemistry, a particularly difficult field due to the recalcitrance of catalytic processes.

He was born in Carlsbad, in the Sudetenland, Czechoslovakia, on March 29, 1927, and was raised as a nominal Roman Catholic. He grew up in pre-World War II Czechoslovakia with middle class parents, themselves cultural remnants of the Austro-Hungarian empire. During the 1930s and 1940s, his family clan was split asunder, politically. His family connections in Czechoslovakia were thoroughly disrupted by the German occupation followed by the subsequent Soviet occupation.

The Gestapo arrested Martin at the age of eleven. His father, a hero during World War I and later an attorney, was abused by the Gestapo to the point of disablement during those years. After the occupation of the Sudetenland in October 1938, the family (mother, father, Martin, and older sister, Suzanne), were rescued by a WWI comrade of his father's. They escaped in a taxi to the unoccupied part of Czechoslovakia before the "protection" of Slovakia was accomplished by its occupation. During that brief interregnum, it was arranged in a fashion not uncommon in those times for Martin to be adopted by an English bachelor. His sister was adopted by another person. This maneuver enabled the family to move to England in March of 1939, just before the deluge.

Martin had been well educated as a child, and entered the British school system at Worthing. After graduation from secondary school, he competed for and obtained a place at the Imperial College of the University of London. He obtained his baccalaureate in 1948, and entered the graduate school. After just two years, he completed his requirements for a Doctorate in Chemistry in 1950.

As a student, he brought a strong overlay of mathematics to his chemistry, an unusual characteristic for a chemist, and one that was to remain typical of his work during his career. Distinguished Professor of Electrochemistry at Texas A&M University John O'M. Bockris tells the tale from their days at Imperial College of Fleischmann's propensity for mathematics. When visitors stayed overly long, Bockris offered to help them with their questions by calling in one of the school's students to further clarify the discussion. Fleischmann, by way of explanation, gladly covered the chalkboard with differential equations and such, until the visitors allowed as how it was necessary for them to be on their way. Fleischmann's skill with mathematics would play an important part in the cold fusion episode.

For his thesis, Fleischmann studied the transport of hydrogen gas through a thin platinum metal foil. This was also prescient of the controversy. He was recognized as an intellectually expansive scientist. Bockris, who was two years ahead of him, was his lecturer in only one course. The two of them were looked upon as just a little intellectually wild, but they were also seen as especially promising products of the school.

After graduation, Fleischmann married Sheila Flinn, whom he had met three years earlier. They had three children, and by 1995 the family had expanded to a count of seven grandchildren.

One of Fleischmann's most noticeable traits dating from his school days is his ability to extemporaneously articulate what needs to be said even under the most trying of circumstances. This trait indicates an awareness of his larger circumstances as others see them, a rare gift. In the adversity that followed the Utah announcement, a single cross word never emerged from his lips; never did he slur those who would drive him out of America. He was an invited guest lecturer for the British Association for the Advancement of Science in August 1992, at Southampton. During this lecture, he was able to speak of the events of the previous three and a half years without rancor.

From Imperial College, Fleischmann went to Durham University.* His impact on the school was considerable. When he arrived, most electrochemistry was done by measuring the current and voltage applied to the electrolytic cell. Fleischmann brought to the laboratory much more in the way of instrumentation. This improvement was widely recognized, and led to a rejuvenation of the field. All in all, he distinguished himself during those years.

* Durham had two campuses in those days. The north campus was at Newcastle and the south campus was at Durham. Each had its own (independent) chemistry department. His assignment was at the Newcastle campus, and he recalls that he visited the Durham campus only once. These are separate schools today; the Newcastle campus has become Newcastle University.

At Southampton University, Sir Graham Hills, chairman of the department of chemistry, had the task before him of building up the chemistry department. He was looking for an outstanding leader for the electrochemistry position. He cobbled together a named chair and an annual sum of research funds sufficient to the purpose. He made an offer to Bockris, his first choice. By that time, Bockris had moved to the University of Pennsylvania in Philadelphia and declined the offer.

In 1967, Hills invited Fleischmann to accept the position of Faraday Professor of Electrochemistry. Fleischmann accepted. His charge, when he arrived at Southampton, was to build a world class electrochemistry group. Eight years later, an American named B. Stanley Pons would select Southampton for his postgraduate studies for exactly that reason, and he would be followed two years later by a New Zealander named Mike McKubre, who had also selected the school for that reason.

Fleischmann received many major awards that were available in the field during his years at Southampton. From 1970 to 1972, he was president of the International Society of Electrochemists. In 1985, while no longer employed at the University, though he continued to carry the title of professor, he became a Fellow of the Royal Society of London, the most prestigious honor for a scientist that England had to offer.

When financial support to the University was reduced by the government in 1983, Fleischmann took early retirement. However, his research did not let up. He maintained an active professional life consulting with his peers at Harwell, England (the British nuclear research facility), and at the University of Utah, Salt Lake City, with Pons.

The credentials of Fleischmann and Pons as electrochemists were excellent. Stanley Pons was a prolific author and head of the Department of Chemistry at the University of Utah. He enjoyed the recognition and respect of his peers. Martin Fleischmann achieved world recognition within his profession for his own contributions and for the high caliber of electrochemistry research at the University of Southampton.

An old topic with Fleischmann was that of loading deuterium into palladium to an extreme degree to see what might happen inside the lattice. It was one that he had been pondering for more than a decade before he first visited Utah. Each of the two chemists had done extensive work in surface catalyzed electrochemistry during their careers. It would be hard to select two scientists more qualified than they to try their cold fusion experiment. Their sophisticated development of the necessary calorimetry, along with modern data reduction techniques, gave them a surprisingly good control of their experiment. They prevailed when heat proved to be the principal signature of the experiment.

They published eight major papers on their measurements of anomalous power in scientific journals between July 1990 and 1995, and these have easily withstood the published criticism. These publications stand as a definitive statement describing the experiment they announced at the University of Utah in March 1989. With that, their initial experimental work in cold fusion research undertaken at the University of Utah was complete. In 1995, Martin Fleischmann, sixty-eight-years old, retired to his home in Tisbury, England, for a second time. In 1998, Stanley Pons retired to a farm in Provence, France.

Professionally, Fleischmann and Pons were dissimilar in many ways. Fleischmann, besides being a bit older, had about him a grandfatherly demeanor towards his graduate and postgraduate school students that they found enchanting. Also, he was singular, not only in the many honors he had acquired, but in his broad command of the several fields of his specialties. In discussions with associates, he usually offered the summation that moved the subject matter to where it should go next. He was the one to lay out the particulars of a research program to be undertaken. When it was time to decide what the next step would be, he usually carried the day.

Fleischmann and Pons were quite similar in one aspect. Each had learned from his patrimony how to contend with adversity.

Conventional Science

For Fleischmann, the topics of electrolysis and of hydrogen dissolved in metal were old ones. He was well aware that chemists had studied the nature of electrically conductive solutions for more than one hundred years. Even then, these electrolytic solutions were the subject of global, scientific controversy. He had used electrolytic cells throughout his career.

The cells use two metal electrodes in a liquid solution. Of the two, it is the cathode, or negative terminal that is of most interest. It is there that electrons emerge from the electrode and participate in chemical reactions on its surface. One of these reactions breaks down the water to make hydrogen, some of which enters the electrode, but most of it forms bubbles that rise to the surface of the electrolyte and escape. Certain useful chemical reactions can only happen on the surface of a cathode, where the electrons move out from the cathode surface and attach themselves to an atomic structure that is mobile. (Batteries are one form of this type of cell, using various metals and electrolytes.) Fleischmann and Pons's cold fusion cell was one more variation on an old theme for them. In this respect, their experiment consisted of the most conventional sort of well established laboratory technology.

The ability of particular metals to hold, or dissolve, enormous quantities of hydrogen was well known for a long time. This property was used industrially to purify hydrogen by separating it from other gasses that do not dissolve in those metals. The science of hydrogen in palladium goes back to 1870.

Two German experimenters, Dr. Fritz Paneth and Dr. Kurt Peters,[10] experimented with hydrogen in palladium in 1926 and ultimately detected helium that they believed had come from a fusing together of the nuclei of two hydrogen atoms. Their research techniques were sophisticated and ingenious. Within a year, however, they concluded that the helium atoms they detected came not from fusion, but had emerged from the inside surface of their glass containers. The helium they detected was merely contamination. The laboratory glass had absorbed helium from the air prior to the experiment and released it into the experiment.

After WWII, Paneth worked at the Durham campus of the University of Durham, England at the same time that Fleischmann started his career at the University's Newcastle campus. Fleischmann never met Paneth.

In 1927, John Tandberg, chief engineer at the Electrolux Corp. in Sweden, read of Paneth's work. He made a similar claim of fusion with an experiment involving an electrolytic solution and palladium metal, a direct historical precursor of the Fleischmann and Pons work. Tandberg believed he had achieved the creation of helium by means of hydrogen fusion, and filed a patent claim on it. The patent was refused, and nothing came of his work though he persisted in playing with it for several decades.

Fleischmann's interest in the peculiar properties of hydrogen dissolved in palladium was piqued in 1947 when he came across a 1929 paper that reported a fascinating experiment by Alfred Cöhn, professor of physics at the University at Göttingen, Germany.[11] Cöhn saturated one end of a palladium wire with hydrogen gas. He found that under the influence of a voltage placed end to end on the wire, the hydrogen inside the wire migrated along the length of the wire.

Cöhn surmised that, inside the metal, the hydrogen atom's one electron must drift away to join the other electrons that move about freely. The nucleus was left "bare," so to speak, a proton without an orbiting electron to give it atomic structure. The proton nucleus of that hydrogen atom was a thousand times smaller than the atom it had previously been part of and it carried a positive charge. When Cöhn placed a voltage across the ends of the wire, the positively charged proton migrated towards the wire end that was attached to the negative terminal of the battery. Cöhn concluded that when the hydrogen dissolved into the wire, the nucleus of the hydrogen atom was present as a proton.

To the young Fleischmann, it was a wonder. What possibilities might there be in manipulating the nuclei of hydrogen atoms inside palladium metal?

CHAPTER THREE

The Enigma of Discovery

It is no longer certain whether it was a fall day in 1983, or a spring day in 1984 when Martin Fleischmann and Stanley Pons talked shop while they hiked up Millcreek Canyon, a rocky gully that climbs into the foothills of the Wasatch mountains east of Salt Lake City, Utah. Fleischmann was retired from Southampton University, and had started to visit Pons regularly. He was granted the title of research professor in the chemistry department, University of Utah, as a visiting scholar. The relationship between them was both professional and personal. Fleischmann resided in the Pons's house for weeks at a time where they shared interests in skiing, hiking, and cooking. They co-authored dozens of papers on a wide variety of subjects while working together during those years.

Fleischmann's doctoral thesis addressed itself to the topic of hydrogen transport through platinum. His career involved measurement of surface chemical reactions in electrolytic experiments. At Southampton, he had gathered together the components for an experiment in the early 1970s wherein hydrogen gas would be loaded into palladium metal in extreme amount. He had chosen the deuterium form of hydrogen for this experiment to see if nuclear fusion might be triggered. His early attempt at the experiment was abandoned before it began for lack of time and attention. After 1983, Fleischmann once again had the opportunity to try it in his pretense of retirement.

He and Pons had discussed this experiment earlier. Now, while they climbed the barren canyon to its upper end, they started to define its parameters. As Fleischmann explains it, he first understood in 1947 that when one of the three isotopes of hydrogen dissolves into a metal lattice*, it ceases to be a normal atom. Unlike other atomic elements, it becomes just a nucleus.[1] This characteristic does not tell you what will happen, but it does give the experimentalist something to play with.

The discussion lasted during the hike, and through the following night in the Pons's kitchen. Fleischmann identified four experimental systems that might be tried and some of these had sub-system variations. In one of them, a potential was placed across a metal loaded with hydrogen gas. The second was the electrolytic cell that has become the subject of this account. (He does not talk about the other two.) They chose the second, the cell, to work with.

In it, the hydrogen nucleus is moved through a modest potential to obtain the equivalent of a large amount of compression inside the cathode electrode. The metal cathode is composed of tiny crystal grains of the metal, and each crystal is a molecular lattice structure. "Whatever you say," says Fleischmann, "you can achieve conditions in a lattice through electrolytic action which you could never achieve in any other way known."[2]

So the experimental concept was born. An electrolytic cell was designed where the nucleus of the deuterium isotope of hydrogen would be highly concentrated (loaded) in a palladium cathode in order to see what happened.† Experimental work was started in Pons's basement, but that proved an unsuitable place for many reasons. Before the end of 1984, their work was moved to what was then designated the basement level (now called the first level) of the north Henry Eyring chemistry building, Room 1113.

The Meltdown

An early experiment consisted of a one-centimeter cube of palladium suspended in a flask of heavy water containing dissolved lithium metal. Pons's son Joey, who did not have technical training, was a quick, intelligent helper, and he worked for his father as a sort of sorcerer's apprentice. By the late fall of 1984, the experiment had been running continuously for several months. At one point, Pons raised the current from its nominal rate of 0.75 amperes to 1.5 amperes, and at the end of the day, sent Joey to turn off the current. They left the laboratory for the night.

Joey came in the next morning and found the experiment in a shambles.

* The atoms in a metal are arranged, as in a crystal, in lines and ranks. This structure is referred to as a lattice. This lattice structure effects the behavior of the atoms, electrons, and protons when they move about.

† There is no implication here that this reasoning is correct. Many discoveries have been made by looking for one thing and quite unexpectedly finding something else of value.

Fleischmann and Pons reported in their Preliminary Note, ". . . a substantial portion of the [palladium] fused (melting point 1,554C), part of it vapourised, and the cell and contents and a part of the fume cupboard housing the experiment were destroyed."[3]

Kevin Ashley was a graduate student of Pons in the chemistry department. He witnessed the scene the morning after the meltdown. "This one morning I walk in, the door is open and Pons and Fleischmann are in the room with Joey. The lab is a mess and there is particulate dust in the air. On this lab bench are the remnants of an experiment. The bench was one of those black top benches that was made of very, very hard material. There were cabinets under one end of the bench, but the experiment was near the middle where there was nothing underneath. I was astonished that there was a hole through the thing. The hole was about a foot in diameter. Under the hole was a pretty good sized pit in the concrete floor. It may have been as much as four inches deep.

"What really surprised me," Ashley continued, "was that Stan and Martin Fleischmann had these looks on their faces as though they were the cat that had just swallowed the canary. They were clearly not displeased with this mess. They were happy about what had happened. I was rather surprised by this; very surprised by this."[4] Ashley was also able to identify the room in which the event took place as Room 1113 in the north Henry Eyring Building (HEB).

Other persons who were members of the chemistry department during these years have helped to identify the time at which it happened. The building of the south HEB was sufficiently completed during 1984 that the chemistry department was able to move its offices and laboratories over the Christmas holiday season of 1984 which ran from December 7 to January 2 that year. At that time, the cold fusion laboratory was moved into Room 1113. The "meltdown" occurred sometime during the next two months.

Fleischmann and Pons were both elated and chagrined by this event. They knew well how to compute the energy content in hydrogen escaping from palladium. The energy released was much too great to be accounted for by that phenomenon. If it was not a chemical effect then where could so much energy come from? They had succeeded with their little experiment; they also realized the risk they had inadvertently taken. They say they scanned the area with some sort of detector device to see if a dangerous level of radioactivity was present. They thought they saw some increase above the usual background levels, but they concluded that the nuclear process that delivered such high levels of energy was largely without radioactivity. That lack of evident radioactivity was parallel to their later claim of only a few neutrons.

After the accident, they modified the experiment so as to use a flat sheet or small diameter palladium rod cathode. These shapes avoided the center part of a cubic structure (the bulk), which tended to heat up much faster than the surface.

To continue this work in an orderly way, an experimental cell had to be designed. Would it be the size of a liter jar, or would it be the size of a thimble? Scaling was of importance, as it would determine costs. It would also determine whether any experimental effect would be large enough to be detectable. One would expect that primitive experiments were tried during these initial years to establish scaling, but there is no available record. As for costs, they could not possibly bring such an idea to any funding agency. "You expect to achieve nuclear fusion in a high school chemistry experiment?" They would be laughed out of the agency.

Would the design be a cell open to the atmosphere or would it be completely enclosed? The costs of working with a closed cell were avoided by operating with an open cell at a high electric current. Any effects from the two bubbling gases recombining together would be negligible. Gradually, the optimal design emerged. The choice of a Dewar (double-walled) vacuum flask was to prevent heat conduction out of the sides and bottom of the cell. Heat flow out of the cell would be by means of radiant emission to the bath water. A silver plating was added later to the inside near the top and continued down to below the level of the electrolyte. This limited heat loss through the top, and made the heat loss value less dependent on the changing liquid level. The quality of the vacuum was improved to a hard vacuum. The flask's shape was narrow and tall on the inside so that the bubbling action of the deuterium and oxygen emerging from the surface of the cathode and anode served to keep the electrolyte well mixed.* The Dewar's vacuum insulation and the effervescent bubbling action from the anode and cathode rods kept the temperature of the electrolyte sufficiently uniform to permit accurate heat flow measurements.

An overall design goal was to make the mass (amount of material) in the cell as small as possible so that temperature change would require a minimum of energy. That meant using a flask of small volume so that the heat source could more easily drive the temperature up. The cell would give out a strong reaction signature, and possibly enhance hidden reactions that might respond to rapid temperature change. A liquid electrolyte filled the flask above its silvered neck. The platinum anode wire was wound on glass support rods, and the palladium cathode was suspended at the center bottom of the flask. This geometry produced a uniform electrical pressure on the surface of the cathode. This pressure would force deuterium nuclei into the metal without allowing it to escape.

* The inside diameter of the silvered Dewar flask was approximately 3.30 cm, about equal to the length of a standard paper clip.

The Cell

Fleischmann and Pons decided on a high level of electrical current as a minimum level of activity for their cell: about 65 milliamperes for each square centimeter (cm^2) of cathode surface. They would drive the cathode hard. The objective was to override small error effects with a large experimental outcome. The electrolyte was heavy water with lithium metal dissolved in it. The cell is shown in Figure 3.1.

The thermistor (for temperature measurement) and the calibration heater (for inserting electrical heat pulses) are used to calibrate and monitor the rate of radiant heat transmission by the cell to the surrounding water bath.

To "run" the cell, the power supply's positive terminal is connected to the platinum anode and the negative terminal to the cathode. Ions, which actually constitute the current flow, move between the cathode and the anode through the electrolyte solution. When electric current flows through the cell, the water molecules break up into two gasses. Oxygen is produced at the anode (+) and deuterium at the cathode (−). These gasses bubble up to the surface and leave the flask. The energy that was put into the electrochemical reaction that separated the water molecules into the two gasses is carried away from the cell with the gasses. Many students have done similar electrolysis experiments in freshman chemistry classes. Electrolyte solution is added each day to make up for what was bubbled away during the previous twenty four hours.

Once the cell current is turned on, it ordinarily operates continuously day and night for weeks until the end of its life.* Experimenters often try to find an optimum adjustment to the current, a change of its amplitude instantly, or slowly, up or down, once or repeatedly. The purpose in this wiggling is to cause non-equilibrium conditions inside the cathode.

The cell flask is usually submerged up to its neck in a water bath that is held at a constant, lower temperature. This difference causes heat to flow out of the cell. In the examples that follow, the bath fluctuated by only ± 0.01C. Maintaining the temperature with that accuracy required careful preparations, but allowed the necessary energy measurements.

The most critical part of the cell was the surface of the cathode electrode. The excess energy that was claimed would be generated in or near its surface. Many researchers have experienced a buildup of unknown material on the surface of the cathode that the electricity could not get through, bringing the experiment to an end. These problems have always plagued electrolytic experi-

* The power supply often operates in a constant current mode in which the applied voltage is allowed to vary in order to maintain the desired current. The current, however, may be externally programmed to be set at different values as the experiment progresses.

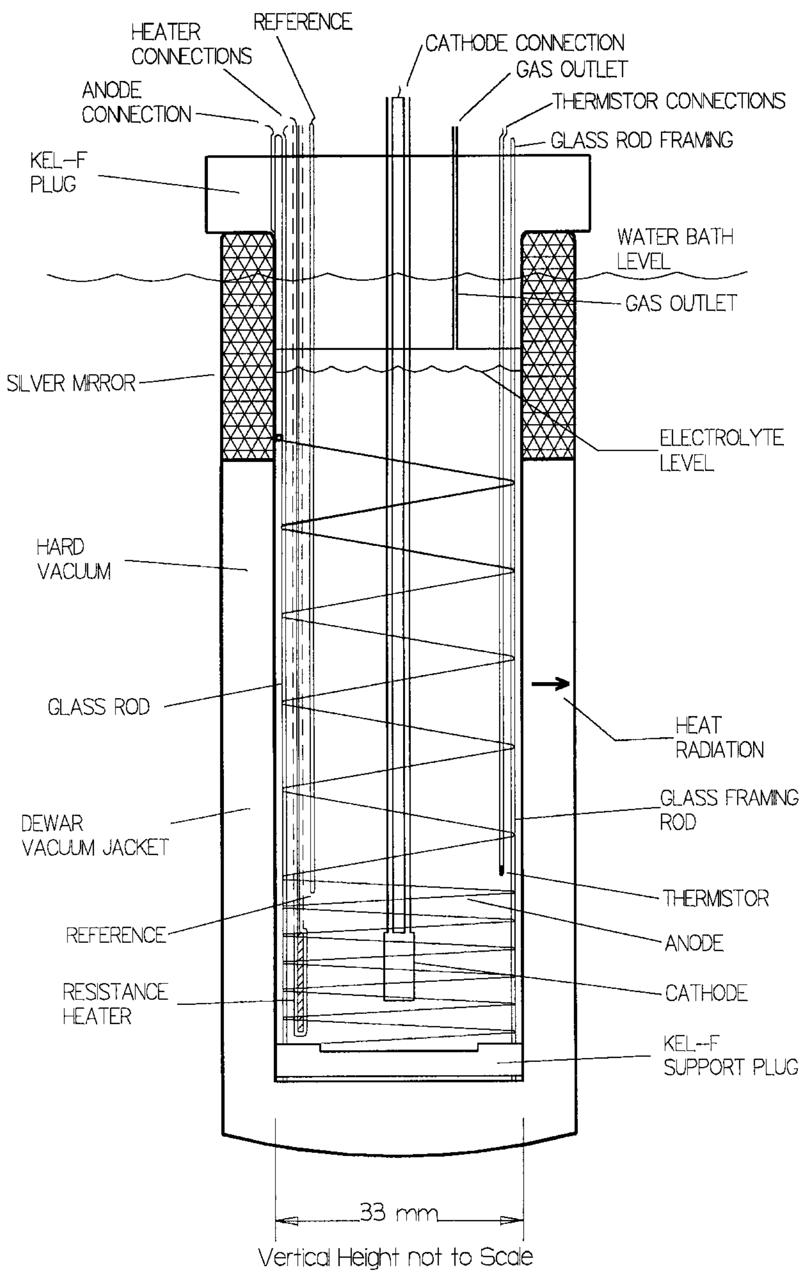

FIGURE 3.1 A Fleischmann and Pons cell schematic layout. Its height is foreshortened.

mentation. It took considerable laboratory skill to operate an electrolytic cell satisfactorily.

The cell design was greatly inhibited by the lack of adequate specification for the palladium itself. The metallurgical processing was a critical factor that determines what happened on both its surface and inside it. Control of the many characteristics of the metal was an exceedingly complicated matter and not well understood.

In the Fleischmann and Pons cell, 95% of the heat is removed from the cathode by radiation, and only 5% by conduction. The radiation is not ionizing radiation, but is *heat* radiation of the kind a person feels from a hot object. The radiant energy flows from each point of the electrolyte volume to each point of the outside water bath volume where it is absorbed. If the temperature of the bath and the cell are accurately known, then the transmitted heat can be accurately calculated.

The most significant question for planning the course of experimental work was, What would be a signature that something of unusual interest was happening? What should the experimenter look for as a result? If one performed an experiment, all the while looking for the wrong kind of indicator, then an important result might not be noticed.

Fleischmann and Pons expected to see neutrons—lots of them. They were convinced that they could measure heat to a few milliwatts (1/1,000 of a watt) of power.[5] They calculated from this estimate that the presence of excess heat would forewarn them of radiation danger. While one might measure both neutrons and heat, their resources were severely limited as their expenses were being met out of their own pockets. The experimental signature that they would look for first would be the anomalous (excess) heat flow.

An important point to remember is that this laboratory work was conventional. Building and operating electrolytic cells was such an established part of chemistry that it was a recognized sub-specialty, called electrochemistry, with its own professional organizations and publications. The Fleischmann and Pons cell was not a development on the fringe of science. These cell systems have long had a fundamental place in the scientific arcade of laboratory technology.

By the autumn of 1988, the two chemists were completely aware that their experiment generated an unaccountable amount of heat power. They had done enough in the way of limited experiments to feel ready to do laboratory work that was more formally structured. The final scale (size) and detail of the experiment had been set. Now, much more carefully documented experiments would be worthwhile.

One of Pons's graduate students, Marvin Hawkins, was put to work building not only the elaborately structured cell but the water bath in which it would reside. He assembled the operating electronics, and the instrumentation to measure time, temperature, and voltage, for each cell. By December 1988, the laboratory was functioning. Many cells were percolating in their baths. At this point, most data were recorded by hand.

Now the laboratory had a payroll to meet. Fleischmann and Pons submitted a proposal to the U.S. Department of Energy (DOE) for several hundred thousand dollars in September.* It landed on the desk of an administrative scientist,† who was in charge of projects that did not fit into the usual categories of energy research. He, in turn, sent it out to be refereed by other scientists. This was a form of peer-review to get opinions on whether the proposal was worth funding.

Steven E. Jones, professor of physics at nearby Brigham Young University (BYU), Provo, Utah, was one of those selected to referee the proposal because of his expertise in the field of what was previously called cold fusion. Jones and a physicist at the University of Arizona had co-authored an article just the year before in *Scientific American* (July 1987) entitled, "Cold Nuclear Fusion."

Jones's work offered two sources for the use of the term cold fusion. He had been experimenting with what was called muon-catalyzed fusion for some number of years. That fusion was done on an atomic scale at room temperature, and bears no import on this narrative. In 1985, he experimented with an electrochemical reaction. He knew that the interior temperature of the Earth was hotter than could be explained by contemporary theories. In addition, he had detected helium-three in gas given off by volcanoes. These considerations meant that something was creating that helium inside the Earth and that it could only be a nuclear process of some kind. So he devised an experiment.

He placed two metal strips in a baby food jar and connected them to a battery. He added a mixture of earth chemicals (salts) for the electrolyte. At the time, he believed he detected some neutrons emanating from it. This claim also had the name of cold fusion.‡ In December 1993, he retracted his claim that neutrons were being emitted by some process that was occurring in the jar. At no point did he attempt to measure heat.

In the fall of 1988, there was no way for one to judge the conflicting uses of the term cold fusion, nor was there a way to evaluate the history of electrolytic experiments on the part of both BYU and the University of Utah. However, there was an exchange of information between them, and then a race for priority of whatever discovery might have occurred. The competition created a bitter conflict between the two universities, as well as between Jones and the chemists. These personal antagonisms lasted for a decade and more.

* The proposal was never funded. Without a grant, they somehow persevered.

† Ryszard Gajewski.

‡ He looked for neutron particles instead of looking for heat energy, because the considerable difficulties in measuring particle radiation were familiar ones and because the considerable difficulties in measuring heat energy were unfamiliar.

Nuclear Measurements

By the winter of 1989, Fleischmann and Pons knew their neutron radiation level was astonishingly low when compared with the amount of heat released and they set about measuring it. One consideration that had to be taken into account was that their experiment was located in the chemistry department. Any attempt to set up nuclear detectors presented a problem as knowledgeable staff would recognize the equipment's purpose.

Serious nuclear particle counting in a routine chemistry experiment would raise cries of incredulity, not unlike those that accompanied the formal announcement of their work. What on earth is going on here? Every one knows that chemical experiments do not involve nuclear behavior. "Whose setup is this? Explain yourself!"

If they had proceeded in this direction, the whole university would have been looking over their shoulder to see the results. Opinions would be offered as to whether the university's resources were being well spent. That would necessarily absorb Fleischmann and Pons's attention and implicitly place an outer limit on the experiment's duration. One can not carry the day in this type of discussion without producing results. The project was so profoundly radical in its conception that peer exposure could not be contained for long. These questions would rise again within the larger world of the state legislature. Was the university using wisely the resources given to it?

Yet, to actually explain what they were attempting would expose the two scientists to a review of their plans by their peers in the chemistry and physics departments. Obviously, some would offer a loud declaration that the experiment was ridiculous. There would inevitably be members who would not be able to contain themselves with the thought of what the two were attempting. Others would be fearful of damage to their own reputations, or to the reputation of the department by association with it. It could damage the university's reputation if outsiders learned what was going on.* Any involvement of their colleagues would likely make the experimental effort completely untenable. They would have had to abandon it and accept the waves of ridicule that would follow them afterwards. Yet, without such help, they had to get some reasonable measurements of nuclear products.

By January 1989, they had been contemplating the existence of an unex-

* An excellent example of this type of response occurred at Texas A&M University in 1994. The University accepted a fully funded contract for Bockris to work in collaboration with an amateur experimenter who had been replicating various medieval alchemical experiments purported to convert base metals into gold. These experiments were carried out over a year and then terminated. (The amateur later was sent to state prison on some unrelated matter.) Bockris suffered at the hands of his colleagues for doing this work. An attempt was made to have the faculty formally remove his title of "distinguished," and ridicule was always just out of earshot. There are those who believe he ruined an otherwise stellar career by his involvement with this nefarious character.

plained heat energy source with negligible particle radiation for about four years. They felt altogether certain that the heat existed and assumed the source must be some sort of nuclear process as no other possibility could be imagined. They had done enough primitive radiation tests to feel sure that their excess heat was neutron free when compared with text book nuclear behavior.

Nuclear measurements were difficult for them at low levels of neutron radiation. While maintaining secrecy, they tried to measure what particle radiation there was. They managed to make something of a mess of it. Pons had gone so far as to ask the head of the physics department how to detect neutrons. The department at Utah did not do plasma research and he was referred to the Los Alamos National Laboratory (LANL).

Pons turned to a health physicist on the safety staff of the university and was provided with a detector for gamma-rays. These rays are emitted when a water bath is irradiated with neutron particles. Neutrons emitted by the cell would be stopped by the water bath, which would then emit gamma-rays for the detector to measure. The detector would also collect gamma-rays from natural sources that were present in the concrete of the basement walls and floors. I have not been able to determine exactly what mistakes they made with the setup and operation. They followed a false signal and obtained and published unexplainable data. The difficulty was greatly embarrassing to them after they went public in March when experienced nuclear physicists discovered the chemists's error.

Then they changed the detector type. They were using one of high sensitivity (sodium-iodide) and they changed to one of high resolution (germanium). By measuring the sought for gamma signal over a long time, they finally detected what they believed was a gamma-ray signal from their experiment.[6] These results were published in June 1992 and can be seen in Figure 18.1, page 256.

Fleischmann and Pons maintained that their tritium measurements were valid, but they never reported their results in a formal paper.[7]

During those winter months of 1989, Fleischmann and Pons collected data as fast as they could. Some was collected by hand and later it was done on computer. Pons brought it home for analysis. That raw data has never been released. It was the data selected from those experiments that they submitted for publication in the Preliminary Note. It was peer reviewed, requested changes were made, and the final manuscript was received with an imprinted date of Wednesday, March 22, 1989, the day before the public announcement.

Their seminal, full-length publication on excess heat energy used data from experiments that continued through the summer and fall of 1989. It was submitted in December and published in July of 1990, in the same journal.[8] They later defended that paper in a second article published in July 1992.[9]

They were also involved in the institutional conflict between the University of Utah and BYU during the winter of 1989. There were visits to BYU by the technical and administrative officers of the University of Utah. Initially Fleischmann and Pons wanted to postpone any publication or announcement of their work to allow themselves another eighteen months of experimentation. Too many people were informed of this secret research for such a course to be practical and the secret was too momentous to contain.*

Jones made a commitment to speak at the meeting of the American Physical Society (APS) scheduled for May at Baltimore, Maryland. The APS was the principal professional organization in the United States for physicists, so Jones's speech would be noticed. Both BYU and the University of Utah agreed to preempt that presentation by simultaneous publication in *Nature* magazine without knowing if the magazine would accept and publish either paper. The University of Utah, in turn, preempted that agreement by announcing on March 22 that it would hold a press conference the next day. But late that same day,† the *London Financial Times* preempted the press conference with an article. The newspaper announced in its first sentence, "Two scientists will today formally announce that they have carried out controlled nuclear fusion in a test tube."

If, indeed, this experiment demonstrates the discovery of something, what could it be that was discovered? The sixteenth-century captain sailing on the high seas might come upon an uncharted landfall, circumnavigate it to conclude an island was discovered. Columbus after crossing the ocean sea realized he had not found the culture of the orient. In his lifetime, he never learned what it was that he had discovered. Similarly, Pierre Curie never learned that he had opened the door to nuclear power. The enigma of discovery is that often the discoverer never quite learns for certain what it is that he has discovered.

If the Fleischmann and Pons observation of excess energy flow can be validated, then that piece of observational science, standing alone, is sufficient to begin a new area of scientific study. That observation, *without an understanding of its cause or its source,* is of definite interest to science because it appears to contradict the physical law that says energy can be accounted for. It is worthwhile now to look at the Fleischmann and Pons experiment in quantitative terms. After that, the critics will do their worst with what was claimed by the two chemists from Utah.

* I do not mean to imply here that what they had to announce was true. It was momentous *as presented,* and that was an enormous difficulty for all to contend with at the time.

† London time is seven hours ahead of Salt Lake City time. If we are following events in Utah, the *Financial Times* announced its message on the previous day, Wednesday, March 22, 1989.

CHAPTER FOUR

A Power Burst

A power burst comes on slowly, like a sneeze. The buildup takes weeks. The burst itself may stay for 48 hours in order to complete its cycle.

Imagine a modern chemistry laboratory, a room that is the size of a two-car garage crowded with wide, sturdy workbenches. At the place where the benches butt against the walls are steel shelves rising to a height of seven feet. The shelves hold electronics, power supplies, instruments, computers, and printers. On several of the benches are open baths of water, each a yard square and a foot high. Suspended over each bath are steel supports holding one, two, three, or four glass flasks, each almost entirely submerged in the bath. In each flask, or cell, is one variation of the Fleischmann and Pons experiment.

Imagine further, that sitting in front of the bench is a tall, lanky, bearded scientist by the name of Ed Storms. He was described aptly by one writer as the Hollywood image of the Biblical Moses. On his stool, Ed sits somewhat folded up while watching the cell's instruments for signs of activity. He watches for hours, for days. In his words,

> I put the cell into the calorimeter and it went through a few weeks doing essentially nothing. Then all of a sudden it just took off. It just started making significant heat. I was as surprised as anyone, let me tell you. You know, you sit there in front of the apparatus forever, and think, "this is all so much nonsense." This isn't really real. This waiting goes on for weeks, maybe months.
>
> Then all of a sudden the readout device shows the cell has started taking off. And you say, "Oh-oh, what's gone wrong now."

> You start playing around with everything you can think of that might have gone wrong to see what has happened. After a while it suddenly dawns on you that nothing is wrong. This is what it is supposed to do.[1]

This episode is one scientist's tale of his feelings and responses as he bears witness to an excess heat event, an anomalous power *burst.*

The experimental cells soaking in their yard sized baths depict one kind of calorimeter. This "open," isoperibolic, type is important because it proved particularly suitable for these purposes.* Power in the cell will dissipate by heat *radiation* to the bath and by a little inadvertent heat *conduction* to the air and bath. Energy will also be dissipated through the wires and tubes emanating from the cell's top. The two gasses given off by the cell carry off some heat energy, and the liquid electrolyte will evaporate from its surface causing a cooling effect.

When the equipment is working well and the cell is not generating excess heat, the experimenter should measure power into the cell as equal to power out, within plus or minus one percent.[2] Absolute levels of accuracy will range from 40 down to 2 milliwatts. This condition gives a set of base line values in preparation for the experiment.

The Experimental Observable

The following example is taken from an early report. A burst excess heat event is displayed in the two illustrations, Figures 4.1 and 4.2, both taken from the same anomalous event.† The experiment included both the generation of excess heat over a month or more, and a burst event that lasted only a few days.

The part played by the control, or blank, cell was discussed earlier. The two directions in which one might seek control had become a source of confusion. Either cold fusion (literally) or anomalous power may need a control reference experiment and each requires a differently designed control cell. For this reason, in the experiments that follow, Fleischmann and Pons have used two kinds of control cells: those that use plain water in place of heavy water, and others that use platinum cathode rods in place of palladium rods in heavy

* The isoperibolic calorimeter identifies all paths that carry heat out of a cell and measures the heat loss from each of them.

† The article from which these figures are selected was submitted for publication December 21, 1989 and was published on July 25, 1990. It is the definitive paper by M. Fleischmann, S. Pons, M. R. Anderson, Lian Jun Li, and M. Hawkins describing anomalous power generation. (In that paper, Anderson's name is mistakenly given as M. W. Anderson.)

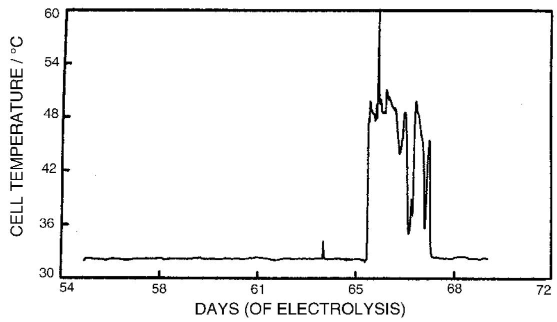

FIGURE 4.1 Fleischmann reported the temperature of the cell's electrolyte liquid plotted against the number of days of continuous electrical excitation.

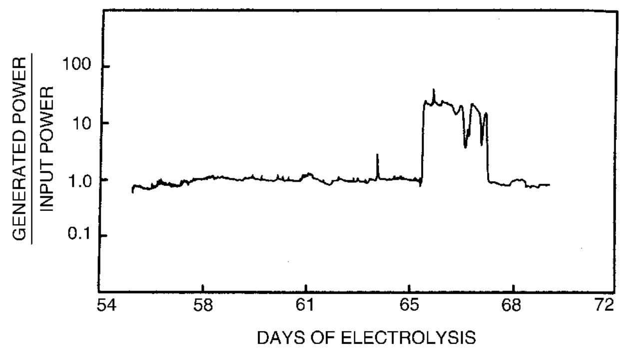

FIGURE 4.2 Fleischmann reported the energy flow rate after the start of electrolysis as displayed for days 54 to 72.

water. Equally important, direct comparisons between control cells and heat generating cells will display differences and similarities that need to be explained.

A Power Burst

In Figure 4.1, the tracing shows a temperature burst.[3] The burst appears as a temperature excursion upwards from the base line. Some experimenters have concluded that the temperature excursion is a necessary part of the energy burst event, that it in some way *enables* the event to unfold.

The vertical scale is marked with cell temperature in degrees Celsius. The horizontal scale shows time increasing from left to right, labeled in days. The tracing starts near the lower left-hand corner. From day one when the electric current is applied until day 55 where this tracing begins little of interest happened, so those days are not shown.

The shape of the tracing reveals some interesting excursions. There is a flat extent to the tracing starting at day 55 that indicates a temperature of thirty-two degrees. The brief positive impulse of temperature that appears at approximately 64 days has been mentioned by Fleischmann, McKubre, and Storms as a precursor of the large temperature excursion that follows.* The burst of temperature starts shortly after day 65 and lasts for about 48 hours. During that time, the temperature varies widely, but it stays near 48C for much of the time.

In Figure 4.2, the tracing shows the burst of *power* for the same experiment. It is labeled as a ratio of generated power to input power.[4] The value of the power generated divided by the power input is drawn on the vertical scale.† Note the scale: the base line is approximately at a value of one, where input and generated output are equal.

The excess generated power is relatively small from the start of the experiment until day 55. This interval does not appear in Figure 4.2. The generation of power equal to the input power starts at day 55. By that time, the generated energy from day one is about one megaJoule‡.

The generated power, Figure 4.2, is shown to be about equal to the input power during the period of 240 hours from day 55 to day 65. A burst of power occurs between days 65 and 68. For about 48 hours, the ratio of the generated power divided by input power is approximately 20. Afterwards, the cell returns to its ratio of approximately one. The cell is turned off at 70 days.§

How much energy does this add up to? The energy generated is computed by adding up the power from the beginning of the experiment. The total amount is just under four MJ.** This amount of energy is equivalent to approximately one kilowatt-hour of electricity.[5]

The amount generated during the 48 hour (2,880 minutes) burst is approximately two MJ or about one half kilowatt-hour. This is equivalent to a

* Fleischmann refers to this temperature impulse as "the onset of positive feedback."

† Power input is the voltage (less 1.54 volts) multiplied by the current.

‡ One joule is equivalent in energy to one watt-second, that is, one watt continued for one second; 3,600,000 joules (3.6 megajoules or MJ) is equivalent to one kilowatt-hour of electricity.

§ The Dewar flask had a vacuum of about 10^{-6} Torr. The palladium cathode was a 0.4 diameter x 1.25 cm rod, 1.57 cm^2 in charging area, and 1/2 cubic centimeter in volume; the liquid electrolyte concentration was 0.1 Mole of lithium deuteroxide (lithium dissolved in heavy water). The current density was 64 milliamperes/cm^2 and 100 ma actual current. The bath temperature was 29.87C.

** The total *specific* excess energy for the experiment was seven and a half million joules. The specific energy is the energy/cubic cm of cathode. Where the cathode is 1/2 cc in volume, specific energies will be twice the actual energies.

1,500 watt electric stove burner running on a "high" cherry-red color setting for approximately 27 minutes. Given the small size of the cathode—it is 1/100 the volume of the stove burner—this is an enormous amount of energy. If that heat energy were not removed as rapidly as it is generated, it would quickly vaporize the cathode.

To better visualize this amount of energy, consider operating the 1,500 watt stove burner from a chemical source like a battery. A typical 12 volt automobile battery is one of the most compact electrical energy storage devices known. One with a 60 ampere-hour rating holds enough chemical energy to operate that burner for 29 minutes, compared with 27 minutes for the cell power burst. Roughly speaking, the burst generated the same amount of energy that is stored in the automobile battery. Notice how little chemical material there is inside the Fleischmann and Pons cell compared with the size and weight of the battery. Chemical storage can be discarded as a possible energy source in this experiment.

The total generated energy over 75 days was 3.75 million Joules of energy. "It is inconceivable that this [energy] could be due to anything but nuclear processes."[6] This judgment stands firm after twelve years. During this time, no one has suggested in the peer-reviewed literature how that amount of energy might be provided in a Fleischmann and Pons cell by other means.

Note that the cell was operated for about ten weeks to thoroughly investigate its potential for activity and that the most interesting action started at seven weeks. At this rate, approximately three experiments can be completed in one year if the results of one experiment were to inform the design of subsequent experiments. From the beginning, it was clear that cold fusion experimental work was enormously time-consuming.

Many experimenters found that they did not have the laboratory skills needed to keep the cell running for more than two weeks. The electrodes gradually become covered with "foreign" matter. A description, circa 1883, complains that "the formation of decomposition products on the electrodes . . . was the most vexing problem of electrolytic measurements and the main source of experimental errors."[7] The Fleischmann and Pons cell derives from a hoary practice more than a century old.

Another description says, "I can attest to the fact that [in surface catalyzed reactions] a very minute fouling of the surface can drive a reaction in a totally opposite direction than what you thought."[8] A single monolayer of contaminant on the cathode might disrupt an experiment. The surface may show more than one crystal face of the metal with different reactions possible on the different faces. The most likely cause for the variability of experimental outcomes is the variability of the cathode surface.

Considerable skill was required if errors in measuring heat were to be limited to acceptable levels. In cold fusion laboratory practice, it was heat *flow* that was measured. This was an even more demanding skill.* Fleischmann and Pons generally followed the practice of using current in excess of 64 milliamperes for each square centimeter of the cathode surface to operate their cells. The electrical energy used to separate the oxygen and deuterium in the water molecule was allowed for when they calculated the energy balance of the cell. (If some portion of the gasses recombined, the energy released would appear as pseudo excess power.) Some oxygen or deuterium gasses are inevitably dissolved in the water where a part of them might recombine. These effects of recombination were kept to negligible proportions by operating at high current levels.

As explained in Chapter 1, p. 11, recombination of the effluent gasses was measured as less than 1% in the experiment plotted in Figures 4.1 and 4.2. By the end of 1994, there was nothing in the scientific literature to undermine that statement, nor was there by 2001. There was a claim in one 1995 paper that significant recombination was found when a cell was operated at the minuscule level of 100 microwatts (0.0001 watts).[9] That such recombination occurs at exceedingly low levels is well known to scientists working with electrolytic cells.

Some experimenters measured the amount of water added every 24 hours to the cell and found that it corresponded to the amount that would be electrolyzed by the total current through the cell, which was accurately known. Others measured the amount of gas leaving the cell. Recombination of the gasses as a potential source of error was well under control.

Skeptics assumed that as heat measuring techniques improved, the measured excess energy would incrementally decrease in order to remain within the calorimeter's margin of error. By the end of 1994, it could no longer be argued that the best claims of anomalous power were due to faulty calorimetry.†

* There may be some concern by practicing scientists that the computation of the output heat flow from a cell requires the subtraction of two large numbers. That circumstance is always undesirable. Small fluctuations in the two large numbers can make the difference number meaningless. While this may have been a difficulty, it is one a professional scientist takes in stride. By giving attention to the accuracy and precision of the large numbers, the difference becomes meaningful.

Also, it should be mentioned that as larger power levels were achieved, some of the difference numbers become themselves large numbers.

† Scientific methodology places the same intellectual discipline on the critic as on the experimenter. If a skeptic wishes to challenge this claim, that argument should be accompanied by a corresponding paper published in the peer-reviewed literature for perusal by the scientific community.

Energy Conservation

One of the venerable scientific "laws" is that of conservation of energy. It states that energy can be neither created nor destroyed. This conservation principle includes the conversion of mass to energy in nuclear reactions.* As a mechanical or chemical process unfolds, the amount of energy present can be determined in principle at every step. From the beginning to the end of the process, that amount of energy should always be the same value after taking into account additions and deletions of energy from outside the process as well as additions from nuclear sources.

Scientists usually arrange their formulae so that when they finished accounting for the energy, the formula's value calculated to zero, the pluses balancing the minuses. If, when the formula is evaluated, its value is above zero, this signifies more energy is present than can be accounted for and a minus sign signifies less energy. If the calculation does not "zero out," then the experimenter has overlooked some part of the energy flow. This zeroing out of energy serves as an excellent *control* for the Fleischmann and Pons experiment. An inert piece of palladium for the cathode served this purpose well because they could be electrolyzed for weeks without giving the slightest indication of anomalous behavior.

The energy conservation law is one of the oldest and most revered in science. Any *validated* experimental outcome that challenged the certitude of the law would demand the attention of the scientific community. The scientific community could not rest until that challenge was resolved.

No one working in the field of cold fusion suggests a contradiction of that principle. When more energy comes out of an experiment than can be accounted for, which is the basic claim made in March 1989, that *apparent* violation of the law demands the most conscientious attention of the scientific community. That challenge to the law of conservation of energy constitutes the principal source of motivation for those who work in the field. Scaramuzzi addresses this point, ". . . there are results that are real, for example, the excess heat and the nuclear ashes do exist, in spite of the lack of reproducibility and of all the difficulties . . . If they were not real the field would have been abandoned many years ago."[10]

* In chemical reactions, energy is only converted from one kind into another kind, the total amount remaining constant. This characteristic of the total being a constant amount is called the principle of conservation of energy. Einstein's famous equation $E = mc^2$ allows calculation of the energy (E) equivalent of mass (m), where (c) is the speed of light in a vacuum. In nuclear reactions of fission or fusion, a small portion of the mass may be converted into substantial amounts of energy. This comprehensive use of the term conservation of energy is intended throughout the book.

Scientific protocol requires that the measurement of anomalous power be a correct measurement *in all respects.* To illustrate mistaken protocol, the following quotation is typical. "Although the . . . experiment is considered by many advocates to be the premier evidence for excess heat, no nuclear reaction products were reported!"[11] The history of scientific protocol shows that *none* need to be reported for the observation of excess heat to be accepted into the inventory of scientific observations. While it would be nice to know of any nuclear products, their lack carried no indication that the heat measurements were erroneous. In the Utah announcement, the hypothesis is that the products are unknown and therefore the information is inaccessible. Scientific inquiry would be reduced to a benighted state if it were acceptable to dismiss the heat measurements by pointing out the lack of nuclear products. Nothing that was observed by scientists would be considered worth scientific study until after its cause was firmly identified. But without scientific study, little in the way of cause could ever be found.

The total amount of energy that the cell consumes and generates can be measured during the experiment from start to finish. It gives a measure of the experiment as a useful energy generating device. Fleischmann and Pons never claimed the experiment as a useful device for the delivery of energy to some economic purpose.*

Consideration by the skeptics of the energy sum of the active cell over time would imply a large energy storage capability within the cell. Storage is an unrewarding concept. The energies involved are greater than 50 electronvolts per palladium atom. This amount of energy is sufficient to vaporize the cathode if an attempt were made to store it chemically in the cathode. Alternately, an hypothesis of temporary, in-out, storage in the atom's nucleus is unheard of. Nuclear storage mechanisms have never been demonstrated. Additionally, the "in" part of the cycle would be detected by the calorimeter. It would jeopardize the credibility of this text to allow the hypothesis of an unknown energy storage mechanism of suitable capacity as the source of the anomalous power. By comparison, the Fleischmann and Pons hypothesis of an unrecognized or unknown nuclear source is most reasonable.

The measurement of anomalous heat power is necessarily the result of one of four possibilities. There is an undiscovered *error* in the heat measurement, there is some yet unnoticed or unknown *chemical* source, there is some well understood *nuclear* source, or it must be hypothesized that there is some unknown *nuclear* source.† Beyond those four, there is only consideration of

* I omit here off-the-cuff predictions of commercial readiness that have been offered by Dr. Pons from time to time. As a student at MIT 50 years ago, I learned to take the visions of easy commercialization by academic scientists with a grain of salt.

the abrogation of the law of conservation of energy in the fashion described by the philosopher of science Karl R. Popper, whose logical impact we review in Chapter 8, page 106.

The possibility of undiscovered error in heat measurements is adjudicated by requiring replication of an experiment in an independent laboratory, by inviting thorough review of the procedures by experts, by emphasizing variety in cell and calorimeter design, and with the use of control experiments.

The alternate to error is that nuclear fusion might be occurring. Neutrons have not been observed in sufficient quantities in the Fleischmann and Pons cell to ascribe the excess energy to conventional fusion. Fleischmann said at the announcement, "Well, the interesting phenomenon about this is that the rate of generation of tritium and helium-three is only one billionth of what you would expect if the fusion reaction [causing the heat] was one of those experienced in high energy physics. So we have a relatively low rate of production of neutrons."[12] And further, "It is evident that [recognized] reactions are only a small part of the overall reaction scheme and that other nuclear processes must be involved." That last possibility is the one of interest. It is that there occurred in the cell a nuclear reaction of some unknown kind.[13]

The category of "other nuclear processes" mentioned above does not permit the inclusion of miracles. One skeptic had even suggested in all seriousness that three miracles were required to explain anomalous heat. There were at last review no miracles registered in the physics textbooks. After the exploration and understanding of cold fusion claims is complete—and this writer does not expect to see that accomplished in his lifetime—there still will be no miracles incorporated into the physics textbooks.

Experimental methodology also concerns itself with the part played in science by the *limited* experiment. For example, a research laboratory is funded from year to year by some agency to do research. Each year (or two) the laboratory submits proposals for further research. Generally, the sponsoring agency must see interesting results from year to year to justify continuance of the funding. In a case like this, how does the laboratory plan the next year's proposed work? To merely recommend a particular direction of experimentation would be too risky. If that research plan produced no significant or interesting results during the subsequent period, it would be too late to try another research direction. The laboratory's future funding would be put at risk.

Some laboratories are institutionally funded from decade to decade. If the

† There are other theories. They are so speculative that I have not tried to encompass them in the text. Briefly, Randell Mills's theory is described in Chapter 21, p. 296, others favor a theory of a vacuum energy source.

output is poor, then the laboratory personnel are changed to remedy the situation. Other laboratories, however, need to survive from one grant to the next.

In those laboratories, that risk is finessed in the following way. During the current year, the director tries those experiments to be proposed for the subsequent year. He performs what I call "limited" experiments. There may be a lack of standard controls, a possible lack of statistical verification, no proof that contamination was not involved, little documentation, and so forth. But most emphatically, the director must be sure his outcome is right; the laboratory's future depends upon it. Those experiments that produce an interesting result are then proposed for the subsequent year.

So there exists in practice a double standard of laboratory work: one standard that is sufficient to persuade the experimenter himself, and a higher one that is sufficient to persuade his peers. The first category provides direction to the individual scientist. Scientists who do limited experiments with such a high degree of expertise that they come up with the right result have a substantial career advantage over those who can not. The second category of laboratory work consumes much greater amounts of time and resources, but it produces publishable results. I label the experiment done only for the experimenter's benefit the limited experiment.

Acceptors

Some skeptics mistakenly referred to scientists who work in cold fusion research as "believers" or even "true believers." The terms were derogatory, and they were meant to be. They implied that commitment to the field was religious in its nature, a leap of faith. Unfortunately, writers of science at the broad-audience scientific journals also adopted the use of this slur.

As the episode wore on, there were an increasing number of scientists who *accepted* the confirmations of excess heat generation that emerged in 1989 and 1990. For them, the Fleischmann and Pons experiment had hit upon something of scientific interest.

This acceptance was entirely independent of concerns with the lack of reproducibility of the experiment, the frequent failure of independent laboratories to obtain excess heat, and the largely evanescent attempts to find a nuclear theory to support the claims. These *acceptors* looked at the successful scientists, their experiments, the instrumentation, and the resulting data. When they found the measured anomalous power to be scientifically credible, they became, not believers, but acceptors.

Those acceptors of anomalous power considered the lack of experimental reproducibility a hindrance. It greatly reduced experimental efficiency. Many tries were necessary to get one cell that worked. Fleischmann and Pons obtained 100 percent reproducibility in their laboratory at the University of Utah during the winter of 1989. Their work had used palladium all taken from the same production lot. On the morning of the announcement, Marvin Hawkins, the graduate student assistant, set up four new cells. In doing so, he used up the remainder of the batch of palladium that had always worked. None was saved for later analysis, as it was not yet clear that batch variation would be a substantial hurdle. For a conference in October of that year, Fleischmann presented a set of 31 active cells of which 23 were reported as generating more than 20 mw of excess heat, and 13 other cells were control experiments that were not expected to generate excess heat.

Other scientists, who were successful in generating anomalous power, found they could only do it occasionally. Some of them found that performance depended upon the particular sample, or lot, of the palladium metal used for the cathode element just as did Fleischmann and Pons. So their early laboratory work had a significant element of luck in it. But it was not dumb luck; it was smart luck. The resourceful experimenter learned to isolate, identify, and deal with the materials variability. Other experimenters went for a year or more in a "dry spell" when the palladium they purchased did not work. During this hiatus, the metallurgy of the palladium cathode constituted a significant part of the field of study. It was also the most secret part.* Significant progress in the metallurgy of the cathode metal was considered to be especially valuable and proprietary information. In general, it was not published.

The generation of anomalous power was the signature of a successful experiment. That success involved an *apparent* violation of the law of conservation of energy. An explanation or interpretation may properly be demanded, and eventually one will be found—no miracles are allowed. How close was the field of cold fusion research to an answer?

Fusion is difficult to bring about. With two-body reactions, extremely high particle velocities are needed to bring two deuterons together close enough so that they fuse. There is an international effort to build a device called a *tokamak* to accomplish fusion. Such a process is referred to as "hot" fusion. Its development has been a slow and expensive project, and any potential for commercial operation remains out of sight.

The tokamak reactor was the reference point when reporters referred to the Fleischmann and Pons experiment as "kitchen chemistry." This colloquialism led the public, and many scientists, to assume the experiment was easy. It

* Galileo, while trumpeting his new telescope widely, kept secret his method for grinding Europe's best lenses.

was not. It had four difficult stages to be mastered: the continuous operation for months without "gumming up," the generation of excess heat power, the design of a wideband calorimeter system to measure the heat flow even in the case of a rapidly changing temperature, and the achievement of one percent accuracy.

With this discussion of an example of the generation of excess heat both in a burst and in an extended interval of many weeks, we turn our attention to the assault and criticism directed at this episode by orthodox scientists.

Part Two

CRITICISM

CHAPTER FIVE

Baltimore

Physics was introduced into the university curriculum at the turn of the twentieth century to round out a liberal arts education. Its growth in the university stagnated because physics was an experimental science that required expensive facilities. The development of physics teaching and research facilities in the United States prior to World War II inched along when compared with the postwar period.

The federal government's influence came to dominate physics research in 1941 and remained as a large, continuing presence. The Manhattan Project was immense, possibly the largest single program of any government in its time. The postwar federal government greatly influenced physics research with its weapons research investment and it branched from there into commercial nuclear power and particle accelerator facilities. The government also undertook development of nuclear fusion power as a open-ended research program. These projects offered lifetime career employment for those with a Ph.D. in nuclear or plasma physics.

Long term, well-funded government work brought with it considerable benefits for those so employed: the time to devote to national professional organizations, the time to prepare and present papers for publication, to travel to international meetings, and so forth. In many ways, a career in nuclear physics was not unlike holding an academic chair at a prestigious university.

Those benefits provided the influence whereby many physicists found appointment to government advisory roles. This dual support and participation enabled the community of American physicists to assume a powerful voice in energy policy development in the White House* and in the Department of Energy (DOE). When energy policy became centralized under the DOE during the 1970s, it was assigned nuclear weapons development, nuclear power responsibilities, and direction of most of the national scientific laboratories.†

This paramount role of physicists in the American establishment was best described by Daniel J. Kevles, a professor of the history of science at Caltech, in his 1995 book, *The Physicists: The History of a Scientific Community in Modern America.*‡ Six brief quotes will give the flavor of the physicist's position in American public life during the latter half of the twentieth century.

> . . . this generation was dominated by physicists who seemed to wear the "tunic of Superman" in the phrase of a *LIFE* reporter, and stood in the spotlight of a thousand suns. (p. 334)
>
> . . . after the atomic bombings . . . physicists of the Los Alamos generation became a kind of secular establishment—with the power to influence policy and obtain state resources largely on faith and with an enviable degree of freedom from political control. (p. ix)
>
> Whichever side they took on issues of arms control and defense, physicists remained honored and empowered because they remained essential in determining the shape and capabilities of American national security. (p. ix)
>
> It was a time when Americans ranked nuclear physicists third in occupational status . . . ahead of everyone except Supreme Court Justices and physicians . . . (p. 391)
>
> . . . much of the Los Alamos generation's leadership counted the Kennedy White House as its own. (p. 390)
>
> . . . Whatever history might conclude, in the mid-1960s American physicists headed a community of scientists who . . . had collectively become "something very close to an *establishment,* in the old and

* The selection of the individual Presidential Science Advisors followed a different process that resulted in a greater diversity of disciplines including electrical engineer, physical chemist, and geophysicist. The later rise of government sponsored medical research has matched the historical support for physics.

† One exception was the Office of Naval Research which had been established by a separate item of Congressional legislation in 1922 at the behest of Thomas Edison.

‡ Harvard University Press, Cambridge, Massachusetts, 1971 and 1995.

> proper sense of that word: a set of institutions supported by tax funds, but largely on faith, and without direct responsibility to political control. (p. 392)

Within the profession, one specialty stood out from the rest: “High-energy physicists were among the most prominent members of their profession—key figures in the nation’s strategic defense and science policymaking councils . . .” (p. xi). D. Allen Bromley, a physicist from Yale University, was scientific advisor to President G. H. Bush. In a review of his time as advisor, he pointed out that, “. . . the advisors and almost the entire membership of the [President’s Science Advisory Committee] were physicists . . .”[1]

Those physicists who spoke out publicly on cold fusion research were following closely in this tradition of articulating national policy in science. This influential position on matters of prime importance was held by physicists for many decades as attitudes of rule and governance became endemic. The scientific techniques that made physics itself so successful strongly reinforced these attitudes. One technique was to devise the definitive experiment, one that forces nature to reveal its structure. That kind of experiment is, of course, the very purpose of experimental science particularly in nuclear physics, where it had proved eminently successful with experiment after experiment in unlocking the structure of the atom.[2] The new knowledge was highly definitive and was put to work promptly. It emerged as the technology of the atom bomb, the electrical generating plant, and the quest for a fusion source of electrical power.

These technologies involved physicists, their literature, and their professional societies in a visible leadership role that went well beyond government policy making. Physics, and particularly nuclear physics, was looked upon by many physicists as the senior science relative to other sciences although in later years it was somewhat eclipsed by particle or “high energy” physics. Physicists expected to have the final cut in ascertaining what was and was not to be labeled science.

This outlook contrasts starkly with that, for example, of the geologist examining the question of plate tectonics. Since no definitive experiment was possible, geologists accumulated evidence for nearly a half century before drawing conclusions. From this perspective, physicists confidently saw physics as a more scientific discipline than geology.

This view of the proper role of physics was evident when the cold fusion furor broke. Cheves Walling was an eminent chemist in Pons’s chemistry department at the University of Utah and a member of the National Academy of Science. He drafted a paper with another department chemist, Jack Simons, suggesting a nuclear mechanism that might provide a heat source in the Fleischmann and Pons experiment. They sent a copy of their paper to Dr. Steven E. Koonin, a well-known theoretical physicist at Caltech. He replied, "You have a real problem. These are all the right questions to ask. I don't have any answers, and neither do you."[3] With that slap in the face, the precedent was established that there would be no dialog about nuclear physics with the other disciplines of science.

The outspoken physicists were without the proper expertise when it was time for the community of scientists to evaluate the claim of anomalous power generation. They spoke out with the voice of a few skeptics. They made no assertion of professional expertise in the disciplines of electrochemistry and calorimetry. Lacking that, they simply *assumed* the calorimetry must be wrong. They then proceeded to forcefully promote that assumption from spring 1989 onward. These skeptical physicists can trample other specialties in science, it would seem, but others may not trample theirs.

Fusion science had been the home territory of plasma physicists for several decades. A plasma is the fourth state of matter after those of solid, liquid, and gas. A flame is the most common example of a plasma. In parts of it, the electrons have broken off from their hot atoms due to their high energy. The flame's intense light is generated as they return to their proper orbit within the atom.

The velocity of the particles in the center of a hot fusion reactor (tokamak) was high enough to produce deuterium-deuterium or deuterium-tritium nuclear collisions. The resulting nuclear fusion reactions release nuclear energy. It was only natural that an announcement dubbed "cold fusion" would bring to the fore those trained in plasma physics as its chief evaluators.

Pathological Science

The term pathological science has been used historically to describe the more outstanding incidents of mistaken discovery of incredibly wrong science. Nuclear physicists were the first to charge the two Utah scientists with practicing "pathological science." This charge was a serious one because it implied that the scientific community had no obligation to evaluate their claims. This charge required a careful look.

Robert W. Wood was an optical physicist at Johns Hopkins University, Baltimore, Maryland, and one of America's most eminent professors at the turn of the twentieth century. He played a definitive role in the case of the claim of discovery of N-rays.

Professor René-Prosper Blondlot was the leading French scientist of his day and head of the physics department at the University of Nancy. He had watched the discovery of x-rays become one of the great accomplishments of experimental science. A few years later, he was convinced that he had observed a new kind of invisible, penetrating ray. He named them N-rays, in honor of his city, Nancy. Blondlot asserted that these N-rays could be diffracted by an aluminum wedge, much as light was diffracted by a glass prism. He worked in a darkened laboratory because his experimental results depended upon his being able to observe a dull circle of light.

Other physicists, mostly French, replicated his experiment and reported similar results. Approximately 100 papers were published to report N-ray experimental results.

Professor Wood was aware of Blondlot's claims. With the encouragement of his associates in England and Germany, he took a steamer across the Atlantic one summer near the turn of the twentieth century on a vacation trip with his family. On arrival in France, he sent his family by train to Paris and rode on to Nancy in the early autumn of 1904. He participated with Blondlot in performing the N-ray diffraction experiment.[4] As Wood told the story, he slipped the experiment's critical aluminum wedge into his pocket unnoticed in the darkened laboratory, and watched while Blondlot continued to record his experimental results. When Wood published the story of his maneuver, Blondlot's claims for N-rays came to an abrupt end, as did his career.

A conflict over the reading of scintillation counts arose in 1923. A misreading caused a controversy between the Cavendish laboratory at Cambridge University, England, and the Vienna Radium Institute in Vienna, Austria. It happened as follows.

> Vienna had reproduced the Cavendish's light-element disintegration experiments and published completely different results . . . [James] Chadwick laboriously reran the experiments . . . The results confirmed Cavendish's earlier count. Chadwick then went to Vienna where he found that the scintillation counting was done by three young women . . . Chadwick observed the young women at work and realized that because they understood what was expected of the experiments they produced the expected results, unconsciously counting nonexistent scintillations. To test the technicians [the women] he gave them, without explanation, an unfamiliar experiment; this time their counts matched his own. Vienna apologized.[5]

Counting visual impulses by eye was singularly prone to error.

Bergen Davis, a professor of physics at Columbia University, was the proponent (c. 1929) of an outlandish experiment. Irving Langmuir, a renowned American physical chemist and Nobel laureate, invited him to visit his laboratory in Schenectady, New York, and give a seminar on his purported new discovery. Afterwards, Langmuir suggested a return visit to Davis's laboratory. A few days later, Langmuir took an early morning train down the Hudson River valley to New York City, and spent a day in Davis's laboratory doing his experiment. Langmuir participated by doing each single function that Davis did. Eventually, he guessed that the body motions of a laboratory assistant during a critical step in the experiment were telegraphing a clue to Davis. He suggested a change of arrangements so Davis could not observe his assistant. After that change, the experiment always failed.

The "polywater" episode is an example of pathological science, the kind of mistaken science that was charged against the cold fusion claims. The topic of polywater emerged in the early nineteen seventies from the Soviet Union (Russia) with the claim to have discovered a new molecular formation of water (H_2O). Only one conference was held in the West to discuss the subject, and "The number of full length technical papers [published] . . . was fewer than ten."[6] The experiments were plagued with low signal to noise ratio. Careful laboratory work at Los Alamos National Laboratory ultimately demonstrated that what was called polywater was nothing more than contamination.

Full-length technical papers published in the field of cold fusion research numbered more than twelve hundred by the end of 1994, many showing experiments with a comfortably high signal to noise ratio. It was not reasonable to make a comparison of the two fields of study, as was attempted by some skeptics.

During his long and successful career, Irving Langmuir made something of a pastime of reviewing cases of mistaken scientific discovery. In a 1953 colloquium, he described three cases in the basic sciences: R. P. Blondlot of the University of Nancy, Bergen Davis of Columbia University, and Fred Allison of Alabama Polytechnic Institute. Each experiment and its "discoverer" had captured the attention of the scientific world. Scientists eventually demonstrated that each one of the claims were empty. Over a period of years, Langmuir abstracted those characteristics that the false discoveries had in common.

In three of his six cases, the experimenter visually observed a flickering light to collect the experimental output data (Langmuir did not include the Vienna case). These three cases had in common that the experimenter's eye and brain were the detector or sensor instrument. This laboratory technique was especially prone to entice the operator into a pattern of self-deception. As Langmiur put it, ". . . these observations are near the threshold of visibility of the eyes".[7] Of course, this light detection technique by eye was soon overtaken by electronic detector instrumentation.

Two examples, that of Blondlot and of Davis, demonstrate how visual measurements can go awry, and how two of America's best experimental scientists, Wood and Langmuir, went about setting things aright: *the critic went into the laboratory and participated in the questionable experiments. He practiced the experimental protocol and performed the calculations. His criticisms, then, were well informed.* Langmuir's investigative methodology was *participatory.*

SUMMATION

Langmuir's Criteria for a Pathological Science

Irving Langmuir's criteria for a pathological science can be condensed from his original lecture of December 1953.*

1. The maximum effect that is observed is produced by a causative agent of barely detectable intensity. The magnitude of the effect is substantially independent of the intensity of the cause.
2. The effect is of a magnitude that remains close to the limit of detectability or many measurements are necessary because of the very low statistical significance of the results.
3. There are claims of great accuracy.
4. Fantastic theories contrary to experience are suggested.
5. Criticisms are met by ad hoc excuses thought up on the spur of the moment.
6. The ratio of supporters to critics rises up to somewhere near 50% and then falls gradually to oblivion.

* Langmuir, Irving, "Pathological Science," (*Physics Today,* vol. 42, October 1989), p. 44.

When he declared a science to be pathological, he knew what he was talking about.

The accusation that the Fleischmann and Pons experiment was a form of Langmuir's pathological science was demonstrably wrong. Noting Langmuir's first criteria, the experiment's outcome varied in a large proportion to the driving current thus failing the first requirement.[8] Criteria two and three are also not affirmed. Four is not applicable; Fleischmann and Pons offered a nuclear source, not as a theory, but as an hypothesis, a substantially different sort of thing. Their theory about a mechanism for fusion may prove wrong, but it was not fantastic. The insistence that the Fleischmann and Pons experiment was an example of Langmuir's pathological science can be seen to be in error.

Other Pathologies

The accusation that cold fusion was pathological science brought with it a few other pathologies of a different sort.

Dr. D. R. O. Morrison, a physicist at CERN, Geneva, Switzerland, declared that, "Cold fusion is best explained as an example of pathological science."[9] His arguments discussed the ratio of supporters to skeptics, Langmuir's last consideration, and the ratio of successful to unsuccessful excess heat experiments, a criteria that was not a part of Langmuir's method.

He gave a paper at the Baltimore APS meeting on the "Status of Cold Fusion." He explained that he was studying the "mistakes" of science. One got the impression that he stayed deeply in the midst of cold fusion studies so he could say at some later time that he watched its failure from the inside and that he was in a position to know the authentic history of its rise and fall. It must have been discouraging for him to see his target topic continue to levitate year after year.

Morrison mistakenly refers to the discrepancy between the amount of nuclear emissions and excess energy as a flaw that only the critics were sharp enough to spot. "But scientists quickly recognized a drastic discrepancy—for each watt of power there should be 10^{12} neutrons per second (a million millions) but only a few were observed . . ."[10] It was Fleischmann who first described that discrepancy.

Morrison's view of cold fusion did not change. After another conference in December of 1993 he said, ". . . nothing at this conference changed [my] mind that [cold fusion] is pathological science."[11] Explaining himself at an earlier conference, he pointed out that, "In 1953 Irving Langmuir gave a delightful lecture on pathological science . . . where he discussed some cases such as N-Rays, where a number of good scientists reported wrong results."[12]

Morrison appears to have overlooked the first four items in Langmuir's list that concern the claims of unusual scientific measurements. They do not fit Fleischmann and Pons's claims. For example, Langmuir's first specification requires an absence of proportionality between the experimental excitation and the anomalous power as claimed. Fleischmann and Pons show three successive electrical currents exciting the cell of 8, 64, and 512 milliamperes in their Preliminary Note. They claim the cell's responses were 0.036, 0.493, and 3.02 watts of excess power output. This progression of input and output is not evidence of an absence of proportionality. Whether these numbers were right or wrong is not the point either. It is the *claim* that was charged with being pathological. The claims as presented did not fit the Langmuir characteristic that called for an output value independent of the intensity of the input.

What was the significance of the failed experiment? Morrison had made great hay with the topic. He seems to have found that it was equally as significant as the successful experiment. He lectured at Baltimore and at the University of Utah (September 1989) on the subject of the number of failed experiments as compared with the number of successful experiments. My investigation concluded that the counting of failed experiments conveys no diagnostic information with regard to the Fleischmann and Pons phenomenon. This topic is developed more completely in Chapter 8, p. 106.

Morrison is emblematic of the almost unlimited verbosity of e-mail type communications. E-mail networks carry not only unruly critiques, but often versions one, two, and three of the critique.[13] It was as though the world had the time and interest to watch someone do their homework. The verbosity and ego-centricity of e-mail communications greatly curtailed its usefulness. Also, that characteristic of the medium seemed to be present in many other academic fields of study. I generally found that the study of print publication was more rewarding for the time committed.

Morrison presented a request at Baltimore that was exemplary. He asked, ". . . for humility and sympathy for everyone," involved in this mutual adventure. At that time, and in those circumstances, it was a profound offering. It still is.

In an article published in September 1989, the director of one of the national laboratories recognized that the Langmuir criteria did not fit the Fleischmann and Pons claims, but he was still sure that their claims were pathological science. So he and his co-author conjured up a list of criteria for such "degenerate science" and, lo and behold, their criteria fit the Fleischmann and Pons experiment precisely. This event is treated fully in the next chapter.

The critic of cold fusion research needs to be an active laboratory participant. Full exposure to laboratory procedure is a requirement. This experience is the ordinary way in which a scientist gains confidence in, or discovers an error in a claim. This is especially true if the experiment is one that happens to be revolutionary in its implications. It may even be appropriate to have a cot in the corner, should the work prove tedious and extended.

Dr. John R. Huizenga, professor of chemistry and physics, at the University of Rochester, New York, qualifies as a skeptic of cold fusion. He played a preeminent role in its public evaluation and derogation during the first six years of the cold fusion saga. His book, *Cold Fusion:The Scientifiic Fiasco of the Century,* informs us of his manner of evaluation.* I searched it in vain to learn of his actual experiences with cold fusion experimentation in the laboratory. It could reasonably be wondered if he had ever even walked through a cold fusion laboratory.

The same thing could be said about Morrison. He tried his hand at writing a peer-reviewed critique of Fleischmann and Pons's calorimetry papers of 1989 to 1992.[14] The two chemists replied with a comprehensive display of the fine details that must be accommodated for successful calorimetry. Morrison apparently then abandoned the field.

There was another aspect of Langmuir's concept of pathological science that was not recognized by the skeptics. If one looked at his examples in the hard sciences, they involved unusual laboratory apparatus designed and built specifically to serve as the basis for the extraordinary claims. The apparatus of Fleischmann and Pons, however, was quite conventional with a history more than a century old. The electrolytic cell through which a current was passed to give off oxygen and hydrogen is well known to undergraduate students. The

* It has been published in two editions, 1991 and 1993.

cell the two chemists designed for their cold fusion work appears to be tightly configured, but otherwise perfectly conventional. Their experiment was a modest variation on the most ordinary sort of laboratory technology.

Was cold fusion research pathological science? No. It may prove right or it may prove wrong, but it was not pathological. More important, there is apparently no useful standard by which one can avoid the sometimes lengthy and expensive effort to determine the correctness of a purported new field of science. Declaring cold fusion to be a pathological science was seen as a short cut to understanding it. There is no shortcut; the experimental work will have to be followed through to the end, wherever that may lead.

Judgement at Caltech

It is worthwhile to remember from our perspective at a twelve year remove that University of Utah President Peterson in 1989 called for evaluation and judgement of Fleischmann and Pons's work by the scientific world.

> The full story of the research . . . announce[d] today will not be known for months or years as others confirm and challenge and enlarge their ideas and their data.[15]

He never imagined what would take place just 39 days later. Peterson's statement can be contrasted with a retrospective comment made by Professor Koonin the week after the American Physical Society (APS) held its spring meeting on May 1, 1989 in Baltimore. He reminisced about the formation of his judgement on the cold fusion claims, "So I would say already by about the 17th or 18th [of April] . . . I think at that point we really started to get worried . . . I decided finally . . ."[16] The seventeenth of April was just twenty-three days after the Utah announcement. His final judgement appears to have been made not later than the April 25, thirty-one days after the announcement.

Dr. David Goodstein, Vice-Provost at Caltech, did not participate in the Baltimore meeting nor in the preparation for it. His assessment of it, however, written five years later, articulated precisely its principal achievement. He wrote,

> For all practical purposes, [cold fusion] ended a mere five weeks after it began, on May 1, 1989, at a dramatic session of the American Physical Society in Baltimore. Although there were numerous presentations at this session, only two truly counted. Steve Koonin and Nathan Lewis, speaking for himself . . . [both] from Caltech, exe-

> cuted between them a perfect blocked shot that cast Cold Fusion right out of the arena of mainstream science.[17]

The APS assigned two evenings for special sessions on the new subject of cold fusion, Monday and Tuesday, May 1 and 2. What took place in 20 hours at Baltimore set the conditions of debate, and reporting of the debate, for subsequent years. The meeting disposed of the ongoing evaluation of the Utah claims by placing them permanently into a small box. That intellectual box, or ghetto, became the most salient characteristic of the field of study. Ten years later, Scaramuzzi could say,

> However, after ten years, in spite of undeniable (although not overwhelming) progress in the field, there is hardly any communication between this small CF community and the scientific world at large . . . I have experienced with distress the lack of communication with the rest of the scientific world, mostly because I am aware of the rigorous scientific approach with which the research has been performed by the ENEA Group in which I have been operating in Frascati . . . There is still no effective dialogue between the CF community and the traditional scientific world.[18]

A hint of the debacle that took place in Baltimore was first seen at a National Academy of Sciences meeting on April 18–20 in Washington, D.C. At this gathering of scientists, the theme of stopping the cold fusion fantasy was bandied about in the corridors. The spring meeting of the APS in Baltimore, eleven days hence, was fortuitously timed to serve as a platform.

Professor Steven E. Koonin was one of America's recognized theoretical physicists. He took an intense interest in the Utah cold fusion episode from its start. He called to Professor Jones, a nuclear physicist at BYU, to ask if the scheduled announcement at the University of Utah that afternoon was consequential. Jones informed him that it was.

Within a week, Professor Koonin had completed a first set of fusion calculations and submitted the paper to *Nature* for publication.* In the first three weeks he wrote three papers, had been invited to give presentations at a gathering of nuclear physicists at Erice, Italy, and had also been invited to give a presentation at Baltimore.

In retrospect, there was a lot of education for him in this activity. He was surprised to find a whole book solely on the subject of hydrogen in metals. He was astonished to learn that the subject of possible hydrogen fusion in metals

* Koonin calculated the rates of fusion of deuterium nuclei when they are compacted.

had been experimentally studied as long ago as 1926. At the start, he knew only one electrochemist.[19] But he was a quick read.

He felt that the two Utah chemists were off by a factor of a billion, presumably referring to the claim of D+D fusion. He had dismissed the hypothesis of a source of energy in presently unknown nuclear processes, although other comments of his indicate he was well aware of that hypothesis.

Koonin gave us the arguments that led to his conclusions about the cold fusion announcement. Koonin's conclusions were based in part on Lewis's work (below). (1) Lewis allegedly found errors in Fleischmann and Pons's calorimetry, (2) Lewis was unable to generate anomalous power in his own experiments, (3) Koonin could not understand theoretically how cold fusion was possible. Koonin had reached a more significant opinion one week after the APS meeting, "It is looking to me more like it's outright fraud at this point."[20]

Nathan S. Lewis, professor of chemistry at Caltech, was an experimentalist. He experimented with various electrolytic cell designs within the scope of the information available to him. He tried to produce anomalous power, tritium, neutrons, gamma-rays, and helium. He put together an ad hoc team that ultimately consisted of twenty-one graduate students and postdoctoral associates culled from the corridors of Caltech. This large number of technicians was astonishing for an academic setting; Fleischmann and Pons had a support team numbering exactly one. The difficulty in making use of such a large group was illustrated with the following example selected from the effort to discover "high temperature" superconducting materials two years earlier.

Dr. Paul Chu, University of Houston, was recognized for having discovered a high temperature superconducting compound. He had tried a maneuver similar to Lewis's during the several months of frenzy leading up to the discovery. Chu had an assistant, Ru-Ling Meng, who was responsible for the critical steps in preparing samples. This maneuver was described in a contemporary account.

> In the hopes of trying a greater number of potential superconducting compositions, Chu augmented his overworked team of graduate students and postdocs with three undergraduates. They were assigned the exacting but tedious tasks of weighing chemicals, mixing and grinding powders, and overseeing the baking and cooling operations. But after three weeks of trying, nothing seemed to work. Perhaps they hadn't ground their starting materials finely enough, or maybe they mixed the chemicals in the wrong ratios. Whatever the reason, Chu reluctantly told Ru-Ling Meng, who had a special knack for the tricky synthesis, that she would have to get back to the grind.[21]

A cold fusion laboratory's performance depends greatly on the details of technique such as cleanliness, the careful handling of components, preserving an electrode's surface exactly as it ought to be. To put together an ad hoc group to do surface-catalyzed electrochemistry was preposterous, like collecting some physicists at a convention to build a tokamak.*

Unfortunately for Lewis, Pons would not communicate with him. He may have had some concern about the ultimate purpose of such a large staff or there may have been other reasons. Lewis had to get the cell specifications by whatever means he could manage. His team collected reports from the press, facsimile networks, and by telephone inquiry, to learn the design of the Fleischmann and Pons cell. For example, he determined the overall flask dimensions from a photograph in which one was held up by Pons. He said he proportioned the diameter of the flask to the diameter of Pons's wrist.

Every known variant of each element in the cell was incorporated into one experiment or another in Lewis's laboratory. Although an Edisonian "try everything" technique is not unknown in science, it inevitably gives the impression of waste as it has the appearance of being a somewhat mindless strategy, one that consumes excessive resources. One of the post-doctoral members of Lewis's team, Reginald Penner, gave his view of that kind of experimental procedure. He tells how, "The first two weeks were an incredible roller coaster. Every day we learned something that made us think everything we had done so far was wrong. So we'd say, 'That's it! That's the thing!' and make a new cell." "This casting [of the palladium cathode] business really bummed us out. It implied that everything we had done up to April 20 was wrong. Which wasn't true. We found out later that Pons and Fleischmann hadn't used cast palladium at all, but just regular extruded wire."[22]

Loading

It was suggested during 1989 that the numerous failed experiments might be due to some threshold effect in which failure always results if the experiment operates below a particular value. One such threshold tentatively emerged by the spring of 1990. It became clear that merely letting the palladium cathode absorb deuterium to the level it was pleased to reach was not sufficient. To generate excess heat, the ratio of deuterium atoms, compared with the number of palladium atoms, had to be greater than a threshold value or the experiment could not work. This was referred to as the *loading* ratio, the level to

* To a great extent, this ad hoc method decreased the likelihood of success of the Japanese government's experimental efforts in the mid-1990s in cold fusion research. The U.S. Government has several national laboratories available to it that are maintained precisely for the purpose of evaluating scientific questions.

which deuterium atoms have been loaded into the palladium metal. I believe Michael McKubre, SRI International, Menlo Park, California, was the first to explore this phenomenon.*

Trying a multitude of possibilities was only one of several avenues open to Lewis. In contrast, McKubre expressly did not set out to copy the experiment. He decided that since his group had worked intensively with deuterium in metals previously and had observed no anomalies such as excess heat, he knew the interesting region, if one existed, must be at the high loading levels. Until now, he assumed, these *had not been obtained and, therefore, had not been studied* in the open literature. McKubre was quite explicit about this.

> We set about it in a different way from most of the more famous people, other people whose experiments are now more famous. We didn't attempt to reproduce the F and P experiment as understood by close examination of newspaper clippings, for example. We didn't build a cell like theirs at all. What we did was take the hypothesis that under conditions of high loading in an electrochemical environment the deuterium palladium system could be made to give off heat and possibly nuclear products. Given that hypothesis, how would you go about testing it? We devised an experiment that we believed would achieve those conditions independently of any knowledge of the electrochemical apparatus and cell geometry of Fleischmann and Pons.
>
> So the first experiments we did were at elevated pressure and reduced temperature, both of which favor achieving the high loading conditions [D/Pd]. We didn't use open cells. It was an electrochemical cell with a modest over-pressure of deuterium gas.† We started working with the elevated pressure and reduced temperature experiments and obtained good loadings, and it was reasonably replicable. In our first experiments we saw what we thought was evidence for excess power.[23]

McKubre had an advantage over Lewis because his team was immersed in the chemistry of deuterium dissolved in palladium at the time of the Utah announcement. He was in a position to lay out a course of research that brought him to the position of corroborating the Fleischmann and Pons claim of anomalous power. Reaching that point took five years of laboratory research.

* Others, i.e., Kunimatsu and Fleischmann, were coming to the same conclusion at about the same time.

† About 1,000 pounds per square inch of pressure inside the closed cell.

Caltech underestimated the time required to evaluate the Fleischmann and Pons claims by a factor of about fifty.

For his scheduled talk at Baltimore, Lewis developed a multifaceted criticism of the Utah claims. In addition to his experimental work, Lewis devoted time to recalculating the performance of the Fleischmann and Pons cells as reported by them in the Preliminary Note. He found that one column of their published results went beyond what was normally accepted as data reduction (the calculations that turn raw data into meaningful values). They had included a column of values that could be anticipated as resulting from a superior, but as yet untried, electrochemical operation. Such an extension of calculation was improper for them to include unless special attention was directed to it, which it was not. The column provided data for claims of much higher power output to input ratios than they had actually achieved.

Lewis's own experiments demonstrated to him that there must be major deficiencies in Fleischmann and Pons's calorimetry: without mechanical stirring of the electrolyte solution the heat measurements would be wrong. He concluded that Fleischmann and Pons had committed the mistake of not having *stirred* their cell's electrolyte sufficiently to obtain accurate temperature measurements. Lewis thought this would cause temperature variations large enough to vitiate their claim for anomalous power.

During the month of April, Koonin and Lewis arrived at these conclusions, and started planning what would be done at the Baltimore meeting. They agreed that the Utah scientists had not achieved the generation of anomalous power. Koonin concluded that his theoretical calculations eliminated any possibility of deuterium-deuterium fusion; that no one could figure out how cold fusion might be theoretically possible; that the Utah nuclear measurements were erroneous; that no one was seeing the necessary neutrons; and that there were gross calorimetry errors in the Utah experiment. But mostly, ". . . nobody could think of how it could work . . ."[24]

Koonin spelled out his plan for Baltimore: ". . . I was going to hit really hard . . ."[25] He decided carefully what words he would use, "I talked to a lot of people before I settled on those words . . ."[26] It was with alacrity and fervor that Koonin and Lewis prepared for what was to be Baltimore.

The Assault

Although the meeting occupied two evenings and over 40 papers were presented, Koonin's and Lewis's on Monday evening did the heavy work. (The three press conferences that were associated with the two evening sessions proved important. These are reviewed in the next chapter.)

Koonin's presentation showed the deep conflict experienced by those with expert knowledge when faced by an utterly impersonal heresy whose potential sweep of change was quite inconceivable. He went through the calculations of deuterium-deuterium fusion under what he took to be the conditions inside the palladium lattice, and proclaimed that it could not happen. He did nothing with the Utah hypothesis that, ". . . other nuclear processes" caused the effect. He demonstrated that conventional nuclear reactions in the quantity needed to support the amount of anomalous power claimed by Fleischmann and Pons were impossible. No one, however, had claimed that the anomalous power was due to well-known nuclear reactions.

He presented his calculations and results to the assembled physicists in a professional manner, although with arrogant overtones when passing judgement on other physicists' work, ". . . he got it mostly right." As he came to the end of his lecture, he switched from science to politics. There was the matter of Fleischmann and Pons and how to protect the world from them.[27] Koonin offered this denouement to the gathered professional audience. "We are suffering from the incompetence and perhaps delusions of Drs. Pons and Fleischmann,"[28] a comment he knew was likely to destroy their professional stature. The audience sat quietly for a moment, possibly waiting to see if the sky would fall, and then it burst into enthusiastic and sustained applause.

The assembly of physicists had found deliverance. Now they could go back to their desks relieved of this crazy anomaly. Hot fusion research funding would not be threatened. From this time forward, the cold fusion episode for the orthodox was only argumentative politics; politics and nothing more. The Utah fusion threat had been quickly and successfully contained.

That considerable response of the roomful of physicists ought not be attributed entirely to the persuasive powers of Koonin and Lewis. As scientists the two did not carry great authority within their respective professions. They were only a couple of especially competent professors. Their polemics on that evening in May simply triggered the pent-up emotions of the audience. The rank and file physicists knew from the announcement that Fleischmann and Pons were offering bogus science either out of audacity or ignorance or a little of each.

The falsity of some of these accusations against Fleischmann and Pons was recognized immediately by *New York Times* science reporter William J. Broad.

> . . . and one called them incompetent. They are far from that. Dr. Fleischmann, 62 years old, is a past president of the International Society of Electrochemistry and a Fellow of the Royal Society, the top honorary society for British Scientists.[29]

But Broad was the only reporter to notice.

Lewis's presentation in Baltimore followed immediately after Koonin's. It came across to the audience as a thoroughly competent, comprehensive, analytical and experimental review of the Fleischmann and Pons experiment. Did Lewis successfully *copy* the Fleischmann and Pons experiment? Certainly not. The information needed to copy it simply was not available.*

Consider the length of the experimental run. It can be calculated that only ten days ought to be required.† Fleischmann and Pons's definitive experiments ran for 10 weeks, even though a clever experimenter can get some results earlier. Figures 4.1 and 4.2 in Chapter 4 showed that in some cases the most interesting things happen after more than seven weeks have passed. In a paper published two years later, Fleischmann and Pons say, ". . . The minimum time for a single experiment had been three months."[30] Lewis was reporting on his laboratory experimental work just thirty-nine days after he started.

Another question concerns achieving proper loading of deuterium into the palladium. Lewis did not mention this topic during his APS presentation, perhaps because he was unaware of its importance. It was one of the first considerations of those scientists who were more experienced in hydrogen-in-metal (hydride) systems.‡ In his published paper (August 1989), Lewis states his achieved loading, "The D/Pd of 0.77, 0.79 and 0.80 obtained from these measurements were taken to be representative of the D/Pd for the charged cathodes used in this work."[31] By the end of 1993, McKubre's experimental reports specified that the minimum loading needed to allow the generation of excess energy was 0.90[32] (although some had published values as low as 0.85). This value (0.90) was confirmed by others working in the field. Only at levels above 0.90 will the Fleischmann and Pons phenomena of excess heat occur, thus making it worthwhile to look for neutrons, gamma-rays, tritium or helium products.

At Baltimore, Lewis stood before the audience of 250 or so physicists and

* In 1993, for example, Fleischmann and Pons reported in the peer-reviewed literature the operation of a cell and calorimeter at the boiling point of water in an experiment that generated over 100 watts. (*Physics Letters A,* 176, May 3, 1993) pp. 118–293.) Some of this experiment and its calorimetry was corroborated by being successfully reproduced in part by a team at the French Atomic Energy Commission laboratories in Grenoble, France. (G. Lonchampt, L. Bonnetain, and P. Hicter, "Reproduction of Fleischmann and Pons Experiments," [ICCF-6, vol. I, October 1996], p. 113.) That reproduction required twenty-four months of work, close consulting with Pons, and is not yet complete. (It is said that Pons provided the Pd cathode that finally made the experiment work.) This example gives the reader some inkling of the difficulty of the Fleischmann and Pons electrolytic cell experiment.

† The infusion time of deuterium into palladium metal is well known. One can use this as an argument for estimating the required loading time of the experiment. Some experimenters have suggested that additional factors are involved in the loading that extend the overall time considerably.

‡ A hydride or deuteride is a metal that has absorbed a great deal of hydrogen of deuterium respectively.

gave a lecture in absentia to Fleischmann and Pons on electrochemical laboratory technique. He explained as a professor might to a freshman chemistry class, that in this experiment one must *stir* the electrolyte exactly so if they want their temperature measurements to be correct.* The Utah scientists had failed to stir their cells (see Figure 3.1, page 39, no mechanical propeller is evident). It seemed quite beyond Lewis's imagination that a lifetime experience working with such cells might have gone far beyond the awkward notion of using propellers or such to provide adequate stirring.

Lewis's cell design did not mirror the Fleischmann and Pons cell. Pons had used an overly large diameter flask for the photograph from which Lewis had taken his measurements. The flask was designed for much higher power levels, but never used. Lewis's cells probably needed mechanical stirring to give correct data because of excessive diameter and lack of a vacuum insulation.† In his paper, Lewis says that there was no vacuum whatsoever in his Dewar flask, and, while giving its volume, avoids mentioning the inside diameter. He also does not mention that Fleischmann and Pons transferred heat out of the cell by means of radiation while he used conduction. Lewis's notion that he had copied the performance of the Utah experiment was a fantasy.

Lewis, nevertheless, was thoroughly persuasive with the audience and with the science reporters about the failure of Fleischmann and Pons's work. I have not been able to find any confirmation that Lewis ever again invoked or defended his absolute assertion that lack of stirring invalidated the Utah claims of generating anomalous power. Yet he never revoked that absolute assertion to America's science reporters.

For the physicists in the audience, Koonin and Lewis's successive putdowns of the two Utah chemists along with their work was delicious. Their sustained applause indicated acceptance of Koonin's and Lewis's implication that the scientific evaluation of the Utah claims was now complete.

* Wilson et al., dealt with this issue in his 1992 technical review of Fleischmann and Pons's calorimetry, with the statement, " . . . inadequate mixing within the cell . . . does not appear to be a problem." (Wilson, et al., *Journal of Electroanalytical Chemistry*, July 1992.) Lewis never responded in the literature to Wilson's or Fleischmann's refutation of his criticism.

† Building a mechanical stirring capability into the cell would add to the complexity and cost of the cell. The cell is surrounded by a meticulously insulated and temperature controlled bath. Since cold fusion experimentation was already expensive, great care was taken by Fleischmann and Pons to design a cell, and a system for operating the cell without mechanical stirring and still keep the performance and measurements entirely adequate to their scientific purpose. At times the experiments were set up in what is called a factorial manner. Five experiments would or would not be present in each cell. There are 32 permutations of five binary values. Then the 32 cells would be run for three months. It gave multiple results for each of the five experiments.

CHAPTER SIX

Four Press Conferences

The most revealing aspect of the Baltimore meeting was the manipulation of the press. Three press conferences accompanied the first evening session, two before it and one afterward. There was a fourth press conference at the Electrochemical Society (ECS) meeting held in Los Angeles one week later.

There were ninety-nine science reporters registered for the conference due to the widespread national interest in the Utah announcement. The first press conference started at 4:00 P.M. on Monday afternoon, May 1, 1989, before the first evening special session on cold fusion research. A second started at 5:00 P.M. on that same afternoon. The third took place, not on Wednesday after the two sessions were complete, but on Tuesday morning at 10:00 A.M. This arrangement was quite unusual. Ordinarily, one press conference would convene after the proceedings to allow for any questions the press might have. These three scheduled press conferences demonstrated an aggressive intent.*

The first press conference had little content. This was to be expected since the presentations had not yet taken place. It ended at 4:30, with the announcement that there would be a second press conference in a half-hour.

The 5:00 P.M. press conference was the second of the three. It belonged to N. S. Lewis and there he revealed his purpose to the assembled press, the

* It is not likely that the assigned leader of the two special sessions was involved in the planning of the press conferences.

I have concluded that the press conference in March was forced upon Fleischmann and Pons by two factors: the revolutionary content of their claims, and the university setting that prevented further secrecy. These considerations played no role at Baltimore.

cream of America's science reporters, a few hours before his evening presentation. He averred, "If we're going to have publication with press conferences, we should have peer reviews as press conferences, too."[1]

He reminded the reporters present, "As most of you know, we've been working on this since day one, in fact, since the evening of the [Utah] announcement." He then stated Caltech's intent, "we're going to do the experiments necessary to see if this works."[2] The reporters, it seemed, were to understand that if the Fleischmann and Pons phenomenon did not exist, Lewis would do the experiments necessary to demonstrate that fact. Caltech was prepared to prove a negative, if necessary. Thus his testimony to the press was *absolute.*

He opened the press conference with an extended summary of the presentation he had not yet given. He ended it with the following statement.*

> We've uncovered a lot of methods that do not work. For instance, not stirring your solutions [in the cell]. You have temperature gradients[differences between one point and another]. The one electrode will inherently generate more heat than the other. The electrodes being big pieces of wire, also were cooling pins. They are efficient at removing heat from the system if you do not agitate the system and stir it. The temperature you measure depends on where you put the thermometer. You can get a very large range of errors this way and those errors *place serious doubt on the accuracy of the numbers that were measured by Pons and Fleischmann.* When we stirred the solution uniformly to obtain measurements that were independent of where you put the thermometer, we see no evidence for any excess heat.[3] (Emphasis added.)

A reporter asked, "Do you think that it was possible that in some of these results, where people did report some excess heat, that this failure to stir the solution could have caused them to get these [excess energy] measurements?" Lewis's unqualified answer was, "Absolutely." Another reporter asked, "Pons thinks he's proud of the fact that . . . he himself around December 1 produced this so-called excess heat, but that doesn't mean anything, does it?" Without demurring to the questioner's blatant attitude, Lewis replied in full, "Of course not."[4] Both answers were wholly unmitigated: "absolutely," and "of course not," period. Lewis came prepared not only to report on his own work, but to report on the work of Fleischmann and Pons as well.

* The terms *anomalous power* and *excess heat* (flow) were used pretty much interchangeably during this period to stand for the Fleischmann and Pons phenomenon. Where these terms appear in the quotations that follow, I have let them stand as they were originally used. In some cases, it is not clear whether the speaker is referring to heat energy or heat power.

A few minutes later, he said, ". . . we believe the excess heat will turn out not to be there."[5] There was no room allowed in these three answers for any possibility other than that excess heat energy did not exist in the experiments done by Fleischmann and Pons. For the nation's science reporters, the first APS special session at Baltimore *began* on that note.

The third press conference was held the next morning with Koonin as the respondent. He repeated large parts of his 20 minute presentation of the evening before. He then reinforced the evening's vilification of Fleischmann and Pons by adding a measure of ridicule. He said, in an ill-constructed simile, "It's all very well to theorize how fusion might take place in a palladium cathode . . . One could also theorize about how pigs could fly if they had wings, but pigs don't have wings."[6]

These savage words of satire were intended to destroy the Utah chemists. After that, their leaving from America was only a matter of time. Pons was driven to abandon his U.S. citizenship. Let me emphasize, it was not only that the Utah scientists were deemed wrong; the whole episode was much larger than that. The damage came as thickly in the press conferences as in the technical sessions.

It is interesting to note Fleischmann's response, since he is a particularly adept speaker. In an August 1992 invited speech to the British Association for the Advancement of Science (BAAS), he said, ". . . America has developed a conformist society . . . It was not that we were wrong; it was that we must stop."[7] Continuance would inevitably give a glow of legitimacy and thereby threaten the establishment's verdict. Fleischmann's point about conformity gains emphasis when one considers that it was unthinkable that the *American* Association for the Advancement of Science (AAAS) invite him to speak to one of *their* meetings.

The Caltech contingent was probably as surprised as anyone to observe the unintended consequences: cold fusion science moved from the inhospitable United States to a more hospitable Japan. It thrived there from 1993 to 1997 (and continuing into 2001). As the decade unfolded, research continued in Japan, China, Italy, Russia, India, and France, with no little continuing in the United States.

The ostracizing of scientists who work in the cold fusion field did not happen entirely in this one step. After the discrediting of Fleischmann and Pons in the eyes of the scientific elite, the next step was to extend knowledge of that fact to the national elite.

A high price would be paid for the conversion of this national debate from one about science to one of politics. Evaluation of the electrochemical experiment announced in March was to continue, taking five, ten, twenty or more years to complete. The U.S. evaluation was to be done under the most severe and generalized scorn. Public derision or the threat of it was always present: Who would be ridiculed next by some old antagonist who smelled a fresh opportunity?

The American Physical Society's Baltimore meeting occurred the biblical 40 days and nights after the first publicity about cold fusion. At Caltech, Koonin "could not think of how it could work." Lewis, after his five weeks of Herculean labors, found no excess heat or nuclear products. So much work to no avail, except that their labors availed well for their particular purposes. The Caltech crew to this day look upon that Baltimore evening as a solid success. As Koonin put it in his retrospective commentary, "The institute of Caltech comes out like a hero . . ."[8] Indeed, it *was* completely successful, but only in political terms.

Neither Koonin nor Lewis were especially powerful members of their professions by rank of office, awards and honors, or publication of textbooks. Their importance to us was due to the impact they imposed upon a new field of scientific activity.

Four broad-audience scientific journals (*Scientific American, Nature* (London), *Science* (AAAS), *Chemical & Engineering News* (American Chemical Society)) have remained largely silent in the subsequent ten years. This avoidance of cold fusion research news provided a protective cover for Lewis's assertions about Fleischmann and Pons's experiments. The world of science twelve years later knows only what Lewis claimed. Lewis was never obliged to defend his claims concerning Fleischmann and Pons's work. His claims stand as the last word on the subject, largely because the extensive work done and published since 1989 was not reported to the scientific community. This blackout of sorts was a principal accomplishment of the four press conferences.

By going beyond their own work, by speaking about the experiments of other scientists and of the psychological stability of those scientists, by doing so with professional acumen, articulate expression, and unbounded confidence, and by conveying this to an audience of established but now threatened physicists, Koonin and Lewis in twenty hours consigned cold fusion science to a ghetto. Thirty-nine days after its announcement, cold fusion studies became a scientific heresy.

Professor Koonin recorded his feelings about the meeting a week later. He expressed his confidence in a job well done. He was not pleased in the sense that, ". . . I think it has destroyed those two guys,"[9] after all, he continued, "you're not an assassin . . ."[10]

Six years later, Koonin's ridicule of Fleischmann and Pons had become institutionalized within the APS and voiced by their spokesman at an official APS meeting, Dr. Robert L. Park.[11] At a San Jose meeting, he titled his formal address, "Pigs Don't Have Wings: When Scientists Fool Themselves." He opened it with a repeat of Koonin's ridicule of the two Utah chemists' work.

At Baltimore, the public aspect of cold fusion was converted from science to politics. From that point, it was only a matter of each of us, you and I, choosing sides. The Baltimore event was well characterized by a comment of the late Petr Beckman in his newsletter, *Access to Energy,* on May 8, 1989, "Most of the . . . physicists at the convention applauded these and other instant experts who have found a short cut to glory by understanding everything in five weeks of guesses about a phenomenon that still puzzles two respected scientists after five years of laboratory work."[12]

Los Angeles

The vilification continued. The Electrochemical Society (ECS) held its annual meeting in Los Angeles one week after the Baltimore meeting. Lewis, Fleischmann, and Pons were present and gave talks. Lewis gave a restrained version of his Baltimore presentation.* To some extent, the speakers and the audience were following different agendas. The audience of electrochemists was only partially aware of the extent of the calumny that was heaped upon Fleischmann and Pons the previous week. On the other hand, the two chemists had no choice but to use their time to defend themselves.

Fleischmann responded to Lewis's claim that lack of stirring "absolutely" invalidated his results. He presented a video showing how quickly the cell's bubbling action causes mixing. When a red dye was added to a cell, the bubbles from the electrodes mixed the solution quickly. The dye was fully mixed in twenty seconds in a flask where the temperature was recorded every five minutes. This dispersion of the dye in a flask well insulated against heat loss by conduction implied a uniform temperature.

Fleischmann recognized the mixing issue as it was a staple of electrochemistry. He had selected the easiest way to achieve uniform temperatures. He set a high minimum value for the electric current, a high vacuum in the Dewar's wall for conductive insulation, and a low value for the flask's inside diameter, 3.30 cm. These were principal cell design values.

Lewis's flask did not meet these requirements. His Dewar vacuum was one atmosphere—none at all. He avoids revealing the inside diameter saying he used "dimensions very similar to those used" by Fleischmann and Pons.[13] Too large a diameter and a lack of a vacuum insulation on the outside surface of the electrolyte ought to be sufficient to cause Lewis's cell to give erroneous temperature readings. His statement that Fleischmann and Pons's measured power was wrong because of non-uniform temperatures was itself an error.

The demonstration of excellent heat measurement appeared much later

* There is no verbatim record of the proceedings of this ECS meeting.

when Fleischmann and Pons balanced their control cells to better than one percent. How easy it was to fool oneself into believing things about another's experiment when that other experiment was 1,000 kilometers distant.

Nature's Washington editor, David Lindley, specifically referred to Lewis's talk in his report of the meeting. "But the centerpiece of Fleischmann and Pons claim, that the heat is produced in their cell in amount too large to be explained by purely chemical process, was dissected by Nathan Lewis . . . who ascribed the energy generation to poor calorimetry . . . at the end of the meeting the physicists were left with the comfortable feeling that cold fusion was dead."[14]

A question about helium-four evidence was brought up at this Los Angeles meeting. Pons had mentioned that he first detected it in the gaseous effluent from heat generating cells the previous December (1988). He thought this helium-four was a nuclear ash resulting from the generation of anomalous power. He had mentioned it to Walling and Simons in March.* Fleischmann and Pons's opinions about the significance of the helium differed. Fleischmann assumed that the nuclear reactions, what ever they were, would leave their product in the bulk of the palladium. Pons's assertion that he detected helium in the effluent gasses came from a limited type of experiment. Under the onus developed the previous week in Baltimore, such data could no longer even be even alluded to in a public forum. Pons withheld his intended discussion of evidence for helium-four.

The fourth press conference in our series took place in the late evening after the technical conference. About twenty cameras were present. J. K. Footlick reported:

> . . . a physicist from the California Institute of Technology—a non-journalist who had crashed the press conference—commandeered a microphone and began shouting loaded questions at Pons and Fleischmann. Soon everyone was grabbing microphones and interrupting each other; a number of people, some of them physicists cholerically denouncing the work, stood on chairs to shout. Pons and Fleischmann sat stony faced in the television lights, perhaps stunned, certainly angry.[15]

The "physicist" from Caltech who commandeered a microphone was Dr. N. S. Lewis.

* We will see in Chapter 16, p. 223, that the claim has since been rescinded. It has been to some extent corroborated by several dedicated experiments in independent laboratories beginning in June 1991, although one cannot yet say that it is validated.

The audience saw the meeting as so much shadowboxing. Altogether, it did a disservice to Fleischmann and Pons. After the resounding attack of the previous week, the largely friendly audience wanted a powerful response. What they got was a discussion of a few assorted issues. The two chemists were now permanently on the defensive.

Santa Fe

Santa Fe was the nearest vestige of civilization to the Los Alamos National Laboratory (LANL) at Los Alamos, New Mexico. It was the site of a cold fusion conference, "Workshop on Cold Fusion Phenomena," on May 23–25, 1989, sponsored in part by the U.S. DOE. At that time, LANL had a large program underway. Virtually the entire cadre of those working in the field attended. This included members of the newly commissioned DOE Panel on Cold Fusion (Panel). Their accomplishments will be reviewed in the next chapter.

The two and a half days of presentations covered about everything pertaining to cold fusion research: nuclear and calorimetric data, positive and negative results, and some initial theoretical musing on the possible energy source. The name "believers" was introduced as a derogatory term for those who took the excess heat reports seriously, and many of those scientists who were not sensitive to the impact of word connotations used the term themselves. Approximately fifty papers were included in the published conference report.

It was true that the purpose of the meeting was to continue the debate on the reality of "cold fusion" announced two months previously. Many were surely looking for a correlation between nuclear activity and the amounts of heat claimed. The contingent of those who by then had taken a serious interest in anomalous power—the "believers"—had a different sense of purpose. They wanted to exchange views with others of like persuasion about the best way to design their next experiment.

The meeting was orderly and civil in contrast to the APS conference earlier in the month. Although reporters were present, the scientific points to be made were directed to the assembled scientists. Dr. E. Storms, a LANL scientist, offered the first emphasis on the need to obtain high D/Pd loading.

These "believers" were starting on a high risk and high stakes track. They knew that reaction products from well-known nuclear processes had not been well demonstrated. It was a rare and precious moment in their careers: the chance, from the very beginning, to work in science on something new and fundamental.

The Coup de Grâce

The DOE's Panel released its preliminary report in July. The report was thoroughly discouraging about the possibility of there being any "useful energy source" from cold fusion research.

Nature published Dr. Lewis's paper in August reporting on the work he presented at the Baltimore meeting. By September, Fleischmann and Pons were virtually without peer support. The Baltimore meeting in May and the Panel's negative interim report in July combined to destroy their reputation within the scientific community in America.

A major newspaper article that appeared in September contributed further to the destruction. Although the U.S. academic, political, and social leaders were aware of the disgrace cast upon Fleischmann and Pons, they received it second hand in short press quotes. That disgrace would be set forth now in detail in a newspaper read by America's intellectual elite. It happened in a long article in the *New York Times Sunday Magazine,* September 24, 1989. This may be thought of as following the Baltimore assault with a *coup de grâce.*[16]

Reports of confirmations and failures of the Utah experiment were falling off now. The March bandwagon jumpers were returning to more familiar research. A few long-timers were just getting their research underway for the several years required to evaluate the Fleischmann and Pons effect.

As far as the nuclear physicists were concerned, evaluation was complete. One of those was N. P. Samios, Director of the Brookhaven National Laboratory, Upton, Long Island, New York. He collaborated with Robert P. Crease, assistant professor of philosophy, University of New York, Stony Brook. They co-authored a description of the controversy in a newspaper article that was a lampoon. It said that the cold fusion episode was just another incident in the long history of pathological science, the science of things that are not so.

Considering the level of prestige the *New York Times* carries in the academic world, the Washington, D.C., community, and the national media, this amounted to a public tar and feathering of Fleischmann and Pons. From this point forward, they faced a hostile national elite as well as a hostile academic community. The two authors positioned the two chemists from Utah thus, "Today . . . Pons and Fleischmann continue to cling to their assertion that they have found something new. 'We are absolutely sure of our result,' Pons told the *Wall Street Journal,* earlier this month."[17]

The article introduces the public to Langmuir's term "pathological science." It states, "In fact, there may have been another factor at work—self-deception. Pons and Fleischmann apparently fell victim to the experimental scientist's worst nightmare."[18] The article describes how two established scientists with a good record fell into a pattern of self-deception that left them clinging to a cliff. How humiliating for them and for us all.

The first photograph in the story was that of Fleischmann and Pons, the second was of Blondlot who fell into a pattern of self-deception with his N-rays. The caption for both pictures uses a parallel construction: "Victims of self-deception? Martin Fleischmann . . . and Stanley Pons told Congress they had achieved fusion at room temperature. Below, turn-of-the-century scientist René Blondlot thought he saw N-rays."[19]

The theme digressed into the story of unlimited energy from hot fusion and the almost insuperable technical difficulties of releasing it. Far be it for a simple bench-top chemistry experiment to touch any of that. Then there was an explanation of how the electrolytic cell works. It was delivered, however, as though the authors considered the cell a simple "kitchen experiment." Its formal name of surface-catalyzed electrochemistry was not mentioned. Dr. Samios was beyond his realm of expertise when it came to chemistry, catalysis, surface kinetics, and calorimetry.

A striking characteristic of their lampoon was the heavy application of satire and childish over-simplification:

> No need to create a sun in the laboratory, no need for the equivalent of a hydrogen bomb. Just a tub of heavy water, an electrolyte, two electrodes, and some current.[20]

In an unusual and admirable accreditation, the authors stated that the Utah announcement did recognize that the neutron count was much smaller than would be required if conventional fusion had been at work. Samios and Crease's technical criticism ran as follows,

> Equally surprising was the relative lack of neutrons produced . . . If the excess heat that Fleischmann and Pons reported was due to fusion, then they should have found a neutron flux of 10^{13} neutrons per second in their laboratory . . . But they reported only 10^4. They attributed this to the fact that a new form of reaction was taking place.[21]

The authors did not explain to their elite audience how the two chemists reached that conclusion or, in their circumstances, what better conclusion was possible. Unfortunately, the authors went off the rational track and indulged themselves with more satire.

> They attributed this [lack of neutrons] to the fact that a new form of [nuclear] reaction was taking place. Wonderful! No need for tons of lead shielding, no vexing problem of waste disposal, no need

> to decommission plants because their parts had become too hot to handle.[22]

With that bit of derision, the Fleischmann and Pons claim for the anomalous power phenomena was dismissed out of hand. No attempt was made to suggest what error was concealed in their chemistry. There was no need to. Baltimore had taken care of that item.

The story's principal thrust was to describe the event as another example of pathological science. The authors enlisted the name of Irving Langmuir. Interestingly, they did not use his criteria. As they abruptly put it,

> Pathological science, [Langmuir] said, has a characteristic set of symptoms, and he drew up an informal list based on his own experiences. We have drawn up our own, based on ours.[23]

Well now, is it polite to ask, Why? Langmuir's list was not ". . . an informal list based on his own experiences," as asserted. It was based on his study of six cases and it was presented in a scientific symposium, a quite different sort of thing.

The two authors ". . . have drawn up our own [list], based on our [own experiences]." Again, is it polite to ask, What experiences? Have they too made a study of pathological science from which to derive their own list? Or did their ad hoc list serve only the purpose of this one article?

Crease and Samios's approach to the subject of pathological science was a marvel of manipulation. First, having made full use of Langmuir's name, they tossed his work overboard. They substituted a new list generated, heaven knows how, but designed solely to condemn the work of Fleischmann and Pons. They point out, for example,

> When an experiment begins producing results, the experimenters must still make sure that the readings represent a true profile of a scientific phenomenon—that the data have not been produced by something else in the environment or idiosyncrasies of the equipment. To guard against these "systematic effects" . . . scientists run experiments over and over, making small changes in the equipment to see whether the effects change.[24]

It is hard to see how this might apply to the Fleischmann and Pons experiment where each experiment runs for about ten weeks. Doing the experiment over and over, with the results of each run informing a "small change" to be made in the next run, as they describe it, might have required a decade. The authors seemed oblivious to what the two chemists actually did and to the myriad of consequences that followed from the nature of their experiment.

They proceeded to the heart of their story by listing and explaining the four pathological symptoms they have conjured up for their article. It is important to review their listed symptoms to understand the impact their thinking had on the national elite.

"Symptom No. 1: Too Many Miracles." This symptom expresses Samios's incredulity at the two chemists for hypothesizing a new, unknown nuclear process. There was no explanation of how Fleischmann and Pons rationally arrived at that hypothesis.

"Symptom No. 2: The 'Discoverers' Are Outsiders." The authors failed to show any self-awareness. Crease, as an assistant professor of philosophy, was certainly an outsider, if anyone was. Fleischmann and Pons were doing their stock-in-trade work to develop their electrolytic cell. That was their field and their expertise: surface-catalyzed electrolysis, chemical kinetics, and calorimetry. Furthermore, while they originally expected to get a lot of neutrons, to their surprise they got only heat, and measuring heat is another area in which they were, or rapidly became, insiders.[25] They were certainly not outsiders to the extent the two authors were in this field.

"Symptom No. 3: The Discoverer Has Not Tried to Kill the Discovery." The one example they give is the second hand comment that "They hadn't performed the experiment . . . with ordinary water instead of heavy water."[26] Again, they seemed oblivious to the technical considerations involved in selecting control experiments from among several possibilities.

"Symptom No. 4: Inability to Repeat the Experiment Is Met by Ad Hoc Excuses." Here they raised the "recipe" theme on which Professor Huizenga holds the patent. "A scientific paper with an inadequate recipe is a tip-off that the author's understanding of their work is incomplete."[27] They failed to realize that science, at the beginning, does not expect or require understanding. That would become the *continuing* purpose of scientific study. In 1903, Pierre Curie did not understand the self-heating of radium. In 1911, Dr. H. K. Onnes did not understand what enabled superconductivity. Nevertheless, both won Nobel prizes.

The Crease and Samios article was not intellectually serious. The publisher's and the authors' only apparent purpose was political: to make the damage done at Baltimore permanent, preventing the emergence of any further public support for the funding of cold fusion studies.

NSF/EPRI Conference

The first meeting of people who wanted to nurture this new field of experimental science originated under the auspices of the National Science Foundation (NSF) and the Electric Power Research Institute (EPRI). It was held in Washington at the NSF facility in mid-October 1989. It was planned by those who wanted to get some real work done on excess heat experimentation by means of a non-media conference.

The conference intended to advance the nascent effort to find (1) the extent of the Fleischmann and Pons effect, (2) a more replicable experiment, and (3) the source of the anomalous power. Huizenga gave us an excerpt from the letter of invitation to the participants.

> This workshop will attempt to achieve scientific dialog among a small number of invited participants (less than 35) away from the press and other forms of public attention. It is planned that there will be a volume of proceedings that will include recommendations and suggestions for future work. A preliminary agenda, including the names of some invited speakers, is being sent to you. The organizers look forward to your input and advice in discussion sessions and in proposing working group reports.[28]

The dichotomy between the skeptics and the acceptors becomes apparent, as Huizenga explains.

> . . . At least two skeptics, on receiving the above letter of invitation, refused to attend because, based on the number of "yeah-sayers" on the proposed [speaking] agenda, they concluded that the meeting simply offered a platform to present again the often-repeated questionable and well-known "evidence" for cold fusion.[29]
>
> On the other hand it was reported that a "believer" had to be persuaded that the meeting was not stacked with skeptics before accepting his invitation.[30]

In a wonderfully ironic quote, he includes a statement by one of the American Physical Society's officers[31] concerning the conference, "The entire cold fusion episode has been played out against a backdrop of academic misconduct."[32]

Those sponsoring the meeting wanted a discussion of anomalous power experiments. This placed a premium on attracting those who claimed success. The skeptics wanted a discussion about whether or not cold fusion exists, a quite different agenda. The acceptors wanted to get some work done in discussing the best evidence and what experiments ought to be tried next; the skeptics wanted to debate whether there was sufficient evidence for them to be persuaded that deuterium-deuterium fusion exists.

There was an acceptance within the engineering division of NSF that the Fleischmann and Pons phenomenon, "cannot be explained as a result of artifacts, equipment, or human errors." With the meeting so justified, the sponsors wanted it to be a quiet work session among those getting results. So, ". . . the sponsors tried to keep the meeting secret, initially planning to transport the participants by bus to an undisclosed location for their three day meeting."[33]

The skeptics raised a great uproar on two counts. They said that the planned conference was loaded with those who had "positive" results, that it lacked equal representation by those who had "negative" results, and was a rally for cold fusion "believers" rather than a scientific conference. The second and determining rebuke was that the NSF as a public institution could not bar the press. So a few more invitations went out, and the meeting went ahead in public.

The director of the physics division, Dr. Marcel Bardon, sent a message to 500 NSF employees just prior to the meeting. He wrote, "It seems unfortunate that a NSF office is now appearing to encourage such discredited work."[34]

It was a gross political act to write in that fashion to that many NSF employees when it was another part of NSF that sponsored the conference. That message told each employee that they ultimately must decide their *loyalty* between the science division and the engineering division of NSF. The implicit reason was that any continuation implied legitimacy, and legitimacy refuted the considered judgement of the physics establishment who were determined to be the decision-makers. The physics division wanted to wield whatever influence or authority they possessed to stop work on the Utah claims. That is how a conformist society is built.

Even within such a conference, the participants would have a behavioral concern. Who would be selected next to be publicly ridiculed, to be demeaned in front of others at a gathering of 200 peers in the manner of Baltimore? Would some selected person be singled out as a deluded and incompetent troglodyte disguised in a scientist's white lab coat? One would speak up only with great caution, or better yet, listen only, leaving the risk of exposure to others. Nevertheless, this was a chance for those having some degree of success generating anomalous power to compare notes with others and to plan further work.

Scaramuzzi much later expressed the need well, ". . . those who started working on it and got positive results believe in the reality of their results and are willing to go on until a better comprehension of the phenomena is acquired, . ."[35]

There was another aspect of the NSF meeting worth noting. Lewis was present (he gave a tutorial on electrochemical systems). Fleischmann gave a presentation in which he reported on 28 active (non-control) cells that had undergone electrolysis for three months, none of which was mechanically stirred. Twenty-three cells generated anomalous power greater than 20 milliwatts. He also argued that the recombination phenomenon did not effect his results.

When his presentation was done and the floor opened to questions, Lewis had nothing to say. Nor did he offer any argument for having told 250 physicists, and the science reporters most emphatically, that without a mechanical stirring of the cell one could know, "absolutely" that their results were wrong. At this critical moment, Lewis was silent on that matter. He had accomplished what he wanted to accomplish at the Baltimore and Los Angeles press conferences. He had no reason to speak to the subject ever again.

The skeptics had much public fun by ridiculing any mistakes made in analysis. The skeptics' sarcasm was only human. Their perception was that the participants were trying to analyze something that was not there, as a shadow-boxer hits something that is not there. It is always amusing to the onlooker. It seems like fair game as a target of mirth.

What was disconcerting in this behavior was that the science reporters also indulged themselves. For example, a press representative insisted at adjournment that some member of the conference's panel repeat an imaginative and wholly speculative statement made by a famous attendee, who had left early.[36] Most of the leadership refused to be intimidated by such juvenile sport. Unfortunately, no one had the presence of mind to speak up and expose the ridiculous pose the press had adopted.

All in all, the attack on the conference by the orthodox establishment exacted its toll. The technical report of the meeting was never distributed.

CHAPTER SEVEN

The DOE Panel

Admiral James D. Watkins, Secretary of Energy, requested in April 1989 that his department investigate and report on the cold fusion claims. The Department of Energy (DOE), under whose aegis the new claims fell, had a long standing advisory group for technical matters, the Energy Research Advisory Board (ERAB). Under direction of the Secretary, the ERAB established a Panel on Cold Fusion (DOE Panel) consisting of experts in the appropriate specialty fields.

It had twenty-three members of whom two were from the ERAB, others were selected from industry and academia. Both co-chairmen were academics, Dr. John Huizenga, professor of chemistry and physics, at the University of Rochester, New York, and Dr. Norman Ramsey, Nobel Laureate and professor of physics, at Harvard University. The Panel's work spanned the months from April to November 1989. It submitted an interim report in July and a final report in November.

What Admiral Watkins learned about cold fusion studies from the Panel's report was revealed in a talk he gave two years later. He concluded that it was all, "Just bad science . . ." and that, "Two members of the scientific community made everyone in white lab coats look fraudulent."[1]

What exactly did the Panel see as its purpose? How did the Panel carry out that assignment? What work did the Panel complete? On what grounds did it reach its recommendations?

The operation of the Panel was curious. It went about evaluating "cold fusion" much as a salon in Victorian England might go about evaluating "socialism." There would be a shelf full of books and reports arguing the many wonders of the world of socialism. Lectures by visiting or resident scholars would be offered, and above all, there would be discussions, relentless discussions. Members of the salon would visit some communal encampments to provide the sight and smell of their new-found subject, as well as offering some needed physical exercise. From this activity, the members would gain the satisfaction that they understood "socialism," or at least enough about it for their purposes.

In much the same manner, the Panel collected a five-foot shelf of reports from cold fusion researchers, many of which no doubt were read. There was the staff scientist who was available to lecture them on any subject, should they tire of each other's lectures and, of course, there would be discussions. Six field trips were arranged. Unlike a Victorian salon, the Panel had two co-chairmen who were obligated to submit a report. The report's principal conclusions are examined first.

Conclusions

The DOE Panel's final report gave faint praise to cold fusion: "(2) The Panel is sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system."[2] DOE subordinate agencies realized that if there was no recommendation to resolve any of the claims, and no recommendation to allocate money, they could conclude that the Panel considered the whole matter unimportant. They could assume that the Panel believed the claims to be without merit. Presumably, the cooperative experiments were merely to provide proof of flawed calorimetry. With those flaws revealed and removed from the Fleischmann and Pons experiment, nothing would remain.

There were reasons for this small recommendation by the Panel. The Fleischmann and Pons experiment was touted as a simple one, a "kitchen" experiment. They knew of N. S. Lewis's assertion of bad temperature readings, but may have overlooked his silence at the previous month's NSF/EPRI conference. The Panel may have hoped that discretionary funds would be sufficient to discover what was wrong with the calorimetry. If that were true, then all experiments would revert to zero anomalous power.

Those who knew how government careers are made and broken recognized the obvious risk to any laboratory director who ventured to follow in the footsteps of those scientists who had been described in a publication of the American Physical Society to be "hucksters,"[3] and at their Baltimore meeting, to be "incompetent and possibly delusional."[4] It was only in the month preceding the Panel's final report that Dr. Bardon, at NSF, sent a memo to five hundred staffers referring to "such discredited work."[5] The Panel's final report clearly conveyed to government agencies that there was nothing of interest in the assorted cold fusion claims.

In particular, its final report frequently mentions the lack of ". . . convincing evidence, for useful sources [of energy] . . ." This wording constitutes a gross distortion of the Panel's assigned task, a distortion that allows out of hand dismissal of much experimental work. The instruction given by the Secretary to the Panel shows that he was wise enough to know that identifying a useful source of energy is far too much to ask of a review Panel. The Panel had no such charge from the Secretary.

On an ethical point, the Panel repeatedly mentions the discrepancy between the amount of heat that was claimed and the number of detected neutrons that were claimed, and does so without giving proper attribution to the discoverers of this important relationship. In the Executive Summary there was the statement, ". . . Others . . . report excess heat production . . . and [conventional] fusion products at a level well below that implied by reported heat production."[6] In no case was scientific protocol followed by giving proper attribution of this important discovery to Fleischmann and Pons.

There was something much worse here than lack of ethical procedures. The Panel *never* really evaluated the one observation which was the most unexpected to its discoverers. The Panel preempted the issue by commenting that, ". . . it would require the invention of an entirely new nuclear process . . ."[7] The panel did not mention that this was what was hypothesized both at the announcement and in the Preliminary Note. The implication here was that the Panel never clearly identified Fleischmann and Pons's claims.

A lack of neutrons in the presence of anomalous power was the principal surviving claim announced at Utah. The Panel used that relationship to indict what was announced, never separating the one from the other. Neutrons, if detected, the report said were, ". . . at levels 10^{12} below amounts required to explain the experiments claiming excess heat." The circular logic continues, "the present evidence for the discovery of a new nuclear process . . . is not persuasive."[8,*] That the lack of neutrons in the presence of unexplained heat might *itself* indicate the discovery of a new nuclear process seemed completely

* The paragraph in full, the fourth in the Executive Summary, reads as follows.

Neutrons near background levels have been reported in some D_2O electrolysis and pressurized D2 gas experiments, but at levels 10^{12} [1,000,000,000,000] below the amounts required to explain the experiments claiming excess heat. Although these experiments have no apparent application to the production of useful energy, they would be of scientific interest, if confirmed. Recent experiments, some employing more sophisticated counter arrangements and improved backgrounds, found no fusion products and placed upper limits on the fusion probability for these experiments, at levels well below the initial positive results. Hence, the Panel concludes that the present evidence for the discovery of a new nuclear process termed cold fusion is not persuasive."

SUMMATION

Charge to the DOE Panel on Cold Fusion

The Secretary's original instruction to the DOE-Panel on cold fusion was well drafted, reasonably specific, and yet sufficiently general in its requirements. Watkins charged the Panel as follows.

Specifically, I would like the Board to:

1. Review the experiments and theory of the recent work on cold fusion.
2. Identify research that should be undertaken to determine, if possible, what physical, chemical, or other processes may be involved.
3. Finally, identify what R&D [research and development] direction the DOE should pursue to fully understand these phenomena and develop the information that could lead to their practical application.*

* ERAB Panel on Cold Fusion, *Final Report of the Cold Fusion Panel,* (Department of Energy, Washington, D.C., November 8, 1989), p. 2.

beyond conception by the Panel. The Panel begs the question it was charged to investigate.

How did the Panel actually go about its work? A specific requirement to evaluate the Fleischmann and Pons claims was notably absent from the Secretary's charge. The Panel was left to seek its duty in the mélange of claims and counterclaims that followed the Utah announcement.

Organization

The DOE Panel, co-chaired by John Huizenga and Norman Ramsey, solicited and received a large quantity of formal and informal reports prepared by scientists who had been experimenting in the field since the Utah announcement. It submitted an interim report in July. Since this differed little from the final report, it will not be mentioned further, except to note that its content leant credence to the observation that the Panel formed its final opinions altogether too early in its work.

The first instruction to the Panel was to ". . . review the experiments and theory." What it did about the theory was to concentrate on the impossibility of generating excess heat by means of deuterium-deuterium fusion. How they dealt with the experimental part of Watkin's charge was central because experimental results are the driving force of new science.

Delegations from the Panel visited six laboratories during the summer months. It was not clear that they did much at the laboratories except visit, in the lightest sense of the word. Their visit to Salt Lake City, for example, ought to have been one of the most important. They arrived at the city in the evening and left the following evening. Subtract from that the time needed for the necessary auto travel, introductions, breakfast, lunch, and there are few hours left for the technical part of the agenda. Other laboratory visits were equally superficial.

Another example of the Panel's effort to, ". . . review the experiments," came at the Los Alamos National Laboratory. Two researchers there had many years of experience working with tritium and knew how to avoid contamination problems. They reported the generation of tritium from several cells. They were subsequently visited by one member of the Panel in an interview that lasted for just seventeen minutes. They never heard from the panel member again.

There was no indication of Panel members spending sufficient time in any laboratory to gain some acquaintance with the experimental work at hand. Two of the six visits were to laboratories that had reported generating no anomalous power. The Panel did not invite Fleischmann and Pons to meet with them in order to expand upon their press announcement, or to discuss further what they thought might be the nature of their discovery.

The visit to McKubre's laboratory was held at the offices of his funding agency, EPRI (at its request) not at the laboratory itself. In Chapter 23, p. 329, I review the visits where, several years later, three senior scientists each spent two full working days in his laboratory in a conscientious effort to evaluate his experimental techniques.* This is the minimum time and effort needed to evaluate technical work. (Langmuir and Wood participated in performing the experiments.) The Panel, by the scientific standards of the twentieth century, did not fulfill a reasonable review of the experimental activity.

Watkin's second instruction was, "Identify research that should be undertaken to determine . . . what . . . processes may be involved." The Panel recommended two excellent items where research should be undertaken. These are the report recommendation numbers 3 and 5.

> (3) The Panel recommends that the cold fusion research efforts in the area of heat production focus primarily on confirming or disproving reports of excess heat . . . [9]

No single, better recommendation could have been made. The other recommendation was also right on the mark.

> (5) Investigations designed to check the reported observations of excess tritium in electrolytic cells are desirable.[10]

* They were experts in the requisite specialties, and they concluded they could find no flaw in McKubre's calorimetry or experiments.

There were many reports of tritium being generated in cells. The Panel reported that ". . . a careful analysis of an electrolytic experiment must be carried out if one is to interpret the specific activity value of tritium after electrolysis . . . as anything other than electrolytic enrichment."[11] The lack of review of the experimental work becomes clear at this point. The Panel's two-paragraph discussion consisted entirely of a general outlook. The Panel simply did not learn whether researchers had already done the required ". . . careful analysis . . ." They made no attempt to find out about this during their seven-month investigation. Confirmation that tritium was being generated in electrolytic cells would be evidence of nuclear processes at work and thereby establish the research as a legitimate scientific activity. The government did not establish a calorimetric laboratory, or a tritium generation evaluation facility in the years after the Panel's report was submitted. Virtually nothing was done.

The sophistication of the Fleischmann and Pons experiment was underestimated not only by some physicists, as might be expected, but also by some electrochemists. The time required to evaluate the experimenter's laboratory efforts was also grossly underestimated.

It is relevant to estimate the size of these recommendations by the Panel. For the calorimetry research, the funds required had to be sufficient to establish and operate a laboratory commensurate in length of time, size, and staffing with that of McKubre's laboratory at SRI International. His laboratory took about five years and $6 million to accomplish precisely the task of recommendation number three. Recommendation five requires that the Fleischmann and Pons heat effect be achieved first. Accomplishing number five should take about the same amount of time and effort.

The last charge from Admiral Watkins was to advise what direction research and development should take in order to (1) fully understand these phenomena, and (2) to ultimately lead ". . . to their practical application." Repeated statements in the final report about the lack of a useful energy source does not constitute a legitimate substitute for fulfillment of this charge. The Panel failed to recommend any program under any caveats. It was quite firm that no funding be allocated to "fully understand" what was going on and what might be made of it. This instruction was utterly ignored in what might be termed a dereliction of duty.

Only one member of the Panel published a paper about his work on the Panel.[12] Allen J. Bard, chairman of the Department of Chemistry, University of Texas, Austin, Texas, had attended the EPRI/NSF conference the previous month. His paper set forth his conclusions as a member of the Panel. There was no indication that this work was done by a task group organized within the Panel; it appears to be his own evaluations and conclusions even though the reports to which he refers were those collected by the Panel and were available to its members.

Bard shows that for his analysis, he had available at that time only four written reports of anomalous power generation. In the discussion that followed his presentation, it was made abundantly clear that (1) the field was moving fast and (2) cells had to run up to 90 days to find out what they could do, and (3) there were many more successful experiments that would not be available in time for his analysis. Still there was no mention of the Panel taking an additional year or so to follow up on its soon to be released final report of November 1989.

It appears in retrospect that the DOE Panel tried to draw final conclusions too early. The schedule was imposed by the Secretary, but that would be an excuse, not a reason. People of the caliber appointed to the Panel know how to deal with such constraints. They could have recommended some continuing research in order to let the dust settle and gain some perspective. They could have recommended the Panel be reconvened in two years to reconsider its recommendations. Any continuing activity would have met the need. The Panel failed to be sufficiently circumspect. The Secretary's later attitude with which the chapter opened accurately reflected the Panel's report. At the highest levels of government, this brought official interest to a halt: no research, no patents, no collegiality.

How could the Panel have gone so far wrong? It went wrong simply by *doing no work.* In physics, work is only done if something is moved; pushing hard for a long time against a stone wall does not count as work. Members of the panel were properly industrious in their obligations to the Panel and its chairmen. They were organized and instructed, however, in such a manner that no real work got done.

In my earliest interviews with Panel members, I learned that there was no division of responsibility assigned among its 23 members. No select group of members were directed to examine the technical reports and laboratory work that was concerned with a particular kind of cold fusion claim, e.g., tritium generation, and then report their conclusions in an official sub-Panel document. No staff were available—possibly assigned to the Panel from a national laboratory—who might be given full-time laboratory visitation assignments for a few weeks and report their findings in a sub-Panel document. Such internal reports would have been all but binding upon the chairmen if written by officially appointed sub-groups with specific assignments. No organizational structure was established within the Panel that might prepare a sub-report that the chairmen would have to respect when drafting their final report.

The Panel was maintained as a hard-driving, amorphous body much in the manner of the Victorian salon described earlier. In this way, the Panel leadership was free to write its final report as it pleased, they being subject only to a vote of the Panel members or the threat of a resignation or minority report.

Revolt

There was a revolt, but it was one of delicate extent. As one astute observer pointed out, "It's fair to say that DOE went out of its way to get people [for the Panel] who are sensitive to political nuances."[13] Most panel members were selected from among those who were climbing a professional ladder of some sort, where a public scuffle would only disrupt their career plans. Fortunately, not every member adhered to that criteria.

Norman Ramsey, co-chair of the Panel, was a recent Nobel laureate with no further career ambitions. He was a free agent. He spent several months during the Panel's time hiking in Europe. At the very end, he threatened to resign unless a paragraph he had composed was inserted as the preamble to the finished report. His request carried.[14] We visit this preamble in Chapter 10, page 129.

The Panel's final report was delivered in November to Admiral Watkins. It is worth noting that the letter of transmittal carried the signature of only one of the two co-chairmen, that of Huizenga.

The past three chapters have provided a look at the politics of cold fusion research. It is time to move on to the scientific community's critique of the Utah claims, of which there were remarkably few.

CHAPTER EIGHT

The Critics: I

If the nation's chemists responded at all to the Utah announcement, they did so quietly. The nuclear physicists responded otherwise, and it was natural enough that many of them found it hard to accept the anomalous power claims of Fleischmann and Pons. Many assumed that the two chemists had quite simply misread their calorimeters. This assumption went unstated by the critics because they were not in a position to defend such a statement: they did not know calorimetry and, it came to pass, they had no intention of learning it.

If the measurement of anomalous power gained acceptance as valid, and if the level of neutron emissions was low, then Fleischmann and Pons's hypothesis that there must be some unrecognized or unknown nuclear process at work stands firmly in place. The announcement proved anathema to many physicists. A number of them devoted a significant period in their careers to a continuing deprecation of the claims of March 1989.

David Goodstein told a story that exemplified the instant write off of the Utah heat claims. He reported, "On the evening of the original Fleischmann and Pons press conference, I ran into one of my buddies at Caltech . . . 'What do you think?' I asked. 'If it were true, they'd both be dead.'"[1] It is of interest how casually the two claims were separated: the anomalous power claim was refuted; the unknown nuclear reactions hypothesis was overlooked.

The response of the nuclear physics community was also evident in an AP article just fourteen days after the Utah announcement. It was written by H. W. Lewis, a professor of physics at the University of California, Santa Barbara.

> In light of the laws of nature, it is probably worth putting the matter straight . . . We mortals cannot change those facts . . . Did the Utah team achieve cold fusion? No . . . The answer . . . should have been unambiguous, if only to cut off the kind of wishful thinking with which the country is now awash . . . That they lived to hold their press conference is clear and unambiguous proof that they did not produce any noticeable amount of power through cold fusion.[2]

This type of response, conveyed with absolute assurance, was well enough known in science to be recognized by its historians. They recognize it as a characteristic of a mature science. W. I. B. Beveridge, in his classic book, *The Art of Scientific Investigation,* states:

> Thus in subjects in which knowledge is still growing . . . all the advantage is with the expert, but where knowledge is no longer growing and the field has been worked out, a revolutionary new approach is required and this is more likely to come from the outsider. The skepticism with which the experts nearly always greet these revolutionary ideas confirms that the available knowledge has been a handicap.[3]

Huizenga displayed perfectly that perspective regarding a mature science. "As a field matures, as nuclear physics has over the last half-century," he writes, "the probability of a surprise becomes ever so much less probable."[4]

Outsiders

Huizenga considered the two chemists to be outsiders. Discovery by outsiders? "On rare occasions self-taught outsiders make a discovery in an area largely unknown to them, but this is, indeed, rare. Most fundamental discoveries are made by persons intimately familiar with their research discipline . . ."[5]

Was the announced discovery made in the field of physics or chemistry? It is interesting that this was the question that plagued the Nobel committee when it considered an award for Svente Arrhenius in 1903 for his discovery that the formation of ions is the change that makes water in salt solution electrically conductive. It decided the discovery was in chemistry, not physics. The same conclusion holds in this case. Fleischmann and Pons made their discovery in the field of chemistry where they are not outsiders. Having measured the anomalous power, and noticed that they were not fatally irradiated by the experience, what were they to hypothesize as a possible source? No critic has suggested a better hypothesis than theirs.

"The only evidence," wrote Huizenga, "for invoking a nuclear process was the claim that the magnitude of the excess energy was so large 'that it is not possible to ascribe this to any chemical process.'"[6] Such logic was wholly *acceptable* to him. At no point in his published writing and lecturing about the field has he suggested that there might be some other credible source for the excess energy than a nuclear source. What he could not accept was that a new nuclear reaction might have been discovered by "outsiders," e.g., chemists. Actually, Fleischmann and Pons did not claim discovery of a new nuclear process. They merely hypothesized that an unknown nuclear process exists, while offering no suggestion as to what kind of reaction might be involved.

By way of contrast, the orthodox critics were woefully indolent in casually assuming that Fleischmann and Pons's calorimetry was erroneous. These critics had little or no expertise in calorimetry: they were outsiders. They did not profess skill in it, nor have they mended this fault during the intervening twelve years. Yet they have persisted in their assumption that the experiment had calorimetric error without actually asserting such a position, for they did not have the necessary expertise to defend it.

Fleischmann and Pons's defense in the literature of their calorimetry was a *tour de force.*[7] Their training and established professional skills show what is required for other researchers who may wish to follow in their steps and develop this field further.

The outspoken physicists are a highly cosmopolitan lot. They are people who travel the world, and explore questions that reach to the origins of the universe. Why then were they so precipitous in their judgement about the heat and its consequent hypothesis?

Richard P. Feynman, Nobel laureate in physics, addressed a matter he called "Cargo Cult Science" in 1974. It was the story of unreasoned expectations that have no basis in reality.

> In the South Seas there is a Cargo Cult of people. During the war they saw airplanes land with lots of good materials, and they want the same thing to happen now. So they've arraigned to make things like runways, to put fires along the side of the runways, to make a wooden hut for a man to sit in, with two wooden pieces on his head like headphones and bars of bamboo sticking out like antennas—he is the controller—and they wait for airplanes to land. They are doing everything right. The form is perfect. It looks exactly the way it looked before. But it doesn't work. No airplanes land. So I call these things Cargo Cult Science, because they follow all the apparent precepts and forms of scientific investigation, but they are missing something essential, because the planes don't land.[8]

In their hasty skepticism, these outspoken physicists were acting like cargo cult scientists. They insisted that a nuclear source of energy must have lethal amounts of radiation because that was what they had always known. Once they stated that argument, they saw no need to look into the Utah heat claims any further. They simply assumed such heat claims would be abandoned in due course.

A fine example appears in the book, *The Undergrowth of Scienc*e,[9] where neutrons hold the place of honor as the expected cargo in the planes that never arrive. "Energy Unlimited," a chapter title, evaluates the claimed excess heat by the absence of neutrons at each stage of the unfolding episode. Even the early meltdown is dismissed as a mere hydrogen explosion because of the lack of lethal radiation. It apparently never enters the author's head that Fleischmann and Pons's calorimetry might be accurate and point to a new nuclear reaction.

The nuclear physicists were enormously interested in the Utah report of tritium and neutron generation in the tabletop experiment. Fleischmann and Pons claimed in their Preliminary Note to have measured tritium generated at the rate of 10,000 atoms each second in their cells, and neutrons in roughly similar amount. Their measurement of the neutrons was done indirectly. They had a gamma-ray detector mounted over the (light) water bath so that neutrons emerging from the cell would interact with the hydrogen in the water molecules (H_2O) to produce gamma-rays.

Those physicists dismissed the tritium measurements as probably due to contamination, and they then turned their attention to the measurement of neutron particles. This would lend itself to mere counting, a more pleasing prospect than heat measurements. Some nuclear physicists dismissed heat measurements as too amorphous for their taste.

During the winter of 1988–1989, Fleischmann and Pons could not easily approach the university physics department for assistance in measuring gamma-rays without exposing themselves to consequent incredulity. They obtained a gamma-ray detector from the University's health monitoring department. In their Preliminary Note, they offered what they claimed was the output signal from the detector that showed gamma-rays of the correct energy level so as to confirm that neutrons were being emitted from one of their cells.

It was natural that the physics community would look at radiation detection, counting, and energy levels as absolute sources of knowledge about what was taking place within the cell, because that was their stock in trade. When they examined the gamma-ray signals shown in the paper, some physicists quickly spotted serious flaws in the nuclear detection work of Fleischmann and Pons. R. D. Petrasso, a nuclear physicist with the Plasma Fusion Center at MIT, had followed the unfolding episode from its announcement. He was bothered by some details of the presentation that did not look right. For example, the detected signal as drawn in the Note had the wrong shape. He studied the Note, its errata published shortly afterwards, and TV news clips.

He argued persuasively that the signal shown by Fleischmann and Pons could not possibly be what they claimed it to be. He drew the conclusion that Fleischmann and Pons had not detected neutrons, nor evidence of neutrons.

Petrasso presented his data at the APS Baltimore meeting and it was published in *Nature* on May 18.[10] Fleischmann and Pons published their response in *Nature* claiming a new interpretation and presentation of the questionable signal. Their answer reduced their own estimate of neutrons by a factor of 1,000, but it also contained serious flaws, which Petrasso pointed out in an adjoining reply. Overall, Petrasso's critique was a thoroughly persuasive argument indicating that Fleischmann and Pons had not detected neutrons emanating from their electrolytic cell. At that point, *Nature* withdrew from the controversy by refusing to publish a further defense by Fleischmann and Pons.

In the eyes of scientists, this outcome was damning for Fleischmann and Pons. It displayed a lack of competence with nuclear measuring techniques that put a cloud over their other work. Fleischmann and Pons's credibility never recovered in the opinion of many scientists.

But the fact that Fleischmann and Pons did not do a good job did not of itself mean that there were no neutrons emitted from their cells. Professor M. H. Salamon, a physics department member at the University of Utah, gathered a team of nine scientists from the various technical departments.[11] During the months of May and June (1989), they placed a high-sensitivity neutron detector immediately below a bath containing four cells in the Fleischmann and Pons laboratory.

It is a little embarrassing to describe how they worked. It appears that Salamon and his cohort imposed themselves on the two chemists. (Salamon explains that the University president asked him to "get involved.") Although they asked if they could place their instruments in the laboratory, it was also clear that they and the chemists were not working cooperatively with each other. Fleischmann says that he suggested to them that they ought to put their instruments under (cell) bath number two. They insisted on bath number one, where the original work had been done. Much later, Fleischmann and Pons explained that the four cells in bath number one were planned to be less active, one was acting as a control cell. Clearly the two groups were speaking past each other. They were not working together and that is a requirement if research is to be productive. Salamon's results were published in *Nature* the following March (1990). He reported that no neutrons were detected from the experiments he monitored.

In the meantime, the editor of *Nature* had become cynical, if not angry, at the cold fusion episode. He had come to the conclusion that there was no science in cold fusion research, and refused Fleischmann and Pons space to rebut.[12] The report of Lewis at Baltimore and in the pages of *Nature,* and

Salamon's report in *Nature,* condemned the two chemists forever in the eyes of much of the scientific community.

Fleischmann and Pons published a rebuttal of Salamon, along with their new gamma-ray data in the journal, *Il Nuovo Cimento A,* the Italian journal of science, almost three years after Petrasso's criticism, and two years after the Salamon paper. By that time the scientific establishment was no longer listening.

Their article displayed the record of a gamma-ray signal that, they argued, resulted from a cell's neutrons interacting with the bath water. To detect the gamma-rays, they allowed the detector system to accumulate the signal over a long period of time. The estimated neutron rate was from 5 to 50 per second for each watt of excess heat power. The article was published in June of 1992 (see Chapter 18, page 256).

Steven E. Jones liked to count particles. He maintains a standing offer to take an operating electrolytic cell (provided by others) into a tunnel where there was low background radiation of neutrons, and measure the neutron count of a heat-generating cell.

One needs to know when to look for the neutrons. Conventionally, you look for them when the cell is generating excess power. Jones, however, denied that excess heat power could be measured satisfactorily. He had concluded that no one could really measure a cell's heat output well enough to know if a cell was generating excess heat. He considered calorimetry so enigmatic that it could not even give a yes or no answer concerning anomalous power generation.[13]

Jones would gladly measure a cell for neutron radiation if some other scientist brought to him what was declared to be a cell generating heat. As operating cells that are submerged in a water bath are not transportable, nothing has come of the offer. His overall results in the first decade were that he had measured no neutrons emanating from Fleischmann and Pons type cells.

At the Los Angeles meeting of the Electrochemical Society (ECS), Pons was persuaded that if fusion were really happening, helium atoms would remain in the palladium cathode where they might be found. Fleischmann was also convinced that the nuclear reactions were taking place in the bulk of the palladium cathode. By the end of the meeting, Pons had taken upon himself the task of permitting several of his cathode rods (electrodes) to be analyzed in independent laboratories to see if they held helium atoms. On the face of it, this was a reasonable proposition.

Robert L. Park, public spokesman for the APS, saw it as a *governing* test, ". . . the decisive test for fusion: is there helium in the Pons and Fleischmann cathodes?"[14] Six years later, he saw it similarly, "Everyone seemed to agree on just one thing: if there was fusion taking place, whatever the mechanism, there must be a huge buildup of helium in the lattice of the palladium cathode. Pons and Fleischmann, under great pressure to back up their claims, finally agreed to have their cathodes assayed for helium."[15]

Pons wholly misjudged the social dynamics of the process. By the time the sequence of steps in the tests had been completed, the national media and the scientific community would be watching for the outcome. If the results showed that no helium was found, the audience would be convinced, not that some single test had failed, but that their announced new science did not exist.

As the weeks went by, the world waited for the results to come in. It became clear to Pons that he had gotten himself into a do or die demonstration of the cold fusion claims. One of the first things an experimental scientist learns is the important difference between an experiment and a demonstration. You never offer to do a demonstration without being absolutely sure of the experiment. One rehearses the experiment thoroughly. After that, not a single change in procedure is to be permitted, no matter how small it may seem to be. Only then can the demonstration expect to go well. It seems incomprehensible that Pons allowed himself to be put into the position of performing a public demonstration without having done it previously. Pons apparently reneged on the obligation when it was time to reveal his data on the rods. His performance damned the field in the eyes of many.

In subsequent years, evidence emerged that a helium generating reaction does take place, but not in the bulk of the cathode. Rather, it takes place near its surface. The generated helium is to be found in the effluent gasses. Six years later, one could reasonably assert that no helium ought to be found in the bulk of the palladium except near its surface.

A similar situation presented itself during this period (May 1989) when the University of Utah allowed it to be public knowledge that some cathodes had been sent to a company in England for helium analysis. Park stayed in touch with the university so he could learn the results without delay. When the university was informed of the analysis results, they refused to give them to Park saying that the measurements would be presented in a scientific paper. That paper, published the following March, did not report that helium was looked for or found.[16] Park was incensed at the refusal and remained so seven years later. He marked that day, June 7, 1989, as the day he concluded that there was no science in the cold fusion claims.[17] That day was the end of the cold fusion episode for him.

From 1993 to 1996, whenever technical papers were presented that reported that helium was present in the effluent gasses, the orthodox physics community refused to publicly acknowledge the event. Contrary to what they had been saying, it appears that they really did not want to know about the evidence 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

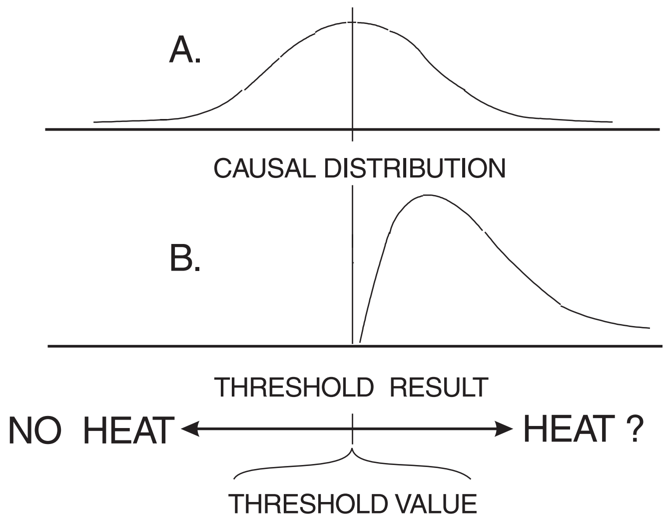

FIGURE 8.1 Illustration of the threshold effect on an otherwise smooth probability distribution curve.

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.

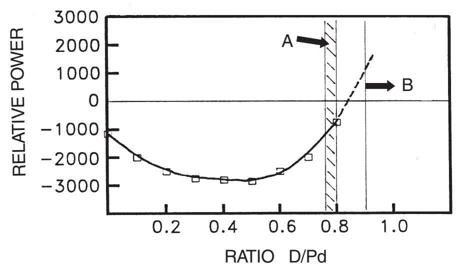

FIGURE 8.2 Fleischmann reported that as the palladium cathode stored more deuterium, its ability to generate anomalous power changed from negative to positive.

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.

CHAPTER NINE

The Critics: II

After the Utah announcement, severe criticisms arose about the presumed lack of suitable control experiments in the Fleischmann and Pons paper. These criticisms came both from individual physicists and from publications. Each demanded an experiment substituting light water for the heavy water because the deuterium supplied for the claimed fusion was presumably provided by the heavy water.

As was mentioned earlier, a dichotomy was implicit in the complaints because two situations needed to be tested. The control test for nuclear fusion required substitution of light water. The test for anomalous power required substitution only of an exhausted palladium or a platinum rod for the cathode.

In April 1989 the *New York Times* wrote an editorial in authoritative tones, "But the two [Fleischmann and Pons] apparently neglected a basic caution that scientists have learned to impose on themselves for fear of being carried away—a control experiment, like repeating the test with ordinary water instead of heavy water."

Dr. Huizenga was emphatic about the need for this control experiment. He said,

> Pons and Fleischmann failed to carry out a number of even the more elementary tests and cross-checks. When questioned about their results with ordinary light water, their answers were non-informative and subject to ambiguous interpretations.[1]

Neither the NYT nor Huizenga showed awareness of the experiment listed in the Preliminary Note showing zero excess power. It can be considered a control experiment. They appeared to be only interested in a light water control. By the summer of 1989, however, there were claims of excess energy generation in light water electrolytic cells. How does one know that plain water constitutes a control test, elementary or otherwise, if the energy source is unknown? They had no basis for asserting tests with ordinary water constituted "elementary tests and cross checks."

The editor of *Nature* (April 1989) scolded the two chemists for not doing control experiments with light water. Fleischmann insisted that he wrote to the editor pointing out that they had done work with light water and had data available, and that the editor did not publish his letter. That data appeared in the Fleischmann and Pons article of July 1990.

Wilson (1992) pointed out that maintaining the heat balance of a cell constituted a satisfactory control: heat in minus heat out must equal zero. If that balance was achieved, that demonstrated substantial control of the calorimetry.

Fleischmann suggested using a "dead" palladium cathode rod in an otherwise identical cell.[2] That change held most values in the cell identical with those of a performing cell, and did so with its cell heat balance and data analysis showing no generation of anomalous power. Attempts to use comparisons between light and heavy water electrolytes were unsuccessful as the change involved many difficulties[3].

W. I. B. Beveridge insists, "Unless the basic needs of the control experiment can be satisfied it is better to abandon the [experimental] attempt." True enough. Did achieving heat balance to 1% constitute a satisfactory control, or did using a "dead" palladium cathode do the job? Orthodox scientists treated the design of a control as a trivial matter, but it was not.

The design of a control experiment is not obvious in a new area of science. Only when its boundaries are marked out can an experiment be designed whose function lay outside the effect of the new area of scientific interest. By the end of 1994, it appeared that the exhausted palladium rod, or a platinum rod, offered the best control experiments for tracking excess heat experimentation.*

After the announcement, there emerged a demand for a Fleischmann and Pons data set that demonstrated the excess heat characteristics as claimed.†

* Chemists assume that the platinum cathode rod will be inert.

† A data set, presumably, would consist of three columns of figures: cell temperature, cell voltage, and clock time. One such data set would be for a control cell. There would be associated items, such as the value of the heater calibration pulses introduced into the cell for reference purposes.

The demand came from both the physics and chemistry departments of the University of Utah. In response Pons allegedly made a promise to furnish a data set and the promise was not kept. If that happened, it was unfortunate. Such a data set might have brought the physics and chemistry departments into the project in a constructive way.

Four Critiques

The principal claim of Fleischmann and Pons's was an extraordinary amount of output energy that emerged in the form of heat. University of Utah President Peterson suggested at the announcement that during the ensuing years the scientific community would have to evaluate the claims. Critiques were published during the years 1989 through 1994 in response to the Fleischmann and Pons publications from April 1989 to December 1994, which together constituted their definitive statement of heat generation in the experiment.

Immediately after the Utah announcement, four major centers for nuclear research hastily assembled several experimental programs in an effort to duplicate the Fleischmann and Pons phenomena: Caltech,[4] Pasadena, CA; MIT,[5] Cambridge, MA; Yale University,[6] New Haven, CT, working in conjunction with the Brookhaven National Laboratory, Brookhaven, New York (Yale); and the British nuclear research center at Harwell,[7] England. All, except Harwell, had to scavenge the newspapers and television channels for technical details on which to base their programs. Harwell had considerable assistance from Fleischmann. All four assumed that neutron particle radiation, rather then heat, would be the critical evidence in support of the Utah claims.

Within three months, the four research centers complained that they were seeing nothing of scientific interest in the cells they had built and operated for however many weeks. This turned out to be their final conclusion.

Only Harwell, to its credit, made available their original "raw" data for review by others. Fleischmann prepared a paper in the spring of 1995 giving his review of their data, which showed numerous instances of apparent excess heat generation in one of the cells.[8] Unfortunately, he says there was not enough calibration data to reach a definite conclusion.

Questions had also been raised whether the MIT cells might have generated unrecognized excess heat. The original data reduction steps were never released for independent review and publication.

N. S. Lewis, Caltech, produced the most damaging criticism of the Fleischmann and Pons calorimetry because his commentary was absolute and it was carried far. He claimed he had replicated the Utah experiment in Pasadena and found it wanting. In particular, Lewis measured widely different temperatures at assorted points within the electrolyte volume. Without a uniform temperature, the calculations of excess heat were meaningless and could be dismissed by the scientific community.

Lewis's replication was faulty. The Dewar used by Lewis in his attempt at replication was too large in diameter and used one atmosphere pressure inside the Dewar walls. Mixing of the electrolyte fluid produces a uniform temperature if it is vigorous enough. The bubbling action of the electrodes stirs the cell, and this stirring is adequate only if the cell is sufficiently narrow. Lewis's was not.

The cell is immersed in a bath of cooler water for the purpose of measuring the cell's emission of heat. Fleischmann and Pons had the ingenious idea to get the heat out of the cell by *radiation* rather than by conduction.* A hard (high) vacuum inside the walls of the Dewar cell accomplished this. In this radiation mode, the heat propagated from all points within the cell electrolyte volume through the glass Dewar walls to impinge on all points within the volume of the bath water. Thus there need be no temperature gradients, only a temperature difference between the electrolyte volume and the bath volume.

In the Lewis cell, heat was removed from the cell by *conduction* to the inside Dewar wall; from there it moves across the air gap to the outside wall and from there to the bath water that touches the flask. Along this conduction path, one can expect significant temperature gradients because gradients are what make heat flow in the conduction mode.

Lewis's cell necessarily did have the large temperature gradients that he demonstrated, but he was mistaken when he assigned that fatal problem to the Utah cell. Most experimenters have learned that there are many subtleties involved in the Fleischmann and Pons cell design.

Our understanding of Lewis's criticism on May 1 lacks perspective because he never defended his work after his targets had an opportunity to learn what he had done and said. However, he did give a retrospective interview to Douglas Smith of the Caltech in-house magazine *Engineering & Science* (as did Koonin) but when his interview tapes were offered to the Cornell archive, Lewis did not release them.† He has not defended his work at subsequent scientific meetings. In particular, the NSF/EPRI meeting, October 1989, saw Fleischmann present his results for many cell experiments, a large fraction of which were claimed as generating excess heat. Yet Lewis did not rise to point out error in Fleischmann's procedure. Because of these several reasons, it is difficult to put an analytical perspective on his reasons for being so critical of the Fleischmann and Pons cell.

* With conduction, each hot spot heats the adjacent cooler spots. With radiation, each hot spot radiates its heat outward to all cooler spots within "sight" in a transparent medium.

† Lewis retains these tapes in his possession. It would be a great service to science if they were released to the Cornell/Kroch archive.

From a later perspective, these four programs were undertaken unrealistically. The lack of information about what Fleischmann and Pons were doing combined with a short experimental period were serious obstacles. Researchers who were not immersed in the techniques of loading hydrogen into metals and who did not have calorimetric skills were at a disadvantage. These research efforts should not have been expected to succeed even though there is skimpy evidence that MIT and Harwell did generate excess heat.[9]

The seminal paper of Fleischmann and Pons that described their calorimetry was published in July 1990. The scientific community gave this claim of excess power a paltry review. As far as I know, only four critiques of their work on calorimetry techniques have been published.*

While numerous papers and three books have critically referenced the original Fleischmann and Pons work, those full-length, published papers that undertook a direct critique of the calorimetric claims of the two chemists are included here. The books do not discuss calorimetry.

The legislature for the State of Utah had allocated five million dollars for further study of their discovery under the condition that the excess heat claim be confirmed. As one part of that requirement, Wilford N. Hansen, professor of physics, Utah State University, Logan, Utah, was commissioned by the Utah State Fusion/Energy Council to make an independent critique of several sets of cell performance data delivered to him by Fleischmann and Pons. Two of the cells were control cells identical to the active cells except that ordinary water replaced the heavy water. Besides the data sets, he was given ancillary information about the individual cells.

Hansen's report to the Council was delivered in the spring of 1991. His conclusions regarding the Fleischmann and Pons cell data are of interest. He expresses the amount of energy in units of electron-volts per atom of cathode material.† For evidence of anomalous power in cell number five, he states, "Just for the two days [out of the cell's operating period] this corresponds to 45 electron-volts (eV) per palladium atom. This amount is already ten times larger than the energy it would take to vaporize the entire palladium electrode. We have thought of no other self-consistent explanation than that the excess heat is real and very significant."[10]

For cell number two, the analysis is described as follows, "The integrated excess heat is . . . about 1,700 eV per palladium atom. This is about 400 times

* There are, of course, many dozens of papers on the subject of calorimetry which list the Fleischmann and Pons reports. This chapter reports on those papers whose content is a specific critique of Fleischmann and Pons's work as published, and where their abstracts and conclusions offer amendment to the Fleischmann and Pons papers.

† The electron-Volt is a measure of energy used by scientists at the atomic level of calculation. Here Hansen is calculating the excess heat generated for each atom of palladium in the cathode.

the vaporization energy of palladium for the electrode of cell 2!"[11] For cell number six, he said, ". . . there is about 6,000 eV per palladium atom excess energy, or over a thousand times the energy required to vaporize the electrode. Putting it this way, . . . we are not dealing with known chemistry or metallurgy. At issue is a profound energy source."[12] For cell number 5, he calculated heat generation at the rate of 1,000 watts for each cubic cm of palladium cathode. Such a value is comparable to the power density within the fuel rods of a nuclear reactor. These are the conclusions of an independent scientist after conducting an independent data reduction of several sets of Fleischmann and Pons's data.

The Wilson Critique

A more aggressive and most revealing critique emerged from a group led by R. H. Wilson at General Electric Co., Schenectady, New York.* They submitted a paper criticizing the calorimetry work in the original article by Fleischmann and Pons.[13] The Wilson paper was published in July 1992 and was followed directly by Fleischmann and Pons's response.[14]

These papers were prepared in the proper manner that included peer review before the publisher accepted them. They got to the heart of the excess power question. Whether cold fusion was a science or not hinged on precisely what was in these three papers: is there, or is there not, an anomalous source of energy which appears as an excess of heat in the Fleischmann and Pons experiment? These papers were the proper battleground. The passage of three years had allowed for the critics to find themselves and their weapons, so that the two sides engaged each other properly fitted out with the necessary information.

An overview of the Wilson critique shows it to be quite limited. The authors found it necessary to qualify their comments with, "appear," or "possible," or "probable," or "potential," which reduces the value of the criticism. It is interesting that they stated, ". . . inadequate mixing within the cell does not appear to be a problem," without further discussion.†

The Wilson team summarized their paper in the following words.

> We evaluate the data and methods of Pons, Fleischmann and coworkers and, where sufficient data are available, conclude that they overestimate significantly the excess heat . . . While our analysis

* It should be noted that GE had a financial incentive to reach a negative conclusion. They wanted to back out of a research contract.

† A précis of the report is included in the appendix.

> shows their claims of continuous heat generation to be over stated significantly, we cannot prove that no excess heat has been generated in any experiment.[15]

In their response, Fleischmann and Pons pointed out that the Wilson calculations still showed excess heat after taking into account their corrections, in one case at the 50% level, far above the uncertainty floor. The Wilson report was not negative. It was supportive in that there was still excess heat after all the criticism Wilson could muster.

The argument between Fleischmann and Pons and the Wilson group was over the manner of computing excess heat energy flow. Regarding the burst of excess energy shown in the original paper (see Figure 4.2), Wilson said, "the 'burst' data [Fleischmann and Pons] present is not greatly reduced by the corrections that we describe." They also state that, ". . . the possible recombination of oxygen and deuterium within the cell is apparently eliminated . . ." So in three crucial areas, that of the recorded burst of energy, the uniformity of temperature within the cell, and the possible recombination of gasses, the Wilson critique supported the Utah chemists' techniques and claims.[16]

Wilson's report also supported the claim of the existence of anomalous power. The authors allowed that several of the cells still showed significant power even after their values were recalculated. In one cell, after Wilson's recalculation, the power amounted to four watts per cubic cm. of palladium and the total amount for the run amounted to four megaJoules of energy. These quantities were beyond what chemical reactions can provide. That the Wilson team at GE did not follow up the Fleischmann and Pons defense with further analysis is a pity.

The four critiques were as follows in the order of their publication. Professor N. Lewis's critique was published in *Nature* in August 1989;[17] Professor W. Hansen's review of the Fleischmann and Pons's data reduction techniques was published in June 1991;[18] the Wilson team's GE critique was published in *JEAC* July 1992;[19] and Dr. D. R. O. Morrison's critique was published in *Physics Letters A,* February 1994.[20] In my assessment, the N. Lewis critique was too hurried to withstand the rigors of comparison with analyses developed over a longer period of time. The evaluation of his critique given to the press at Baltimore (press conference number 3) was mistaken: ". . . the level of sophistication of the current round of experiments is far greater than the level of sophistication of the original Utah experiments."[21]

W. Hansen's work appeared completely credible, and no question was raised about it in the literature. Its strength lay in the multiple calibrations and multiple reduction methodologies he used. This permitted a comparative analysis of the different methods to assure a high degree of internal consistency.

Dr. Morrison's critique is not reviewed here as it was based upon a series of misunderstood and misinterpreted capabilities of the open cell. When these were set forth, he did not respond.

Wilson et al. offered the most thoughtful and comprehensive critical review. It still fell seriously short of what was needed because it did not recognize the central position of earlier critiques and did not take them into account. I am disappointed by its own lack of self-awareness. It concluded with evidence for both substantial anomalous power and equally substantial *disappearing* power as expressed in their recalculation of the Fleischmann and Pons paper.* Nothing was said in the text about either result.

Blame for the lack of more rigorous critiques of Fleischmann and Pons's work must be laid at the feet of the Department of Energy's Panel on cold fusion which was given the explicit charge to undertake a rigorous critique and with those physicists who have turned the scientific community away from its institutional obligation to fulfill such a critique.

Calorimetry at BYU

In December 1993, S. Jones, Brigham Young Unversity, abandoned his claims that he had detected neutron emission from an electrolytic cell experiment of his own design. Since that time he has become a prolific debunker of claims made under the broad umbrella of cold fusion research. He was a co-author of technical articles criticizing cold fusion calorimetry that are discussed in the following paragraphs.

It is important to distinguish between the hasty work of 1989 and the much more careful work of the subsequent five years. In 1995, Dr. Lee Hansen, Professor of Chemistry at BYU, reported on a series of experiments completed in the previous year whose purpose was to demonstrate that anomalous power could be explained as a misinterpretation of electrolytic cell operation.† The source of error claimed was primarily that of the inadvertent recombination within the cell of some portion of the two evolving gasses. This recombination released energy that might be mistakenly seen as excess heat energy. His conclusion stated that, "there is no compelling reason for not adopting the hypothesis that calorimetric errors or failure to account for reactions of hydrogen and oxygen during the electrolysis of water account for all reports of excess heat to date."[22] If the reader will allow me a liberty: there is, for that matter, no compelling reason for not adopting the hypothesis that L.

* Their recalculation of the Fleischmann and Pons work produced answers with negative generated excess heat.

† Although the date shows this paper to be beyond the six-year span of our present inquiry, we review it because of its central interest to our purpose.

Hansen has a fetish for his computer. The quoted statement of conclusion by. L. Hansen, et al. is not an intellectually serious formulation: it is a manipulative ploy. Nevertheless, it should be answered.

He made the claim that his conclusion was applicable to all anomalous power reports (to date). His paper did not include data to allow that conclusion. It's text does not consider the variety of heat power experiments and note for the reader how his experimental results can be interpreted for each instance. At the time the paper was in preparation there were about twenty-five reports in the literature claiming heat power, and they included a wide variety of cell designs.

For example, L. Hansen's report shows that his "excess heat" ends when a partition was placed between the anode and cathode. Oriani used a glass partition in his experiment the report for which was submitted to *Nature* in September 1989 to corroborate the Fleischmann and Pons claim of excess power.[23] Hansen's report, then, was mistaken when, in the abstract, it said, "All [cells] produced excess heat as defined and calculated in the literature reports, but the production could be readily terminated by the introduction of various barriers to the migration of hydrogen and oxygen." But L. Hansen's cell did not produce excess heat in the manner exhibited in the Oriani report.

L. Hansen's report was for an experiment operating at a level of a few milliwatts of power. The experiment displays the minutia of electrochemical cell operation. It was an excellent experiment with which to acquaint graduate students with the electrolytic cell in its many vagaries. Still it claimed, without further comment, that it undermined the conclusions of all reports of excess power. The report's conclusions were an unconscionable exaggeration of the experiment's significance.

What L. Hansen was critical about can be shown as follows. In Figure 1.1, a qualitative picture of the observation of anomalous power was graphically depicted. The evidence for anomalous power was inferred from the direction and shape of the two tracings. The next step was to attach quantitative readings to the figure.

The amount of energy in and out of the cell is known from the referenced report for the experiment. The current and voltage delivered by the power supply that created the large volume of bubbles amounted to 210 kiloJoules over the four days shown. This report also stated that 26 kiloJoules of excess heat was measured. Hanson's argument was that if 13% of the gases recombined, the energy released would amount to 26 kiloJoules. The experimenter then would mistakenly think it was anomalous power when actually it was heat released by gas recombination in the cell.

Fortunately, Fleischmann took measurements in this experiment from which the amount of recombination was determined. They not only measured the quantity of the effluent gasses, but also the amount of heavy water that had to be replaced. The referenced report stated that the measured recombination was less than 1%. That amounted to less than 2.13 kiloJoules. Note that Figure 1.1 represented 45, 66, 86, and 115 milliwatts of power for each of the four days. A maximum of 6 mw out of these quantities might be attributable to recombination during those four days of cell operation.

L. Hansen referred to the question Fleischmann and Pons asked about the depiction in Figure 1.1. He said, "This may answer the question . . . 'How can it be that the temperature of the cell contents increases whereas the enthalpy [power] input decreases with time?'"[24] Recombination of gasses was clearly not the answer. If recombination occurred, control cells would experience it and show apparent excess heat. Fleischmann and Pons's control cells show no excess heat.*

Fleischmann also measured the water replacement volume and found that the amount required indicates that no significant recombination of the gasses occurs.[25] McKubre, Huggins, and Oriani have run "closed" cells that generated excess heat. These and other closed experiments indicated that the well-known recombination factor was well controlled by experimenters and was not degrading the reported anomalous power data.

At the NSF/EPRI conference (Chapter 6), October 1989, Fleischmann said of his cells:

> The current [Faradaic] efficiencies . . . were determined by measuring the combined rates of gas evolution from the cells . . . these efficiencies were higher than 99% as was also shown by the record of [heavy water] additions . . . Such high efficiencies have now also been reported in other work. They can be understood in terms of the inhibition of deuterium oxidation at the anode by Pt-oxide formation and the extensive degassing of the oxygen content of the electrolyte in the cathode region by the vigorous deuterium evolution.[26]

In the same article, Fleischmann and Pons described cells that generate anomalous power and control cells that do not. If the heat generation were due to recombination, both kinds of cells would display a similar performance since their geometry was identical. They would generate apparent anomalous heat in cells that experimenters found to be easy to replicate. Therefore, many scientists, when considering whether excess energy exists, reasonably conclude that recombination effects are insignificant.

Bursts of energy were noted in Chapter 4, Figure 4.2. According to David

* Fleischmann and Pons point out that ". . . the comparison of the precision and accuracy of the heat transfer factor for 'blank cells' sets an upper bound on the rate of reduction of oxygen in the system." They conclude that, "The magnitude of the source can be estimated to be ≈ 2.3 milliwatts for the example illustrated." So they agree reasonably well with Hansen on the existence of the effect, but they point out that the consequence for it in their published reports of anomalous power is negligible.

B. Buehler, BYU, these might be of chemical origin.[27] He suggests that a buildup of chemical layers on the cathode might fracture and result in a sudden release of chemical energy that would be mistakenly interpreted as a power burst of nuclear origin.* He would have to do some rudimentary calculations at least to show that an appropriately large quantity of power could be produced by such a source before his criticism could be taken seriously. Such a calculation would fail to account for the magnitude of the observed bursts. The amounts of energy under discussion here are many orders of magnitude greater than this mechanism would allow.

The outspoken physicists, who were involved in the public debates about the Fleischmann and Pons claims, did not give recognition to the heat flow measurements. Amazingly enough, the physics community consistently took the position that heat flow could not be measured, at least for the purpose of confirming the Fleischmann and Pons phenomenon. After their fine work in the nineteenth century, they largely abandoned heat measurements to the chemists. Scientists in this field, as *acceptors,* were motivated by the apparent existence of excess heat. Thus, much of the confusion.

S. Jones was explicit about that attitude. "Our experiments on heat suggest that you need some sort of additional signature in addition to the heat to confirm that you are really doing something and not just getting a systematic error."[28] Jones insisted that this point of view, ". . . is based on our [heat] experiments." Jones wanted to see, for example, x-rays, because he recognized them as a necessary concomitant of a nuclear process. The thermal measurements could be considered mistaken, as Jones considers them to be mistaken, without the evidence of a nuclear process to provide the large quantity of heat. In this way, the heat measurements of more than a dozen laboratories over six years are held hostage by a coterie of nuclear physicists.†

There seemed to be a nihilistic quality about the reports from BYU on the operation of the electrochemical cell. Experience from the routine use of such cells since their inception in the eighteenth century resulted in a large body of knowledge. This was routinely taught in graduate school. Ordinarily, one assumed these matters to be well considered in any experimental setup. Fleischmann and Pons clearly stated that those considerations were accommodated in their work, "recombination is less than one percent." Still L. Hansen was concerned that the avoidance of recombination "has not been proved." This sense of nihilism was reinforced by what was previously described as ex-

* ". . . if a lithium layer is deposited on an electrode [cathode] under a coating (e.g., silicate, borate, or aluminum coating) and later should the coating crack, then exothermic water-lithium reactions would result, producing 'heat bursts.'"

† I was concerned here with the calorimetric critique of Fleischmann and Pons by BYU. The further topic of the relationship between calorimetric and nuclear demands is developed in the chapter on protocols (p. 174) after the validation evaluation is begun in the next chapter.

aggerated claims, such as the phrase ". . . that probably invalidates all the currently available reports . . ."[29]

It is as though one were to step forward and declare that all claims of excess heat were due to the use of dirty glassware, and do so on the grounds that none of the papers in the field include proof that the experiments started with clean glassware. A review of those papers would show that they do not even mention whether the glassware was washed. To raise the argument is to imply that cold fusion experimenters, including Fleischmann and Pons, are utterly inept until proven otherwise. The two chemists' defense in the literature of their calorimetry during those years does not support such a conclusion. Quite the opposite seems to be the case. The Fleischmann and Pons paper of July 1990 is likely to establish a higher standard for the calorimetry of heat flow, one that measures heat power to 1% accuracy while, at the same time, permitting rapid temperature excursions.

Chapter 14 (p. 192) includes discussion of the experiments of 1990 in which recombination is deliberately induced, so that the results would be affected only by a *lack* of recombination, and that in the direction of decreasing any measured anomalous power. I agree with McKubre, who has said that L. Hansen's work is "mischievous."

The Two Chemists's Mistakes

That statement brings us around almost full circle. If Fleischmann and Pons were correct in their claim of excess power, why all the fuss and confusion? The broadest answer is simply that the claim of having achieved sustained room-temperature fusion captured the intellects and egos of many nuclear physicists. The chemistry community largely ignored the separate claim of excess heat without radiation.

The two chemists made some serious errors in both science and protocol. The most egregious was that of publishing erroneous nuclear measurements. The data included a picture of the claimed signal shape of the radiation. It did not conform to any expected shape from such a reaction, and when that wave form was questioned there was no forthcoming explanation about where it did come from. That item remained a mystery. Their early nuclear data showing that gamma-rays were detected emanating from the water bath surrounding the cells were false.

The other area of concern was the way they offered up their control cell data. Scientists missed the importance of a published experiment that produced zero excess energy simply because their heads were turned by the word *fusion.* They wanted a plain water control. The two chemists did not include one, even though Fleischmann later said that such data were available at the

SUMMATION

Fleischmann and Pons's Errors of Protocol

The following list provides a summary of the mistakes of Fleischmann and Pons.

1. Their statement of announcement in 1989 was substantially inadequate. For want of supervision the role of the attorneys was not limited and the two chemists did not offer coherent public statements for the news networks.
2. The two should have provided briefings for the University of Utah chemistry and physics departments prior to the announcement.
3. The Preliminary Note showed a column of calculated values pertaining to an hypothetical experiment (see Chapter 5, p. 73). It claimed a large power multiple of heat generation. The column of "data" was unsupported in the text. Its inclusion was improper.
4. The measurement they claimed of gamma-rays from the experiment's bath were more than wrong. It is not discernable where the published wave form came from. Their defense of those measurements was not persuasive.
5. It was an error of judgement for them to conduct a public search for the presence of helium in the cathode rods.
6. They failed to share their cell data with other scientists (other than W. Hansen). Under the circumstances of the first five months of this episode, that was a significant failure.

time. Both of these errors had major, deleterious consequences for the presentation of their work to the scientific community.

It has been said that their primary error was the failure to have a preprint report of their discovery at the press conference. My observation is that this item of protocol is well honored in the breach.

In retrospect, it was three years before the topics in their Preliminary Note of April 1989 were published in formal reports. I accept Fleischmann's repeated insistence that they wanted and needed another eighteen months prior to the public announcement, but they did not get it. Too little information was released. The published "news release" could have been much more complete in its descriptions.

The most unforgivable act was to have Fleischmann and Pons announce their work to the press speaking extemporaneously. Just the thought of it is staggering. What they said that Thursday afternoon ought to have been crafted purposefully even if it meant staying up the entire night to do it. It is said that Pons got a little stage fright lecturing a classroom. His public (TV) appearances during the past decade displayed a pathetic inability to speak extemporaneously on the record. Fortunately, Fleischmann spoke well and came through just fine. He emphasized the accuracy of their heat measurements by use of the Dewar flask. He expressly separated their major discovery—the large amounts of heat energy—from the nuclear measurements of fusion products. That was not enough to save the conference. Overall, the announcement content was utterly inadequate to the occasion.

Martin Fleischmann and Stanley Pons now fade from our narrative.* Their contribution was set forth in its principal parts along with their errors. The critique of their work, such as it was, has been presented in perhaps too much detail. During 1990, the burden of their considerable celebrity, the ongoing research needed to support publication of their theses, and the overwhelmingly adverse publicity made their positions in the chemistry department at the University of Utah untenable.

The two were offered an opportunity to start afresh by Minoru Toyoda, a senior member of the Toyoda clan, the founders and principal owners of the Toyota Motor Company. He arranged for the Technova Company of Japan to return them to their research. A laboratory was set up to their specifications in a technology park in Provence, France. They worked together there from 1992 until 1995, when Fleischmann left. Pons left in 1998 and the venture was ended. At the ten-year anniversary, Fleischmann was still active in retirement attending the international conferences and contributing much to them. Pons had left scientific employment and had purchased a farm in Provence. There were some indications that he was doing a modicum of consulting work.

The calorimetric work of Fleischmann and Pons, described in the first four chapters, displayed real evidence of a new phenomenon. This review of the critics' arguments, along with Fleischmann and Pons's laboratory expertise, did not evidence ignorance, oversight, or illusion on the part of the two chemists. By finding the measured heat, documenting it, and defending their discovery with skill and erudition against knowledgeable critics, they demonstrated that they were engaged in a scientific discipline. Fleischmann and Pons were working as scientists doing scientific work during the years 1984 to 1998. Whether they are eventually proved right or wrong, that field of study was a field of true scientific evaluation.

This critique of the first years of cold fusion research does not imply that the scientific community ought to have accepted outright any of the Utah claims. My argument continues to be that the door of opportunity to evaluate their research should have been left open a little longer than five weeks. More knowledgeable critiques became possible in 1990 after the publication of Fleischmann and Pons's calorimetric methods.

An evaluation of the failed experiments was raised in *Nature* magazine in

* For the sake of stylistic variation, the Fleischmann and Pons type of cell with its palladium cathode, platinum anode, and heavy water eletrolyte henceforth will be referred to as the "eletrolytic cell" when such use does not introduce ambiguity.

its issue of October 26, 1989. Its Washington editor expressed the argument as follows.

> Critics, on the other hand, maintain that if you are allowed to keep positive results and throw away the rest you can never be proved wrong: it becomes, as one skeptic put it, religion, not science.[30]

This statement was categorically wrong and widely influential. Most scientists are aware that one can not prove a negative, even about cold fusion research. There is no primrose garden path to a knowledge of nature even if our most prestigious societies and journals state otherwise.

Galileo liked to lecture that he could see moons circling Jupiter with his new telescope. Corroboration of his observation would require that one build a telescope as good as his, and eventually others did. A similar situation arose with the calorimetric instruments used in cold fusion studies. Fleischmann and Pons made an outstanding calorimeter: an utterly simple, open type, that was astonishingly accurate, and offered considerable dynamic range. It took several years for others to reach that standard of accuracy in order to validate their work. No one has yet come up to their standard in all three characteristics: simple, accurate, and dynamic.

The scientific critique of the Fleischmann and Pons anomalous heat claims ran thin. It can be concluded that by the end of 1994, the critiques held no evidence of a deficiency in Fleischmann and Pons's claim to have measured significant amounts of anomalous power. With that, our review of the critics and their works is completed.

Part Three

VALIDATION

CHAPTER TEN

Ramsey's Way

Here begins a description of how the anomalous power claims were validated during the years 1989 through 1994. Chapters 10 through 14 provide respectively the principle invoked, the variety of methodology recognized, a suitable protocol identified, the specific criteria selected, and the abundant laboratory data analyzed. An exhaustive reference list of anomalous power documents for that time period completes the validation.

Norman Ramsey found himself in fortunate circumstances. While he and John Huizenga headed the DOE's Panel on Cold Fusion as co-chairmen, Ramsey had drafted a preamble and insisted it be included in the Panel's final report on threat of resignation. His threat carried his request. Those who work in cold fusion research are indebted to him for his pugnacity.

The preamble reads as follows.

> Ordinarily, new scientific discoveries are claimed to be consistent and reproducible; as a result, if the experiments are not complicated, the discovery can usually be confirmed or disproved in a few months. The claims of cold fusion, however, are unusual in that even the strongest proponents of cold fusion assert that the experiments, for unknown reasons, are not consistent and reproducible at the present time. However, even a single short but *valid* cold fusion period would be revolutionary.[1]

If the experiment is a complicated one, then "a single short but valid cold fusion period would be revolutionary." At the very least, it would establish a new field of scientific research. This statement of Ramsey's is quoted with my emphasis on the word *valid.*

The definition of a complicated experiment needs some refinement. It certainly does not mean that a large team of scientists is needed. Nor does it mean that highly sophisticated facilities, like accelerators, are involved. A complicated experiment is one that contains uncontrolled factors sufficiently influential to determine the experiment's outcome.*

Unfortunately, the cold fusion cell experiment is a complicated one. It was compared to hot fusion experiments earlier in Chapters 4 and 5, where some commentators have pointed out that the hot fusion toroid contains a *simpler* experiment than does the Fleischmann and Pons cell. The two chemists who designed the cold fusion cell have had several decades of professional experience working with electrolytic cells. One picks up a lot of know-how in that time. There were many variables in their cells that were not understood but were important enough to determine the experiment's outcome. Ramsey's criterion clearly applied to cold fusion research, and it can help to untangle the confusion around this subject.

The natural warmth of radium when it was first measured offered a fine example of a complicated experiment. It was proper that Pierre Curie's heat measurement was not held hostage for three or more decades until science reached an understanding of the source of the warmth. It was correct for science to recognize the heat measurement while accepting the fact that it had no understanding of how the measured effect was possible.

During the first half of the twentieth century, the Wassermann test for syphilis was used in thousands of clinics because of its great utility. But it was never completely determinate, as is shown in the next chapter where a closer look is taken of the well-known test that had some of the characteristics of the cold fusion phenomenon.

In the 1970s, there was much argument over the properties of the surface

* *Science* journal addressed the use of the word "complex" at some length in an essay in its issue of vol. 284, April 2, 1999, p. 79. (I prefer the word "complex" to "complicated" for my purposes, but have chosen to stay with Ramsey's word "complicated.") The essay asserts, "Being anxious to move beyond the semantic debate, we have taken a "complex system" to be one whose properties are not fully explained by an understanding of its component parts." They go on to invite each of their authors ***to include their own definition of "complex"*** within their technical articles. The reader who questions my reliance upon Ramsey's idea that there can be defined an entity called a "complicated experiment," ought to read the editorial.

Its purpose is identical to mine. The field of research called cold fusion needs this concept exactly as *Science* needs it for the disciplines of earth sciences, molecular biology, chemistry, and so on. Also, it is with some satisfaction that I find *Science* railing against "the small, elite group of scientists whose ideas provide the theoretical underpinning for much of what is reported here." I am sure that some readers will see parts of this book as so much railing against the small, elite group of scientists whose ideas provide the theoretical underpinning to ignore the reports of anomalous power.

of gold, whether it was hydrophilic or hydrophobic. Many years were needed to sort out the confusion which, it gradually became known, was due to the fact that the surface, after cleaning, became contaminated much more rapidly than was at first realized. That contamination then served to determine the surface's chemical properties. The surface of the Fleischmann and Pons palladium cathode could readily undergo similar transformation through contamination unless the strictest laboratory regimen was maintained.

In the 1980s, it was claimed that there might be a fifth force hovering about among the physical laws that would slightly change the pull of gravity as measured at the earth's surface. Trial measurement of the proposed force required a large-scale operation with high towers above ground and deep holes in the ground. Composition of the earth in the vicinity of the measurement could not be well controlled such that the outcome suffered an important variability that made the experiment complicated. It required several years to determine whether the basic formulae of physics had to be modified, which they did not.

More recently there was announced the first cloning of an adult mammal, a sheep. The offspring clone was named "Dolly". The experiment required over 200 tries before success was achieved, an eminently unrepeatable experiment and certainly one that can be considered complicated. Yet it was given immediate recognition pending a corroboration, which came forth from another laboratory in eighteen months.

These are experiments in which the cause cannot be traced through to its effect. The name of that class is *complicated,* although *complex* is also acceptable usage.

Ramsey Modified

The presence of anomalous heat power has no scientific explanation at the twelve-year anniversary. Its presence challenges the law of conservation of energy, for if the power is real (the heterodox opinion) and if there is no nuclear source possible (the orthodox opinion), then that power may have appeared from out of nowhere. Such an appearance inevitably threatens the theorems of nuclear physics with some degree of change or appendage. For these reasons, a single validated occurrence of anomalous power may reasonably be called revolutionary.

A modification of the Ramsey criterion will better fit our case. I modify it by substituting the term "anomalous power" for the term "cold fusion." The Ramsey principle is applied specifically to the claim of generating anomalous power while producing few, if any, neutrons. The modified Ramsey criterion says that a single short but *valid* anomalous power period would be revolutionary. This statement I refer to as the modified Ramsey criterion, and this writer, not Norman Ramsey, is responsible for the assertion of its significance.

Am I trying to create an easy garden path that leads quickly to sure answers about the Fleischmann and Pons phenomenon? Not at all. On the contrary, I assert that there is no well marked path to the answer for a complicated experiment. The obvious exception to this slow development of a new branch of science occurs when the discipline permits readily replicable demonstrations. Experiments of that sort were common in nuclear physics from its beginning at the turn of the twentieth century, but that experience may be seen as a significant exception to the general rule.

The scientific protocol for corroboration of a complicated experiment requires replication in an independent laboratory. This does not mean that the experiment needs to be repeatable, as there are experiments in science that are quite difficult to repeat. Laboratories that have been successful in cold fusion experimentation have achieved successful results in about one out of ten attempts. The persistent demand for repeatable results comes from a misunderstanding of what constitutes correct scientific methodology. The nature of that misunderstanding is explored in the next chapter.

The reader might ask why not require proof of anomalous power? The idea of proof is beguiling. Ready replication that always obtained the specified result would constitute proof. In the Dolly corroboration (August 1998), DNA checks were considered proof. As was pointed out in Chapter 1, proof professes resolution of the issue in question, but the price for proof may be a King's ransom. To require proof may greatly raise the prospect of a false negative result. This is especially true in a field so new that theory does not provide a guide. In those experiments where proof of the result is not available, demands for proof should be dismissed. Proof may be inaccessible for many years in cold fusion research. It is my purpose to avoid either a false positive or a false negative conclusion.

That lack of accessible proof was the case with the fifth force in physics, with "Dolly" in biology, and with the nature of a gold surface in chemistry. As a consequence, it required many years to find answers in those instances. Proof, one way or the other, was not available when these cases emerged, yet they were eventually resolved within science and by scientific means.

Mistaken claims for discovery have occasionally absorbed the attention of scientists for several years before they were finally abandoned. Would a Ramsey type of criteria have validated such discoveries had it been used? Some mistaken claims did start to climb up the validation curve of progressive corroboration. The polywater claim was confirmed in other laboratories before being snuffed out. The critical component of an effective protocol is for experimenters and critics to conduct themselves like experimental technicians, as did Langmuir and Wood. *Only a careful, laboratory-centered critique of the observed phenomenon provides the means to reject the experimental claim.* That is how the N-ray claim, the polywater episode, and the Bergen Davis experiment were brought to an end. If that course involves a difficult laboratory process, then the rejection may take some time.

The source of the confusion in 1989 should be clear at this point. An unreasonable concern with the place of theory led a few physicists to disregard the corroboration of anomalous power measurements in what can be seen as a vitiation of historical method in scientific discovery. Some scientists were attracted to a strange protocol, one that held back, as though a hostage, the claim for anomalous power while awaiting evidence of nuclear products. As a consequence, the evidence for that claim was not exposed to the scientific community. Evaluation of the power claim was to be put off until *after* nuclear products in commensurate amount were reported. Until such time, the claim for anomalous power based solely upon its measurement would be held aside for eventual burial along with its discoverers. Other scientists, fortunately, followed conventional protocol. They pursued the well-measured observation of anomalous power as is ordinarily done in science. They would pursue it to validation or failure.

Editors of the broad-audience scientific journals chose to maintain the hostage strategy. The science reporters who found themselves involved in the event did not explain this unfortunate pattern of behavior to the scientific community. Most people saw only confusion.

The Skeptical Attitude

The scientific community recognizes the unique rôle of the critic as a necessary partner. His critical faculty contributes by pointing out error, oversights, or misinterpretations of data.

The skeptic, however, plays a different rôle than does the critic. Beveridge defines the skeptic with the following unqualified statement, "There is a very important distinction between a critical attitude of mind . . . and a skeptical attitude." He later asserts, "Perhaps the insistent skeptic serves a useful purpose in the community, but I admit it is not one which I admire. It is said that even today there are some people who still insist that the world is flat!"[2]

A significant number of scientists came to accept the existence of anomalous power during 1989 and proceeded to set up shop in this new field when they could find financial support. This adoption of the new field appalled some of the outspoken critics. They were convinced that no new scientific discovery had been made, a conviction that came solely from their knowledge

SUMMATION

Characteristics of the Scientific Skeptic

In general, skeptics display the following habits.

1. They do not express their criticism in those venues where it will be subject to peer review.
2. They do not go into the laboratory and practice the experiment along side the practitioner (as does the critic).
3. Assertions are offered as though they were scientifically based when they are merely guesses.
4. Questions are raised that concern matters outside of the boundaries of the claimed observation.
5. Satire, dismissal, and slander are freely employed.
6. When explanations are advanced for a possible source, ad hoc reasons are instantly presented for their rejection. These rejections often assert offhand that the explanation violates some physical conservation law.
7. Evidence raised in support of the claims is rejected outright if it does not answer every possible question. No intermediate steps to find a source are acceptable.

of nuclear physics. They were soon recognized as the *skeptics* of cold fusion studies.

Their transition from thinking as a critic to thinking as a skeptic was of their own making. They refused to acknowledge that other scientists could look reasonably at Fleischmann and Pons's data and accept the excess heat values as an accomplished fact. The skeptics slandered such scientists by calling them "believers." In doing this, they labeled themselves as skeptics.

The professional skills needed to criticize the claim of anomalous power as a stand-alone observation were electrochemistry, catalytic chemistry, surface chemistry, calorimetry, and the mathematics of data reduction. The scientists claiming to have been successful in generating anomalous power were expertly trained at the beginning in most of these skills, except for calorimetry, which they studied for months to develop the necessary expertise. The skeptics have assiduously avoided any such substantive learning for twelve years.

The skeptic refused the measured data. He did not care how well or how poorly it was measured. He refused invitations to go into the laboratory to experience the gathering of the data.

The skeptics demanded that nuclear effects be found. During these years, as nuclear effects were found, these skeptics rejected each instance as not being sufficient in some respect. As the skeptic rejects each and every instance of measured excess heat, so they also reject each and every instance of measured nuclear product. They also insisted that the understanding of nuclear processes be fulfilled inside the scientific ghetto, that the most fundamental purpose of science be performed *sub rosa.* The accomplishments of successive corroboration by inventive experiment was not reported in the broad-audience journals.

Little of value can be expected from the skeptics. They are not playing the valuable rôle of the critic even though their actions appear similar. They see only wasted motion and shadow boxing in the activities of the cold fusion scientists. They see the *acceptors* as so many acolytes blindly repeating what Fleischmann and Pons taught them in their quest for the holy grail of cheap, unlimited energy. Commentary by the skeptics cannot be expected to contribute to solving outstanding questions because the skeptics do not acknowledge those questions exist. For that reason, review of their activities during the cold fusion saga is postponed to Chapter 22.

Ethical Standards

There were violations of ethical standards on both sides of this controversy. There is only space in this narrative to touch upon some of the more prominent items.

One member of the electrical engineering department at MIT immediately started to work on theoretical problems raised by the Utah announcement. Later, a laboratory manager told him that a physics professor said he should be fired. Also, his expected award of tenure was opposed by several because of his association with cold fusion research. No doubt such efforts were well meant. The good name of the department, and of the university, was won by many decades of work, and it should not be lightly put at risk. However, there are better ways to express doubts. Threats were unethical.

The APS meeting in Baltimore included attacks that were undertaken to protect the innocent public from the likes of Fleischmann and Pons. There were ethical violations there. Ridicule is a vicious weapon. The use of it in any profession is questionable because of the nature of its impact: it proclaims its target as contemptible and thereby precludes discussion and communication. *Scientific American,* and *Chemical & Engineering News* (C&EN) have resorted to lengthy ridicule, while *Nature* has explicitly recommended its use.

It is unethical for those acting in a professional role to exercise public ridicule, but the APS used it for six years or more through its official spokesman. Apparently, the APS abandoned such behavior after the appearance of the Hoffman book in 1995. Possibly wiser and more ethical judgements are coming to the fore at the APS.

Fleischmann and Pons did not have a preprint of their Preliminary Note available. It was published two weeks later. Others have severely criticized them for conducting "science by press conference." This writer has already set forth his understanding of the enormous pressure that they experienced.

David Goodstein, Caltech vice-provost, provided an excellent vignette of how a press conference may happen.[3] He told of his Italian friend, Francesco Scaramuzzi, an "excellent nuclear physicist of the first rank." Scaramuzzi had joined the cohort of benighted scientists by undertaking cold fusion experiments in his laboratory at the Frascati Research Center, near Rome. He did not mimic Fleischmann and Pons's experiment, but tried one completely of his own design. He chose to use titanium metal loaded with deuterium gas.

Scaramuzzi detected a burst of neutrons emanating from his experiment just a few weeks later, after the Fleischmann and Pons announcement. The effect was thrilling. Neutrons have never been recognized as coming from a hydride experimental system. Nuclear events were always an entirely separate domain of activity from chemical events. The word of his success went through the laboratory like wildfire, but there was no need to inform the laboratory director at that moment.

The only thing to do with such astonishing results was to try to replicate it. When the experiment was "confirmed" by a second run, the whole laboratory was alive with anticipation. The laboratory director at that point was not aware of the exciting results. For Scaramuzzi to not tell him would have been an unimaginable breach of courtesy. He had no choice but to inform the director.

The director was told and (as Goodstein described it) the next morning Dr. Scaramuzzi found himself standing between two ministers of state addressing a national television audience. No published preprint of his work was available for other scientists to examine. Scaramuzzi became a national celebrity and the laboratory was fully funded by the Italian parliament for the first time in several years. Scientists from time to time do give press conferences without a published preprint available for distribution. There are extenuating circumstances when it is ethical to do so.

In the instance of Fleischmann and Pons at Utah, the janitor, as he pushed his broom under their laboratory bench, could appreciate the importance of the experiment—a new source of energy for society. There was no possibility of these two faculty scientists thwarting the social pressure from those who would watch and wager on the progress of their work. There was no way imaginable that Fleischmann and Pons's work could have remained secret after university officials were informed. Relentless gossip would soon begin. Other scientists would not assume a dispassionate attitude towards their claims. The university might have remained free of widespread rumor-mongering for a couple of weeks at most. There was no alternative to the early press conference.

American scientists responded somewhat naïvely by getting angry at a press announcement without an available preprint. This attitude was well articulated in the following quote.

> Much of the circus atmosphere surrounding cold fusion could have been eliminated if the standard scientific procedure of peer review and publication in a reputable journal had been followed.[4]

This quote is not to be taken at face value. The writer assumed that peer review would abort publication because the reported discovery was beyond the experience or knowledge of any possible peer. This is to be expected in the face of a revolutionary discovery.

Secrecy

If Fleischmann and Pons had avoided holding the press conference, not everyone would have been satisfied with mere gossip. Some would have sidled up to them to cooperate with them until they had milked them of their information. They would then run off to their own Great National Press Conference, possibly with a well-drafted preprint in hand.

Occasionally, secrecy is used in a departmental laboratory. Several years prior to our cold fusion saga, Paul Chu, professor of chemistry at the University of Houston, apparently tried to keep his formula for a high temperature superconductor material secret by means of a clever subterfuge for an interval of three weeks until it was presented in a prestigious publication.[5] His manuscript's purpose was to reveal the formula. It actually contained a similar, but wrong, formula repeated throughout the text. Anyone seeing the manuscript before publication would be misled. The formula's "mistake" was corrected at the last minute, just before printing.[6] Many scientists do indeed keep secrets for a time, and carefully choose when and how to release them. Beveridge comments that secrecy is at times acceptable,

> Personal secrecy in laboratories not subject to any [government] restrictions is not infrequently shown by workers who are afraid that someone else will steal their preliminary results and bring them to fruition and publish before they themselves are able to do so. This form of temporary secrecy can hardly be regarded as a breach of scientific ethics . . .[7]

People who argue against this practice ought to look carefully at how some of the early participants in the cold fusion fracas behaved: Caltech assigned twenty-one scientists to their research effort. Fleischmann and Pons would receive credit for their work only if they kept it secret for a limited time, which they did for five years.

Some degree of secrecy will continue to be an accepted practice as long as budgets are short and research scientists are too numerous. The National Academy of Sciences (NAS) recognizes this in its pamphlet "On Being a Scientist," "During the initial stages of research, a scientist deserves a period of privacy in which data are not subject to disclosure."[8]

The idealistic attitudes evinced by some physicists on matters like secrecy may be due to many of them working in a cocoon environment. Isolation from the pressures of the marketplace (except for job searching) was nurtured by government support of the nuclear research community for fifty years. Inventions and patents would be owned by the government, and employment lasted a lifetime. Exploitation of discoveries and inventions were somebody else's business. As a result, there was no reason for any degree of individual secrecy, and every reason to adopt an idealistic outlook.

Chemists to a considerable extent lived in a quite different world. Their livelihood often came from short-term contracts with industrial firms, or from two-year research grants. Their scrambling for contracts led to attitudes that were altogether more pragmatic.

Those scientists who came to accept anomalous power as something that might really exist wanted to get together and talk shop, as technologists naturally do. They wanted to make the experiment more reproducible, and find the source of the excess power. Those desires upset the skeptics, who assumed that the only interesting or even legitimate topic was to ask whether or not cold fusion existed. They insisted that this topic must be settled before there could be any continuation of research.

Those who work in the field of cold fusion gather together about every sixteen months at an International Conference on Cold Fusion (ICCF). For example, ICCF-6 was held at Sapporo, Hokkaido, Japan, in October 1996, ICCF-7 at Vancouver in April 1998, and ICCF-8 in Lerici, Italy, in May 2000. These conferences are organized largely on the assumption that the Fleischmann and Pons effect is widely observed. The purpose of the conferences is to exchange information about their work. One theoretician in the field began his presentation by saying carefully, "I accept the phenomenon of excess energy as real,"[9] only then did he proceed with his paper. A school of scientists had thus come into being and they organized meetings to advance their common work. That was, and continues to be, the purpose and function of these annual ICCF meetings.

The skeptics were actually asking for a different kind of conference when they argued that those scientists whose experiments had failed were not represented by invitation at conferences. If the skeptics wanted a meeting to explore the question of whether cold fusion exists, they could organize such a meeting. These ICCF conferences left the skeptics behind simply because they were organized for a purpose the skeptics could not appreciate—because they were skeptics. The question of the existence of cold fusion was only raised as a matter of personal courtesy to skeptics who might be in attendance. That topic in its broadest interpretation was implicit in the question of the energy source.

On Being a Scientist

The National Academy of Science (NAS) teaches the subject of these four chapters of Part III, Validation. They do so with a broad brush and with the cold fusion episode as an outstanding reference when preparing the second edition of their pamphlet, *On Being a Scientist: Responsible Conduct in Research,* in 1995. They assert that the scientist should use "generally accepted methods," otherwise "other scientists will be less likely to accept the results," an incontrovertible thesis. They mention that violation of this rule was a reason for the negative reaction of many scientists to cold fusion research. They go on to speculate, "The claims were so physically implausible that they required extraordinary proof."

What if proof was inaccessible? Did that circumstance call for Fleischmann and Pons's demonstration of anomalous power to be interred with their bones? How was their demonstration "physically implausible"? Conflict with theory may have called for caution, but all that was necessary to achieve plausibility was to measure the emitted power carefully, as they and others did.

The NAS pamphlet complains that, ". . . the experiments were not initially presented in such a way that other investigators could corroborate or disprove them." Wrong. One could corroborate the claim of anomalous power by replication in an independent laboratory as was done a number of times within two years. The NAS apparently believes that there is no such thing as an experiment that takes two years to reproduce. It sounds a little like the NAS was letting nuclear physics get in the way of science.

Much has been made of the importance of peer review. That is the process whereby a professional journal evaluates submitted articles, and funding sources evaluate proposals. Each submission is sent out to two or three of the author's peers in a specific field. These specialists will pass informed judgement on the submission's merits.

Peer review does not always work well. Henry H. Bauer, former dean and professor of chemistry, Virginia Tech, Blacksburg, Virginia, considers this question in his book, *Scientific Literacy and the Myth of the Scientific Method,*

> So moderately successful scientists learn to adjust to the predictability and mediocrity of peer review by camouflaging their best ideas: they seek support for "normal" research into the known unknown,

> but then use some of the granted funds to follow their pet hunches. Those who decided to look at the possibility of cold fusion, though they were accomplished people of high reputation, knew better than to ask support for their long shot.[10]

Fleischmann and Pons knew better until they were ready to risk conflicts of interest and premature publicity.

The NAS discusses peer-review in a limited manner. It referred disparagingly to the scientist who, ". . . releases important and controversial results directly to the public before submitting them to the scrutiny of their peers." ". . . it should be done when peer-review is complete—normally at the time of publication in a scientific journal." Most scientists agree that this is a good standard. It was more adequately met by Fleischmann and Pons than by some of their most vehement critics who spoke most assertively to the press prior to their own peer presentation and publication.

Something was missing from the NAS rule. It should have inserted into its pamphlet a paragraph for that sort of exception to the general rule. Their well-measured observations, which "defied all contemporary scientific experience"[11] would likely have been cast aside by the editors of *Scientific American* and *Nature* and for the wrong reasons. I trust that this casual, almost flippant, NAS commentary will be reviewed by scientists with the broad experience of several disciplines prior to publication of a third edition.

In her book *Science on Trial* (1993) Judy Sarasohn establishes the responsibility of scientists for the continuing influence of their published papers. This consideration ought to be incorporated into future editions of the NAS pamphlet. She quotes Harvard Professor Paul Doty,

> To forgo this obligation—to leave to others the responsibility of establishing the validity of what you have published—is not only a fundamental retreat from responsibility but, if it became accepted practice, would erode the way science works. For the cutting edge of science moves forward by building rapidly on what is published on the tentative assumption that [it] is correct, not by waiting for others to test each paper's validity.[12]

Doty's admonition applies to Fleischmann and Pons, whose continuing publications have largely met this obligation concerning their initial statements and publications. There is the further question, should Fleischmann and Pons still be considered "incompetent"? Is our knowledge that there is no such thing as excess heat, still "absolute"? Many scientists believed these things to be true. Doty asserts that the authors of such assertions were still responsible to the scientific community for their continuing acceptance.

By the summer of 1989, it was clear that the anomalous power experiments were not readily reproducible while the resulting phenomenon occurred a small fraction of the time with some experimenters. At the NSF/EPRI conference in October, Fleischmann presented the current results that he and Pons had obtained. His data showed that 23 out of 31 cells generated power. Should such results be published?

In nuclear physics, if the distribution of results was shown to be the direct consequence of a distribution of excitation (input), the answer is a firm, yes. But if it is an experiment that simply seems to work only part of the time, then the answer is, no, it is not publishable.

Biology seems to publish by a different standard. Dolly was the single result of 227 attempts, and it was considered publishable. Over several years, that experimental process became more efficient to eventually bring the odds up to about fifty percent. The difference between nuclear and biological standards is an historical one. The rule is that in each discipline standards develop over time that best serve it, and these differences reflect experimental peculiarities of the field.

Sometimes peer review takes the form of visits to a working laboratory. Mike McKubre's laboratory did successful anomalous power experiments from 1989 to 1997 and continuing. He was visited twice by scientists who were eminently qualified in the appropriate technology but who were completely out of the public eye.

The first visitor was an electrochemist fully qualified in calorimetry. A day was spent studying the experimental and measurement processes, and looking at the equipment operation in the laboratory. This previously outspoken critic found nothing wrong with the experimental work. If the results showed excess energy, the visitor could see no basis on which that result might be wrong. He so informed McKubre of his conclusion.

The second visit was by a team of three scientists. One was a well-experienced nuclear experimental physicist. The other two were senior electrochemists, one of whom had written several textbooks in the field. They enjoyed the same visiting routine as the first visitor. They arrived at the same endpoint as the first visitor, that there was nothing wrong with the calorimetry. They so informed McKubre.

Then they were silent, completely silent. Were their individual reputations so important to them that they could not be put at risk by reporting publicly what they had found? What they had found was that McKubre's experiments did reveal the existence of anomalous power as far as these experts were able to tell. Their silence was unethical in view of the importance of the matter at hand and the special expertise the four could bring to bear on the subject.

The skeptic insists that technical reports on cold fusion research would not withstand the rigor of proper peer-review and that they were not of professional quality. Much of that talk was disingenuous. It was based on the understanding that a competent reviewer will expect an additional data in any paper that claimed excess energy, data that showed the nuclear products produced.* If that data were missing, the skeptics assumed the paper would be rejected.

The journal *Fusion Technology* decided, at the beginning of this cold fusion episode, to take a considerable professional risk. Its editor drew up criteria for the acceptance of cold fusion articles that was less rigorous than their usual standards for review and publication: the reported laboratory work was to be judged by unchanged standards; there could be no perceptible error of procedure. Interpretation of the work, however, could be more speculative than would ordinarily be permitted. It established these policies against the recommendation of the editorial board of its parent organization, the American Nuclear Society (ANS). Many scientifically important accomplishments of the first ten years have seen the light of day as a consequence.

The editor, George H. Miley, professor of nuclear engineering at the University of Illinois, Urbana, Illinois, risked his position as editor to do this, and is considered something of a hero within the ranks of cold fusion scientists. He suffered professional criticism. Huizenga said of Miley's editorial policies, "The peer review is lax."[13] As Miley looked at it, "My editorial board would long since have fired me if only they could find the time to get together."[14]

The following letter to Miley as editor of *Fusion Technology* was typical of many criticisms.

> Do you really want to rapidly publish a bunch of "half-baked" work on cold fusion? I expect that Fleischmann and Pons will find the error in their power balance within the next month or so, and all those authors will be desperately trying to withdraw their papers.[15]

Miley explained to his board that science would benefit by publishing articles in this new field. This benefit would happen without particular concern for the cold fusion outcome. Publication would provide a rapid, disciplined communication among researchers. It would provide a record of what was done that might be of great value later even if the field died, much as the record of Paneth's experiments in the 1920s is of continuing interest.

Under Miley's regime, a reviewer examined a paper for freedom from factual errors, and for adequate internal coherence. Considerable leeway was given for speculative theories and choice of experimental targets. The princi-

* See, for example, the discussion of the Oriani correspondence with *Nature* magazine in Chapter 14.

pal complaint raised concerned the choice of peer reviewers. Were you not using people in cold fusion research to review the papers? Was that not a self-perpetuating activity, quite independent of reality? Miley's response,

> This is the way science is . . . You always have people within the community review the papers because they are the ones who are the experts. In hot fusion, I use hot fusion reviewers. You trust professional judgement; you trust the integrity of the reviewer.[16]

And, I might add, you trust the integrity of the editor.

So there was professional review of cold fusion papers in the journals. Much of the talk of inadequate critique in the field turned out to be rumor-mongering. Such talk was unsupported by data or any published survey.

There were, however, ethical lapses in the course of the cold fusion episode. How does a knowledge of those lapses help us to learn whether there was any merit to Fleischmann and Pons's claim to the observation of anomalous power? Answering that question at this point involves two steps. There is the broad question of scientific method: what is it, how flexible is it, how rigid is it? Then one must ask, is the modified Ramsey's way a scientific method that can be expected to provide a reliable answer?

CHAPTER ELEVEN

Variety of Method

Myths of scientific method abound in the cold fusion episode. Skeptics demanded reproducibility. Science does not require that particular characteristic; replication will do. Skeptics demanded nuclear products. Science does not require them today; discovery of the nuclear products tomorrow will serve quite nicely. The electrolytic cell has been described as "high school" science although there is much that is subtle about the Fleischmann and Pons cell. A great deal of grandstanding occurred in the early weeks about a need for "stirring" the electrolyte. As it turned out, the cell was designed to make mechanical stirring unnecessary.[1] Many myths had to be dispelled in order to consider a validation process for the claim of anomalous power.

In this chapter, we touch upon the question of what constitutes science. There are a wide variety of attitudes about exactly what claims lie within the discourse of science. In general, this variability resides underneath the forum of open debate, but it is never referred to by any party in the debate. However, its existence introduces considerable confusion.

These attitudes follow the various disciplines, in general. The anomalous power experiment, which topic occupies the first fourteen chapters of this book, is a chemistry experiment. Its associated calorimetric measuring technique may also be considered a part of the discipline of chemistry. That the experiment—the Fleischmann and Pons experiment—has associated with it the hypothesis of a nuclear source for the energy does not change the discipline of interest because the hypothesis is not a claim. It is for others to pursue the hypothesis and eventually stake *their* claim. That claim will presumably reside in the discipline of physics. This narrative plainly commits itself to the discipline of chemistry.

From the beginning, the scientific community underrated the Fleischmann and Pons experiment. The press referred to it as a "kitchen" experiment. Shortly after that comment was made, one esteemed electrochemist reported that he had spent two weeks with the experiment before he realized its subtlety, and then he had to start over again.[2]

There was no appreciation in the scientific community that the experiment might a difficult one. It was thought that anyone with scientific training ought to be able to copy the experiment. If the claims were true, all that was necessary was to assemble the cell and turn it on. In fact, only a few scientists were clearly successful during the first year. They were mostly electrochemists. None of them were physicists. There was no sensibility at the time that the anomalous power generation experiment might take a practiced skill to do properly. The principal signature of the Fleischmann and Pons experiment was heat. Many of those who tried their hand at it were not aware of this.

An example of the experiment's complexity emerged in 1996. A team in France reported the successful duplication of part of the high temperature and high power experiment that Fleischmann and Pons reported in 1993. This partial duplication took thirty months.[3]

Three months after the Utah announcement, the experimental cell results for Caltech, MIT, Brookhaven/Yale, and Harwell were reported. Those four early experiments claimed to find nothing of interest. By way of contrast, Tadahiko Mizuno required eight months to prepare his second experiment for the start of electrolysis.[4] He claimed interesting results in neutron and tritium evidence, as well as excess heat bursts and the excess heat called "heat after death," the heat generated after the current was turned off. In the reference given, he tells the toil of those eight months in the preparation of his cell. Laboratory methodology was always a critical part of cold fusion research.

Scientists working in this field called cold fusion were impeded in their work after the Baltimore event. They found access to scientific publications, science funds, and to the collegiality of other scientists greatly curtailed.[5] That fact helps to explain why America's scientists generally failed in their early assessment of cold fusion research claims while those in Japan, France, Italy, and India proceeded apace.

The variety of method in science is witnessed in the stories of the life of S. Arrhenius and in the development of the Wassermann Test in medicine. We look at each of them in turn.

Arrhenius

Svante Arrhenius (1859–1927) was a Swedish chemist of renown and a Nobel laureate whose discoveries had aroused feelings of hostility and anger in the orthodox scientists of his day. His story bridged the turn of the twentieth century. His breach with the orthodox scientific community of twenty years duration, his intellectual survival in a small group with his scientific fellows, and the argument whether his discovery was one of chemistry or physics, all parallel the instance of the Fleischmann and Pons controversy.

Arrhenius finally achieved collegiality between himself and the scientific world by persistently teaching his discoveries.* His story is almost prophetic of what happened in cold fusion research during its first decade. He was banished from both his university and the Swedish scientific establishment. He overcame that affliction and went on to great acclaim as a revered elder statesman within the European scientific community. He accomplished much of this by attaching himself to a foreign scientist of recognized integrity.[6]

Arrhenius's principal scientific discovery asserted that when a salt is dissolved in water, each molecule of the salt separates into electrically charged electrolyte particles that had been named *ions* fifty years earlier. It was ions that interacted chemically with other ions in solution rather than atoms or molecules that reacted. He asserted that these ions carried an electrical charge as they moved about and were the mechanism by which the solution conducted electrical current.

There was open and often bitter controversy during the two decades after he announced the discovery. The distinguished German chemist Wilhelm Ostwald stood at his side as a foreign savior and mentor during Arrhenius's most difficult times. Ostwald attested to the correctness of Arrhenius's thesis and he started a new technical publication as a vehicle for spreading and defending the ionic theory. They traveled the world of science arguing Arrhenius' theory. The two of them made a major presentation and defense of his theories in London in 1890.

Arrhenius's redemption began at home when the University of Stockholm's predecessor institution offered him a position as professor. The offer was made under somewhat humiliating conditions which he bore with grace. He was quickly accepted by his colleagues and in two years was elected to the presidency of the school. In 1903, he became the first Swede to be awarded the Nobel Prize and it was in chemistry. He enjoyed the considerable recognition of other scientists continuously until his death in 1927.

It is necessary to look back further to understand the nature of Arrhenius' fall from scientific grace. Scientists had struggled to understand electrical conduction in liquids for the previous one hundred years. Pure water and pure salt acting separately are each electrical insulators. When salt is dissolved in water the solution becomes an excellent conductor of electricity, i.e., an electrolyte. No one understood what actually happened when the salt dissolved in water to make the solution electrically conductive. Arrhenius took a dedicated

* The writer is indebted for the biographical details of Arrhenius's life to the four sources listed in the several endnotes.

interest in this puzzle in his youth and made it a part of his university education. He was then able to put the pieces into place.

Svante August Arrhenius grew up near the world-famous State University of Upsala, Upsala, Sweden. As a bright youth, he turned his attention early to experimenting with electrical conduction through salt solutions. When he entered the University, Professor Robert Thalén, the professor for physics, did not take him seriously and refused him the use of the physics laboratory. He did his experiments in the laboratory of Erik Edlund, Academy physicist.[7]

Arrhenius made the fateful decision to continue with his interest in conductive solutions when he enrolled to study for his doctorate degree. He records the exact moment when he came to his principal discovery about conductivity in solutions. It was on May 17, 1883, that he entered a period of feverish work to write it down. He claimed that the salt molecules in solution divided into electrically polarized particles called ions. He confirmed for himself that he understood what was happening in salt solutions. His full statement of discovery claimed that these ions became the reactive elements for chemical behavior in solution and were also the agents for electrical conduction.

The relationship between theory and experiment was not well understood at this time in the development of modern science. Arrhenius had conceived a theory of ion formation and action. Professor Per Teodor Clève, his doctoral advisor, was a distinguished scientist and the discoverer of the two metals: holmium (holmio was the Latin name for Stockholm) and thulium. Clève considered theory to be something like Henry Ford's history, that is "bunk."

> He came to Clève, his professor of Chemistry, with the new theory formulated in his thesis. "I have a new theory of electrical conductivity," said Arrhenius. Clève was no doubt a skillful experimenter and investigator of the rare earth elements. But theories to him were abominations to be fought or ignored entirely. In the classroom Arrhenius had listened to him for months. Never once had he heard a single mention of the great Periodic Law of Mendeléeff, even though the Russian's Periodic Table of the Elements was now more than ten years old. Clève turned to this chemical tyro, "You have a new theory? That is very interesting. Good-bye."[8]

Arrhenius's thesis dissertation was closely fought, and the outcome was only a partial victory for him. He received his doctorate degree of the fourth class, the lowest of four possible grades, and designated *non sine laude approbatur:* approved not without praise.[9] He could not pursue an academic career at the university.

He responded to the setback by sending copies of his thesis to several prominent scientists outside of Sweden.[10] One went to Wilhelm Ostwald, professor of chemistry at the Polytechnical School at Riga, in the Russian provence of Livonia, later Latvia. Ostwald found his thesis compelling and decided to help him. He traveled by train to Stockholm to meet and befriend Arrhenius. The result of Ostwald's mediation was the offer of a teaching position for Arrhenius in the Stockholm Technical High School (the Högskola, later part of Stockholm University). The world of science was focused at that time entirely in Europe and North America. Ostwald's continued patronage of Arrhenius and his thesis gradually brought the world of science to terms with the existence of ions in solution.

Arrhenius gained the respect and fellowship of his colleagues in the Högskola, a result of his increasing international renown. During this time, Arrhenius married Sofia Rudbeck, one of his pupils. The marriage lasted only two years and produced a son, Olav Vilhelm.

Later, after the death of Alfred Bernhard Nobel in 1896, he became deeply involved in the establishment of the Nobel prize awards and in the adjudication of his will.[11] His principal contribution to the establishment of the Nobel Academy was his insistence that the nomination and award selection procedures be international in their vision. The Academy elected him a member of both the chemistry and physics award selection committees.

J. H. van't Hoff, a colleague in Holland, received the prize in chemistry in 1901, the first year of the chemistry awards. Arrhenius received it in 1903 after a thoroughly contentious candidacy. His discovery of ionic disassociation in solution raised the significant question whether it was a discovery in physics or chemistry .

The physics award committee included Professor Thalén, from Upsala. It had no members from the Högskola other than Arrhenius. The Upsala University members looked down on the Högskola as an inferior institution. Thalén, who strongly influenced the physics committee's position concerning Arrhenius's candidacy, successfully persuaded the committee that Arrhenius's discovery was more properly assigned to chemistry thus absolving the physics committee of his candidacy. Clève, who had once refused any interest in the young Arrhenius's theories, now advanced Arrhenius's cause effectively in the chemistry award committee and was successful in achieving for him the chemistry award.

Two years later, Arrhenius married Maria Johansson. This marriage produced three children: a son Sven, and two daughters, Ester and Anna-Lisa. In 1939, Ester married Tore Dahlgren a major in the army signal corps, who worked in organizing Sweden's psychological defenses. A daughter, Karen, was born to that marriage in 1940.[12]

Karen attended Upsala University where she earned a Ph.D. degree in biochemistry. Later, she settled in the United States where she accepted a position at the University of Utah. There she married a theoretical chemist and chemical engineer, Dennis James Caldwell. Now known as Dr. Karen D. Caldwell, she was director of the Center for Biopolymers at Interfaces at the University of Utah. She continued there until she retired and returned to Sweden in June 1998.

She held positions as Associate Research Professor of Bioengineering with an Adjunct Associate Professorship in the Department of Chemistry at the time of the cold fusion announcement. A colleague and friend of hers, and head of the chemistry department at the university, was Stanley Pons.

She became aware of the March 23 (1989) press conference on the preceding day. She attended it as a member of the chemistry department. Afterwards she spoke with Fleischmann and Pons. In Fleischmann's words,

> After the press conference, Dr. Caldwell came up to us and said, "Well, when my grandfather proposed electrolytic disassociation, he was dismissed from the University. At least that won't happen to you." I said to her, "But you are entirely mistaken. We shall be dismissed as well."[13]

The Critique of Science

The academic world of literary criticism had experienced an ordeal of extended internecine criticism during the last quarter of the twentieth century that amounted to a revolution in its culture. Those engaged in advancing that cultural revolution generally refer to themselves as poststructuralists. By 1989, their revolutionary rheticle was targeted on science, bringing those disciplines under polemical fire. Because of its all encompassing purpose, I refer to this movement as the critique of science.

That critique contends that what are called the physical laws of science are actually only social constructs, like political laws. The critique argues that the Darwinist theory of the survival of the fittest suited the purposes of laissez-faire capitalism and, therefore, was constructed so, and that it emerged as a discovery in that time and place because it suited the particular purpose of justifying the rise of industrial capitalism.[14] That critique holds that "scientific laws" are built by scientists who, at root, are political beings acting politically.

The importance of what is called cold fusion hinges on the question of whether or not it is science. One of the outstanding questions is whether the laboratory work underway during the first twelve years is a pathological activity, a political activity (construction), or whether it constitutes empirical scientific activity. (That it might be a pathological activity, in Langmuir's sense, has already been examined and rejected in Chapter 5, p. 62.)

The cold fusion saga lent itself marvelously to the argument by the poststructuralists that the modern edifice of science was merely a social construction. Its tale ought to have appealed to those who saw the laws of physics as the laws of the physicists. Accordingly, the laws so constructed were not an immutable property of nature. They were merely interpretations of particular sets of data as seen by the scientists. As such, they necessarily expressed the race, class, and gender positions of the interpreters. Such laws would be merely subjective constructions.

It is surprising that more was not made of the circumstances of cold fusion by the poststructuralists. The U.S. scientific establishment had dismissed the whole field of cold fusion studies as a sort of mirage. The government refused to issue patents, sponsor research, or even publicly discuss the merits of the field. The Japanese government, by way of comparison, sponsored significant research, encouraged academic and industrial institutions to do likewise, and issued more than one hundred patents. Two different cultures, both with access to the same base of experimental data, drew opposite conclusions as to the reality of cold fusion. What better evidence could one have asked for to persuade the social theorists that the reality of nature in this case was culturally determined?

These poststructuralists were not timid people, but they did not speak out on the matter of cold fusion research. They were possibly waiting for the dust to settle before committing themselves. If so, when that happens they will find that it is too late for them. Years of research by hundreds of technicians will have firmly established the place where nature stands: whether anomalous power exists will no longer be a question. The culturally determined outcome will come to be seen as a culturally determined *process.* Nature will determine the outcome.

There remains, however, a potential danger in this particular kind of theorizing about the nature of scientific knowledge. One of the poststructuralists' leading opponents tells what that danger is.

> Real harm is being done not to the indifferent body politic, but to the cause of empirical rationality, which has been tacitly devalued by many poststructuralists and explicitly condemned as oppressive by some others. . . . the poststructuralists' reliance on speculative Gallic versions of the "sciences of man" has led them into a grave misconstruction of modern science and an inadvertent reassertion of the Newtonian-Laplacean determinism . . .[15]

Empirical rationality is the historical mode of scientific thought in the hard sciences, and tracking that rationality is the method in this account of the cold fusion saga.

Empiricism begins with observation. The word observation as used in this account does not necessarily refer to seeing in the literal sense. The historical cases of pathological science described earlier demonstrates the mind's tendency to misinterpret what is seen.

Observations will be obtained through instruments. Visual observation still remains important, usually as a guide. The expert gaze reports that all is well with the experiment, or that the cathode rod has bent, the bubbles are distributed unevenly, and it may be time to bring it to an end. Measurements that require precision and repetition are best done with instruments, while qualitative and longer-term evaluations are done well visually.

H. H. Bauer helped to unravel some of the confusion within the cold fusion episode.* He illuminated some important differences in experimental research methodology between, for example, physics and geology. One can control the variables (conditions or parameters) in physics to perform definitive experiments. Such detailed control is not generally possible in geological research where the determining factors are generally not known. Much of chemistry lies somewhere in between those two disciplines. Chemistry was more like geology than physics in its complexity. Unrecognized reactions are more likely to be present. These differences of complexity have to be allowed for when interpreting experimental results. Conclusions have to allow for the extent of unknown factors that contribute to an experimental outcome. The interpretation of a complicated experiment is a markedly different proposition from the interpretation of one where the variables are known. Ramsey's preamble was one way of stating that difference.

Science, from time to time, grows in a somewhat haphazard manner. Because this may well be the manner in which the evidence for anomalous power has grown, I offer the following section as evidence that such growth is not new to science.

Genesis and Development of a Scientific Fact

Ludwik Fleck (1896–1961) was born and lived most of his life in Lvov, which was then in Poland. He worked professionally as a microbiologist of an unusually original bent. He studied the history and development of the Wassermann Test for syphilic infection for many years. He derived from it a sense of scientific methodology. He wrote of those studies in a book with the astonishing title of *Genesis and Development of a Scientific Fact,* published in 1935.[16] His story tells how in a complicated experimental science a scientific fact may

* In his earlier book, *Electrodics,* he explained some of the subtleties and complexities of surface chemistry.

emerge quite gradually over a period of years. His story may be enlightening for those who have had difficulty recognizing the slow establishment of a factual base for anomalous power.

Fleck was educated in the public schools of Lvov, where the use of the German language was a remnant of the Austrian Empire. He received a degree in medicine in 1926 from Lvov University. His early laboratory work began in the study of typhus and other infectious diseases. From 1928 until 1935, he was director of the bacteriological laboratory of the Social Sick Fund and from which he was dismissed for being Jewish.

Fleck spent the World War II years in the Buchenwald and Auschwitz camps where he developed and manufactured vaccines for the German army. He survived the war, although his siblings did not, and continued his technical work in Poland. He authored several textbooks, and more than one hundred scientific papers. He emigrated to Israel in 1957 where he continued his technical work until his death in 1961. His scientific publications included ". . . seven papers on methodology of science, some articles on methodology of scientific observation, on principles of medical knowledge, on the history of discoveries, etc."[17]

His *Genesis* book describes the slow development of the Wassermann serum (blood) Test for syphilic infection during the first decades of the twentieth century. It was an extended process of scientific discovery, a scientific methodology about which little has been written. As Fleck remarked,

> In the course of time, the character of the concept [of syphilic infection] has changed from the mystical, through the empirical and generally pathogenetical, to the mainly etiological. This transformation has generated a rich fund of fresh detail, and many details of the original theory were lost in the process. So we are currently learning and teaching very little, if anything at all, about the dependence of syphilis upon climate, season, or the general constitution of the patient. Yet earlier writings contain many such observations.[18]

He found that the changes go far beyond even those disparate factors. "The explanation given to any [scientific or experimental] relation can survive and develop within a given society only if this explanation is stylized in conformity with the prevailing thought style."[19]

Beveridge pointed out the very same thing as follows.

> In nearly all matters the human mind has a strong tendency to judge in the light of its own experience, knowledge and prejudices rather than on the evidence presented. Thus new ideas are judged in the light of prevailing beliefs. If the ideas are too revolutionary, that is to

> say, if they depart too far from reigning theories and cannot be fitted into the current body of knowledge, *they will not be acceptable.*[20] (Emphasis in the original.)

The development and clinical use of the Wassermann Test evolved, not sequentially as might be assumed, but simultaneously. Those immersed in the saga of cold fusion studies should note if the following passage does not seem familiar.

> Despite every safeguard and mechanization, however, new and unexpected findings continually emerge. From time to time very promising relations and vistas open up, only to vanish again like so many mirages. The reaction occurs according to a fixed scheme, but every laboratory uses its own modified procedure, which is based upon precise quantitative calculations; nevertheless, the experienced eye or the "serological touch" is much more important than calculation.[21]

Note, "From time to time very promising relations and vistas open up, only to vanish again like so many mirages." Is this description not reminiscent of one of the characteristics of cold fusion research? Is it possibly a natural part, if only a small part, of scientific methodology?

Fleck comments on the intrinsic variability even within a wholly successful standard procedure.

> It is possible to obtain a positive Wassermann reaction from a normal blood sample and a negative one from a syphilitic sample without any major technical errors. This was shown very clearly at the [international conference] where the best serologists . . . examined the same blood samples simultaneously but independently. It was shown then that the results did not completely agree either with each other or with the clinical aspect of the disease.[22]

The early evaluation of the cold fusion claims saw the "thought style" of fusion physicists' as dominant. Imagine how they would have discredited the Wassermann Test at the outset if they had demanded a recipe for its absolute replication. They would have insisted that there was no science in the test. The Wassermann Test would have died an infant death, as nearly happened with the cold fusion episode.

There was no reason that such variability should deny a field the status of being a science. Consider that the reaction was one of the most important medical aids used quite successfully in thousands of medical establishments every day and about which many theoretical papers were written.[23]

Fleck explains how the Wassermann Test research moved ahead despite the impossibility of any text of limited length ever doing justice to such an amorphous subject.

> These ideas certainly underwent substantive change in passing through any one person's mind, as well as from person to person, because of the difficulty of fully understanding transmitted knowledge. In the end an edifice of knowledge was erected that nobody had really foreseen or intended. Indeed, it stood in opposition to the anticipations and intentions of the individuals who had helped build it.[24]

Is not the subject called cold fusion studies an amorphous subject?

Fleck's essential commentary upon the scientific methodology found in his study of the Wassermann Test was set forth in the following lengthy quotation.

> The epistemologically most important turning point occurred with the detection of syphilitic antibodies . . . During the initial experiments it produced barely 15–20 percent positive results in cases of confirmed syphilis. How could it then increase to the 70–90 percent found in later statistics? *This turning point represented the actual invention of the Wassermann reaction as a useful test.* The theory of the reaction as well as the historical and psychological circumstances surrounding its conception are of less practical importance. *If the relation of the Wassermann reaction to syphilis is a fact, it became a fact only because of its extreme utility owing to the high probability of success in concrete cases.* The moment this decisive turn occurred cannot be accurately determined. No authors can be specified who consciously brought it about. We cannot state exactly when it occurred nor explain logically how it happened.[25] (Emphasis in the original.)

I will assume that the Wassermann Test was a product of scientific activity, and that its use held the status of being recognized as a scientific test by the scientific community of its day and continuing.*

In the early 1980s, careful measurements were made of what is called beta emission, the emission of an electron from a nucleus that characterized the change of a neutron into a proton. The electrons emerged with a wide variety of energy levels. Carefully done calorimetric (heat) measurements showed that the energy released over a large number of emissions was equal to

* Fleck's book (in English) has been reissued by the University of Chicago Press.

the average energy of the individual emission. At the time, theory said that the energy ought to be equal to the peak energy of the emission. Between theory and measurements, there was a missing quantity of energy.

While those involved in the measurements thought this conflict meant that the missing energy got transported away by a particle presently unknown, most of physics refused the hypothesis and simply waited. They did this on the supposition that there might yet be an error in the heat measurements. In about twenty years, instruments were invented that were able to detect the neutrino. It proved to be the new particle that carried away the "missing" energy. The calorimetric measurements and their corresponding hypothesis of a new particle were vindicated.

This pattern of disbelief may be what is happening in cold fusion research where the measurements will be held in abeyance until the nuclear answers are in. But it should be emphasized that the beta emission case was quite a different than was the careless talk of pathological science in the instance of cold fusion research.

The Strict Criterion

Taken as a group, the American nuclear physicists during the second half of the twentieth century looked upon the discipline of nuclear science as the epitome of American science. That attitude was derived not only from decades of magnificent discovery, but from its successful application to weapons and energy generation, all of which resulted in the ascendancy of the nuclear physicist in the ranks of government as described in the opening of Chapter 5. From that high place, the nuclear physicist came to realize that he was congenitally ordained to lord over American science. While, at the knee of his mentor, he had learned that the definition of science for nuclear science was the definition for all the disciplines. Woe unto the innocent who imagined that the many disciplines of science stood on equal footing with one another.

That definition of what constitutes science was specified in the following protocol. I refer to it as the "strict criterion."

> Science is concerned with the results of experiments. For a result to be of interest to science, it must be reproducible. Furthermore, its reproducibility is only of interest if it is the result of identical experiments. Both the result and the experiment causing it must be identical as well as reproducible. The experiment must be precisely defined by an instruction set. The set must come from the source that claims the discovery.[26]

The demand for proof that was discussed in the first chapter (p. 14) appears now in this strict format. Interestingly enough, it demands a great deal more than proof. It requires that the proof be acquired in accordance with a strict procedure. Thus, within this protocol there existed science; outside of it there was only the void.* Some astronomical observations, because they are not the result of an experiment, may be given a sliver of scientific existence just short of oblivion, but nothing more. The protocol, as expressed, was not meant to be limited to the field of nuclear physics. It was applied, when provoked, to any discipline of science. The strict criterion was clearly the product of a mature discipline in which the criteria have been notched up tighter and tighter over many decades.

Imagine that an astronomer spots a large asteroid and computes that it will soon collide with the Earth. Other astronomers are alerted who also see it and they too compute that it is scheduled to hit the Earth. A nuclear scientist is consulted about the possible use of a nuclear explosive to ward the asteroid off its trajectory. Not to worry, comes the reply, because science does not know of any asteroid threatening the Earth. A search has determined that no experiment was performed which resulted in an asteroid coming towards the Earth. Even if there were such an experiment, it has not been shown to be reproducible. Nor has the asteroid been shown to follow an instruction set prepared by the discoverer. Therefore, science does not know of any asteroid menace to the Earth. Such is the logic of the strict criterion: much if not most of reality resides outside its circumference.

Experimenters by the dozens may have measured anomalous power, but those who followed the strict criterion had no need to show any interest in laboratory accomplishments. The questioning scientist has merely to ask if the experimenter's instruction set came from Fleischmann and Pons. There being no such set, then the question of anomalous power was resolved: it does not exist within the discourse of science.

Scaramuzzi speaks to one piece of this strict protocol.

> A well-known physicists was asked what he thought of CF. His answer was that it was not good science, because of the lack of reproducible experiments. I wrote to him presenting the following arguments: a) I agree that reproducibility is a "must" in experimental research; b) however, a new field, at it beginning, is often characterized by a lack of reproducibility, and it is the task of the scientists operating in the field to understand what is going on, in order to pur-

* There is also a concern that this protocol might have a propensity to propagate error. If there is embedded in the instruction set an original error, then each laboratory that supposedly was corroborating the result would do so by reproducing that original error.

> sue reproducibility; c) this has been done in the case of CF, making meaningful, even though slow, progress (I sent him a paper of mine in which I discussed this problem). My letter did not produce any effect, in the sense that he did not change his mind, and went on demanding reproducibility, as if it were an intrinsic characteristic of research and not something that has to be pursued.[27]

I attributed the attitude of the physicist referred to above (whose identity was unknown to me) to be the consequence of experimentation with perfect objects in an ideal environment. Without being told, one could be sure he was not a physical chemist, for example.

The establishment of those strict qualifications, as they were presented to me, allows only a sliver of scientific respectability for astronomical observations. Nothing is reserved for, say, a supernova observation, geological observations, or clinical, or botanical, and so forth. In fact, the strict criterion rule places the well-measured observation beyond the circumference of science; the well-measured observation is not a part of the scientific enterprise. Neither our hypothetical asteroid nor anomalous power exists within the discourse of science according to the strict methodology as taught and rigorously applied in at least some parts of nuclear physics. This evaluation explains much of the obtuse criticism—that cold fusion is dead—by scientists of that discipline.

Observation was a valuable part of scientific methodology since Galileo established the practice with his observation of the moons of Jupiter and the mountains on the Moon. It will continue to be so. But each scientific specialty develops over time its own best methodology as an aggregate gathered from successful experiences. For a number of fields, that methodology is merely observation. The "merely" implying that there is no overt manipulation of the field as there is with experimental science. Observation as scientific methodology, as was mentioned earlier, requires only that the observation be done with meticulous care. The expert should be able to find no error in the observational procedure. The replicated, well-measured observation becomes a standalone, evidential, scientific datum. And the astronomers are not to be observation's beneficiaries alone. This narrative uses the standard of necessary replication of a scientific observation in independent laboratories.

In between the rigorous qualification, and the historical standard of careful observation, resides *evidentiary science.* The Wassermann Test was included in this narrative specifically to give a full illustration of that category. This evidentiary category is preparatory to the later achievement of the strict criterion, although, in the Wassermann case, that was never achieved. In individual instances, the time required to move from evidentiary to strict may extend from a few hours to many years. In the case of Dolly, the time required to move from 227 tries to two tries for one live birth was somewhat over two years.

In much of its activity, evidentiary science shares the same laboratories, scientists, publications, funding, and collegiality with strict qualification science. What it produces is not proof, but evidence.

During an interview, a nuclear physicist wanted to illustrate the scientific method of the strict protocol. In particular, its requirement that a discovery be teachable from one laboratory to another. "I saw McKubre and I asked him about his claim to generate excess heat. He said he did it on his own. He said he did not get any help from anybody—here is what I do and something like 20 percent of the time it works. I asked him if you can tell in advance whether it is going to work? He said, 'No, I can't.'"[28]

Since McKubre was not following instructions given to him by Fleischmann and Pons, he was not doing any science that was announced by the two chemists at the University of Utah. Obviously, he was doing something in his laboratory, but this nuclear scientist was not interested in McKubre's experimental outcomes, because, by McKubre's own testimony, he was not following a Fleischmann and Pons instruction set. Whatever he was doing in the laboratory, it was not within the domain of science, at least with regard to cold fusion research. Since that nuclear physicist has a vital and continuing dedication only within science, the anomalous power generation in McKubre's laboratory over seven years was not of even passing interest to him.

One could argue that McKubre did not try to generate and measure excess heat prior to March 1989, that he learned the experimental form and goal from Fleischmann and Pons even if they came to him sensibly by means of the newspapers. Where McKubre and his staff were already involved in deuterium-in-palladium experiments (Chapter 14), the critical Fleischmann and Pons instruction step, perhaps, was to bother oneself to carefully measure the heat output, something not previously considered. Certainly, he must have learned something of critical importance from Fleischmann and Pons. Something that turned the direction of his experimentation after March 1989. Possibly the strict criterion had to be more carefully applied than was done in this instance.

Would the Wassermann Test for syphilic infection pass muster under this strict protocol? Not if it was developed and used in the manner described by Fleck. Under the strict protocol, the Wassermann Test would have to be dismissed from the world of science, perhaps to be called pathological science, because of its lack of absolute reproducibility. Its use then in thousands of clinics for decades might never have happened. The strict protocol fails to adequately limit its propensity to generate false negative conclusions.

The strict qualification as a definition of science had tried for perfection. It strove to never allow the slightest possibility that a non-science item, no matter how small, might sully scientific discourse by its presence. Three aspects of perfection were thus brought together within the discipline of nuclear science: perfect particles, a hard vacuum environment, and a proof requirement (the strict criterion) that was absolute. The supportive argument was that it was just this arrangement that enabled the field's prodigious accomplishments during the twentieth century. For our purposes, there was probably no scientific basis for deploying the strict qualification against the continuing evidence for the existence of anomalous power in the chemistry experiment first set forth by Fleischmann and Pons.

A serious disadvantage of the strict qualification criteria was its capacity to limit the scientific enterprise by generating an unfortunate number of false negative results. In the case of cold fusion, the cavalier refusal of some skeptics to participate in the laboratory experience was scientifically blasphemous. It was the nuclear physicists within the ranks of the skeptics who overtly refused invitations into the laboratory, and did so without a sense of shame. I attribute that strange behavior to their inbred protocol which disallows multiple, well-measured observations as scientific evidence. It should also be noted here that those same skeptics who held the field of cold fusion in thrall for twelve years refrained from clarifying their stance by explaining to the scientific community the methodological reasons for their continuous aversion to the laboratory observations that animated the field. The publication of two books about cold fusion by nuclear physicists did not include the needed clarification of their refusal to participate in the laboratory work.

Thomas S. Kuhn taught that patterns of scientific thought make profound changes from time to time. He called them paradigm changes and found that a new paradigm was usually patterned after the methods invoked in the latest major discovery.[29] Whether a paradigm change will result from cold fusion discoveries cannot be known until the science is set down. Also, there may be more than one discovery involved in the course of elucidating the field. The complexity of the electrochemical cell, and the inherent difficulties of establishing scientific knowledge, make that a slow process.

CHAPTER TWELVE

Protocols

There were numerous parallels between the fields of high temperature superconductivity and cold fusion research. "The new superconductors took center stage on March 22, 1988,"[1] a year and a day before the Utah announcement. Scientists discovered the phenomena of superconductivity at the relatively high temperature of liquid nitrogen during the years 1986–1988.* Its proponents proclaimed, "Billions of dollars were at stake,"[2] which sounded a little bit like the inflated estimates that were originally touted over cold fusion claims. Thirteen years later, high-temperature superconductivity had little to show in return.

A fine description of observational science is given by Robert Hazen in his book on the discovery of high temperature superconductivity. In telling the story of its discovery, he describes the critical observation: ". . . at IBM's Zurich research center [scientists] discovered zero resistivity at a record high temperature (−235°C) in a copper-bearing oxide material—a material that by conventional knowledge should not have been superconducting at all."[3] Thus science begins with a well-measured observation—one that stands alone: no theoretical understanding or explanation is required. In this case, there clearly was none. It also had the advantage of immediate reproducibility

* Superconductivity describes the ability of certain materials to conduct electric current with zero resistance. This property is well known to the physics community since its discovery by H. K. Onnes in 1911. Exceptionally low temperatures are required: approximately −270C. With the discovery of high temperature superconductivity, at −196C (liquid nitrogen), commercial applications of considerable economic value are anticipated.

that was apparently due to the availability of pure inorganic compounds and great intrinsic freedom in bringing them together in a precise mix before baking or sintering them.

There is variety of method in observation. Historically, the scientist observed simply by looking with the naked eye, aided possibly by magnification. The technique is still a principal method in fields like botany. There are disciplines where the scientist cannot experiment by manipulating the object of investigation, such as astronomy. There, the empirical sensing is *only* observation. Contention may arise over questions concerning the conditions necessary for the replication of an observation.

Structured contemplation has an important place in science. It provides theoretical underpinning to observation. It synthesizes an explanation for past observations, including those that were not originally understood. It continues from past experience to predict new observations as a guide to experimentation. Limiting theory to the explanation of observations transforms mere contemplation into theoretical science. Theoretical science thus recognizes observation as a principal source of knowledge.

Scientific Error

An important myth of protocol asserts that mistaken science can be identified easily. The methodology of pathological science was loudly touted as a shortcut to such an answer. There was, it was averred, no need to look thoroughly at what was claimed; the circumstances and the casual glance told all. Furthermore, that glance must be final; pugnacious continuance got socially punished.

From time to time, scientists compile lists of the properties that they believe are characteristic of mistaken science. Three such lists are compared. (1) Irving Langmuir developed his list (1953); (2) R. P. Crease and N. P. Samios composed their own list (September 1989);[4] (3) Robert L. Park (APS) devised his list (1995) under the rubric "What have we learned?"

Langmuir examined cases of reported scientific discovery where the claims seemed outlandish, and collected memorabilia about them. His methodology is of interest because it differs from that of the skeptics. The criteria Langmuir used was discussed in Chapter 5, page 65, but for comparison purposes, they are repeated here in abbreviated form.

1. Effect is independent of the amplitude of the cause.
2. Effect is close to the limit of detectability.
3. Claims are offered of great accuracy.

4. Fantastic theories are offered contrary to experience.
5. Criticisms are met by ad hoc excuses.
6. The ratio of supporters/critics reaches 50%, then falls to zero.

Crease and Samios argued (Chapter 6) that while they rejected the Langmuir criteria, they were still sure that cold fusion research was pathological science. They composed a list of characteristics that identified "degenerate" science. Needless to say, the list they devised fit the cold fusion experiment well.

1. Too many miracles were needed.
2. The "discoverers" were outsiders.
3. The "discoverers" have not tried to kill the discovery.
4. Inability to repeat the experiment was met by ad hoc excuses.

In Chapter 6, p. 87, it was rebutted that (1) miracles were not required in cold fusion research; (2) Samios and Crease were the true outsiders; (3) control experiments were used; and (4) science did not require repeatability, replication was sufficient. Langmuir's pathological science did not fit the cold fusion data. Only Dr. Samios accepted that conclusion publicly, although he did so with considerable indirection.

Park set forth seven rules for what can be learned from this and other episodes in a speech to the APS 1995 spring meeting at a session on "Alternative Science: Foolish, Fraudulent, and Phobic."

> (1) A Ph.D. in science is not an inoculation against foolishness. (2) Because even scientists tend to see what they expect to see, a foolish report by a respected colleague often carries other scientists along on the road to ignominy. (3) It's a thin line between foolishness and fraud. (4) Over time, foolish ideas develop a constituency that would prefer that the issue never be quite settled. (5) Most screwy sounding scientific claims—are screwy. (6) It seems unlikely that there will ever be an idea so crazy that a Ph.D. physicist cannot be found to vouch for it. (7) When a charlatan is exposed, the outrage of his victims is most frequently aimed at the one who strips away the mask.[5]

In order to apply this list, one must first identify the topic as either foolish, or screwy, or charlatanic. If that can be done, however, the lessons themselves are not needed.*

* Park also seems to believe that the use of ridicule is an appropriate mode of expression for the public spokesman of the American Physical Society.

More Myths

Beveridge recalls a description of how a great French bacteriologist* went about preparing to do an experiment.

> [He] was one of those men who achieve their success by long preliminary thought before an experiment is formulated, rather than by the frantic and often ill-conceived experimental activities that keep lesser men in ant-like agitation. . . [He] did relatively few and simple experiments. But every time he did one, it was the result of long hours of intellectual incubation during which all possible variants had been considered and were allowed for in the final tests. Then he went straight to the point, without wasted motion. That was the method of Pasteur, as it has been of all the really great men of our calling, whose simple, conclusive experiments are a joy to those able to appreciate them.[6]

This paragraph exemplifies the scientific method as it is widely taught in books and schools: the scientist performs a carefully structured experiment and from it learns a bit more of nature's secrets. While Beveridge recites an outstanding example, it was not exclusive. There are different methods for the scientific study of nature, each with variations of emphasis. It can reasonably be said that there is no one method of doing science.

The astronomer does not perform experiments on the cosmos. He is limited to inventing new ways to use his instruments, such as the telescope and the spectrometer. Beveridge puts it this way, ". . . important as experimentation is in most branches of science, it is not used, for instance, in descriptive biology, observational ecology or in most forms of clinical research in medicine."[7] In the latter case, the patient's physiological structure is not experimentally rearranged. For example, a scientist administers an experimental drug and then observes what happens.

Bauer—an electrochemist—spells out some differences in scientific method.

> Astronomy has to deal with the evolution of the universe, the birth and development and death of stars; biology and geology seek to account for the evolution of living things and of the Earth. But physics and chemistry share no such concern with inherent, directional change: they delight, by contrast in the discovery of permanent rela-

* Hans Zinsser, writing about the great French bacteriologist Charles Nicolle.

> tionships, and they do experiments in which time is just another controllable factor.[8]

Differences in the subject matter prompt differences in scientific method. The method of the evolutionary fields of study might be called, not so much observation, as watching . . . and watching. Scientists in these fields expect knowledge to be revealed slowly over many years as data about current questions aggregate. They work with a long time span of view, one that is measured in years and decades. By way of contrast, chemists and physicists often see new fields of action develop almost overnight.

There are also significant differences between the outlook of chemists and physicists. Bauer continues,

> With these and other differences among sciences come far-reaching differences in attitudes and method on the part of those who do the science. . . For example, chemists and physicists do not mean quite the same thing when they call a thing "stable": physicists mean that it is stable for all time, . . . whereas chemists mean the thing does not by or of itself change into something else *at a noticeable rate* in a normal environment.[9] (Emphasis in the original.)

These differences can be profound.

> The differences among adepts of the various sciences go beyond matters of theory, method, and vocabulary to subtler habits of thought and even to customs of behavior, to such an extent that the differences among the sciences . . . can aptly be described as *cultural* ones: they involve a great deal more than just knowing about separate and distinct aspects of nature.[10]

In 1989, every researcher assumed he ought to be able to make a cell work. If they could not, they assumed Fleischmann and Pons had withheld some piece of information. There was a great clamor for more information during the first months after the Utah announcement.

Huizenga personified this demand for adequate information. In a TV interview quite late in the episode (1993–1994), he insisted, "If someone claims that they are making and doing cold fusion, he has to be able produce a set of instructions so that someone in an independent laboratory can reproduce those results."[11] He also declared, "I still demand reproducibility and I don't see it forthcoming."[12]

Beveridge speaks to this concern with greater perspective.

> The essence of any satisfactory experiment is that it should be reproducible. In biological experiments it not infrequently happens that this criterion is difficult to satisfy. If the results of the experiment vary even though the known factors have not been altered, it often means some unrecognized factor or factors is affecting the results. Such occurrences should be welcomed, because a search for the unknown factor may lead to an interesting discovery.[13]

Reproducibility

In May 1989, the president of the APS, said that with respect to the questions raised about cold fusion experiments, in the end, the scientific method *including the need for reproducibility* will determine the fate of the Fleischmann and Pons cold fusion claims.[14] The time required to attain reproducibility was not mentioned, as though reproducibility were an inherent characteristic of a correctly done experiment.

The outstanding exception to that position was the Ramsey statement where he said that even a single episode would be revolutionary. The word *single* placed a lower bound on the requirement for reproducibility. In the modified form, a single anomalous power episode would be revolutionary. The need to validate the experiment still remained, however, even after a single excess energy period.

The experiment that produces the Fleischmann and Pons phenomenon may reasonably be called revolutionary, because it implies a violation of the law of conservation of energy (the energy appears to be emerging from nothing as no chemical or nuclear source is evident). The revolutionary science then had three immediate tasks: to develop a more replicable experiment, to find the breadth and extent of the Fleischmann and Pons phenomenon, and to determine the source of the anomalous power.

Differences between the methods of chemistry and those of physics, especially between those of surface electrochemistry and those of nuclear physics, contributed greatly to the confusion and error during the first decade after the Utah announcement. Bauer identifies a telling item of methodology that had a strong impact on the cold fusion episode. "Physicists look to crucial experiments to decide among theories at one fell swoop . . ."[15] Nuclear physicists especially look to the defining experiment that, once successfully demonstrated, can then be exactly repeated in other laboratories. These experiments are *simple* in the sense that the determining variables are under control. Once the experiment is announced, it can be repeated by any physicist versed in the art with access to a suitable laboratory.

Bauer further delineates the difference by looking at the statistics of Nobel Prize awards. "Nobel prizes in physics have been awarded about twice as often for experimental novelties as for theoretical ones, but in chemistry, experimentalists have been so honored five or six times as often as have theorists."[16] The accomplishments in chemistry are determined largely by experiment, while in physics knowledge is considered incomplete and therefore deficient until a correlation is established between experiment and theory.

These differences result from the consequence of specialization, which allows the protocol for each specialty to become individually optimized over decades of research. Bauer points out that, each science—and to a degree each specialization within each science—has thus come to be an idiosyncratic blend of theorizing and experimenting. That circumstance inevitably carried certain distinct notions about what knowledge is and about the degree to which knowledge can be said to be certain. Science encompasses a wide range of knowledge and of diverse views about the nature of knowledge.[17] Bauer characterized the field of physics as follows. "Physics is oriented towards theory: one learns physics as a set of mathematically formulated laws more than as a set of observed phenomena; theory serves as a substitute for individual facts."[18] A reading of Feynman's writings confirms that view of the role of mathematical formulation in physics.[19]

Interviews with scientists who are familiar with electrochemistry describe a field of study that is more like that of geology as described in the paragraphs below. The following excerpt from Bauer show crucial differences between the outlook in physics and in geology.

> Geology . . . is taught primarily through description . . . theory in geology is less specific than in physics and serves to explain after the fact and not as a substitute for individual facts. Naturally, then, physicists tend to regard quantitative theory as the epitome of science and of scientificity; and, secretly or not so secretly, they see geology and geologists as somewhat less than highly scientific. So, too, physicists have learned that it is possible to find distinct, single causes for the variety of phenomena with which they deal, the phenomena themselves being identifiably and distinctly discrete. And for these reasons, and also because they can control the relevant factors, physicists know that they can perform "crucial experiments" that compel nature to deliver definite answers. Geologists, on the other hand, learn that their phenomena overlap one another, that diverse "causes" conjointly produce a given geological circumstance, and that the most scientific approach is not that of seeking crucial tests but that of "multiple working hypotheses," for in geology one must, over long periods of time, be willing to countenance the possi-

> bility that any one of several competing explanations may ultimately turn out to be the best.[20]

Cold fusion research had "multiple working hypotheses" regarding identification of possible sources for the excess power that was claimed during its first decade. It was not possible to know how long it might be before the several competing theories were resolved.

The Utah announcement probably contained a multiplicity of overlapping experiments, exactly which one was running at any moment depended upon the exact experimental conditions on and inside the cathode surface. An outspoken physicist demanded one resolving experiment and demanded it immediately: "For Mr. [sic] Pons and Mr. [sic] Fleischmann[,] the best bet is to disappear into their laboratory and devise a clearly defined . . . experiment that others can reproduce. Until they've done that, they have nothing."[21] Was this not a provincial statement by one who was unacquainted with the laboratory practice of surface chemistry?

Protocol Failure

There was much talk about how Fleischmann and Pons failed to follow convention by not offering a preprint at the Utah announcement. What was not much mentioned was the failure of the community to follow convention since that time. Ordinarily, claims for a new discovery are evaluated by the scientific community. In this case, however, excuses were offered for not doing so or for giving that responsibility a quick once over, a lick and a promise, so to speak. Science was set back by this failure to abide protocol.

One can only ask, where were the chemists and their professional societies? Where were the books written by chemists that told the surface chemistry account of this episode as several books had told about the nuclear physics? Where were the activist chemists in the public forums? They were nowhere to be found.

No single one or group of them came forth to insist that heat could indeed be measured, and to argue that the calorimetry used did meet scientific standards of quality.

But that failure might have been avoided. The shallowness of the science reporting, the inability of the reporters to get beyond the misnomer cold fusion to report judgements of the controversy by chemists, was stifling. Jerry Bishop, of the WSJ, himself became controversial because he reported the views of chemists, as we will see in Chapter 22, p. 310.

It is important to keep in mind the introduction of ad hominem argument because it had an enormous impact. It introduced fear of uncalled-for public humiliation in the answering of technical questions that needed resolution by the scientific community. It greatly inhibited adherence to established protocol.

One of the world's most prestigious journals led the way into confusion by abandoning conventional protocol. In October 1989, *Nature* said, "Both sides seem to accept that the heat measurements will probably not prove convincing one way or another, and that the presence or absence of nuclear products is the crucial evidence."[22]

The four critiques (Chapter 9) of the Fleischmann and Pons calorimetry failed to undermine the experiment's credibility, but none of them could be said to constitute an adequate evaluation of the experiment by the scientific community. McKubre has reported heat bursts outside the standard deviation of random errors by factors up to fifty. The scientific community never evaluated this datum.

The truth was that the physics community was not interested in heat measurements. Their insistence that the most carefully done calorimetry ought to be ignored was a travesty of science. In scientific protocol, there is no support for this degree of prejudice. The established accuracy of McKubre's work went unappreciated by the scientific community.

Again, where were the professional societies? Can the APS show that Fleischmann and Pons defense of their calorimetry (July 1992) is flawed? At that time, the APS spoke to the subject of cold fusion studies often. Why did they not address the important aspects of it? Was the APS complicitous in seeking only punishment?

Scientists or institutions may have chosen to not respond to the Utah claims and they could not be denied that choice. Others may have chosen to wait for nuclear data supporting the claim to become available. That was fine, too. Even the declaration that one is sure there is nothing of value in the claims, was not disallowed by protocol. No individual or institution is obligated to do or say anything. Refraining from participation on the part of the individual scientist or institution was not a diminishment or corruption of science.

But those who would speak for the science community carry a responsibility to see the evidence placed before it for evaluation.

Unidentified Error

Despite the inherent clarity of the proper protocol, there is room for confusion if only because some were willing to be bold while others required a greater aggregation of evidence. EPRI was much bedeviled by this during its eight years of support for McKubre's research at SRI. Many scientists and executives at EPRI accepted the existence of excess power, but some of them did not. I think of the following protocol as the "Doubting Thomas" method of analysis.

One of the EPRI managers who was close to the sponsorship of research at SRI from the beginning has encapsulated a more wary concept of the applicable methodology. He expressed his view in April 1995 as follows.

> Indeed, the lack of any significant measurements of nuclear products suggests that proponents interpretation of the anomalous heat as real, yet unexplainable by any chemical, electrical, or mechanical source, and hence by implication a nuclear phenomena, seems to me to be at least an extremely naïve interpretation, and reflects a very poor understanding of modern scientific methods.
>
> The alternative explanation, that the anomalous heat measurements are not from nuclear reactions, but are the result of unidentified error or artifacts appear to me to be the only viable explanation of the excess heat.[23]

Those holding such concerns deplored the lack of redundancy of measurement, a concern with which everyone could commiserate. No one can be blamed for desiring a higher state of determinism in an important matter: if only there were nuclear products to corroborate those heat measurements. To resolve these frustrations, sometimes the excess heat measurements were assigned to something called unidentified error.

Dr. Morrison attended all of the ICCF meetings until his passing in February 2001. His report on the eighth meeting at Lerici, Italy, May 2000, concludes with an explanation of the methodology he has followed in his continuing skepticism of twelve years duration. He likened the presentation of anomalous power data to a magician performing magic tricks. In this view, "true believers" who present in peer-reviewed journals a demonstration of excess heat are comparable to a magician suspending a body in mid-air. This attitude is apparently due to an unwillingness to separate—at an intellectual level—empirical data from established theory. That is, to separate them for the sole purpose of thinking about them.

> I have often looked at experiments which gave results that appeared to violate the laws of Nature which had been established by previous work.
>
> Later these experiments turned out to be false, but I have often found it very difficult to see just where the error was. But the fact

> that I had not detected the flaw did not mean that the experiment was correct and that the laws of Nature had been violated.
>
> Rather I feel the same as being at a circus watching a magician. Normally he and I know that the laws of nature are being obeyed but there is a trick that is hard to spot. At [his first] trick . . ., I may spot the trick and am happy that there is no problem with the laws of Nature—similarly with [his] trick number two. But suppose at trick three, I do not see how the magic is performed. The magician may say "I won, I tricked you," and it is left unsaid that the laws of nature have not been violated. But suppose the magician says, "You did not see anything wrong with my demonstration, therefore it is true. See, I have supernatural powers. The old laws of Nature have been replaced by new laws." And if I protest, I am told that I have a closed mind, am an establishment figure, and do not face up to the happening performed in front of me. But almost all magicians admit that it is all trickery and the laws of Nature are not threatened.
>
> So if someone comes along and says, "Look—excess heat—do you see anything wrong?", then I feel as if I am at the circus, and although I do not immediately see anything wrong, I am reluctant to give up well-established laws of Nature unless the proof is very strong. Here [at ICCF-8] reports on cold fusion happenings are described, especially in the summary talks by True Believers in cold fusion in their words and then some clues as to possible explanations are offered.[24]

His apparent disregard for specialties outside that of nuclear physics, such as those of electrochemistry and calorimetry, does him a disservice. The core of his consternation over the acceptance of excess heat measurement was the concomitant need, "to violate the laws of Nature." Besides his habitual rejection of all evidence except proof, he apparently carried the notion that the laws he knew so well had to be given up if the anomalous power data were to be publicly accepted as a guide to further research. The idea that the current nuclear science of two-body reactions might reasonably endure amendment by the addition of a class of multibody, coherent nuclear reactions within the lattice of a metal under exceedingly particular conditions ought not to have been so profoundly disturbing for so many years.* If the excess heat measurements are correct, we know then that no laws of nature are violated in the production of that heat. Correspondingly, if violation of the laws of nature were

* It is this writer's observation that Dr. Morrison was for his own reasons angry and bitter at both Fleischmann and Pons. I believe that this attitude greatly influenced his judgement of the field.

necessary for the generation of excess heat, then such generation would not occur.

It should be clear to the reader at this point that the writer does not find the above quoted explanation credible, although it is certainly sincere. An assessment of it is properly included here to better understand the role scientific methodology played in the cold fusion skepticism. This treatment also offers the reader an opportunity to develop his own assessment of Morrison's explanation.

Fleischmann spoke to this question of "unidentified error," as follows.

> It has frequently been asserted that these discoveries were made by "serendipity." This view is incorrect although serendipity certainly played a part in the progress of the work. In my view, the true role of serendipity is the recognition of the significance of unusual results. It is better to guide one's research by a series of logical steps rather than to indulge in a process best described as "Gee Whiz." However, it is also important to accept and explain unusual results rather than to ascribe them to unspecified errors.[25]

There are historical examples where half a century was consumed in finding the source or cause of a scientifically interesting observation, thus establishing full redundancy of measurement. Furthermore, that long effort was the work of acknowledged science; presumably the time would have been much longer had that effort been looked upon as a pseudo-science. It ought to be considered wishful thinking to hope that the source of excess heat will be identifiable in the same era when the calorimetric evidence was first noticed. It is a further failing to label excess heat measurement as pseudo-science, thereby foreclosing the needed research to establish a science of the source. *There is no substitute for the evaluation of the excess heat data by the scientific community.*

Many substantial data sets for anomalous power were collected in the years 1989 through 1994. Only one really good set was needed according to the modified Ramsey rule, and then a second one for corroboration. To turn instead to something called "unidentified error" was a nihilistic response. It is what was said of the things Galileo saw with his telescope. While everyone knew the moon to be immaculate, the moon was seen by him to have mountains on it. This contention found resolution in the academy of the day by attributing those mountains to unidentified error in his telescope. Such a conclusion in the case of cold fusion is nothing less than an abandonment of modern science.

These transgressions of protocol expressed responses by their proponents that were fully sensible. That manner of response may have been one of the historical factors that delayed the onset of modern science until its emergence in the seventeenth century. It was counter-intuitive to realize that the well-measured observation ought to be separated from its cause. The emergence of modern protocol, that allowed the separation of research into these two parts, acknowledged simply that the observation was occurring in the present and that an identification of the cause would often not then be accessible, as was the case with Curie's too-warm radium. If one was to pursue the observation, it had to be done without knowledge of the cause. To ignore the observation, or to wait until the cause somehow became known, was to abandon the methods of modern science.

Let us systematize these differences of methodology. I bring no established expertise to this topic, but I have a rhetorical obligation to explain to the reader my understanding as best I can. Figure 12.1 depicts how the different methodologies relate to one another. It has two dimensions: the experi-

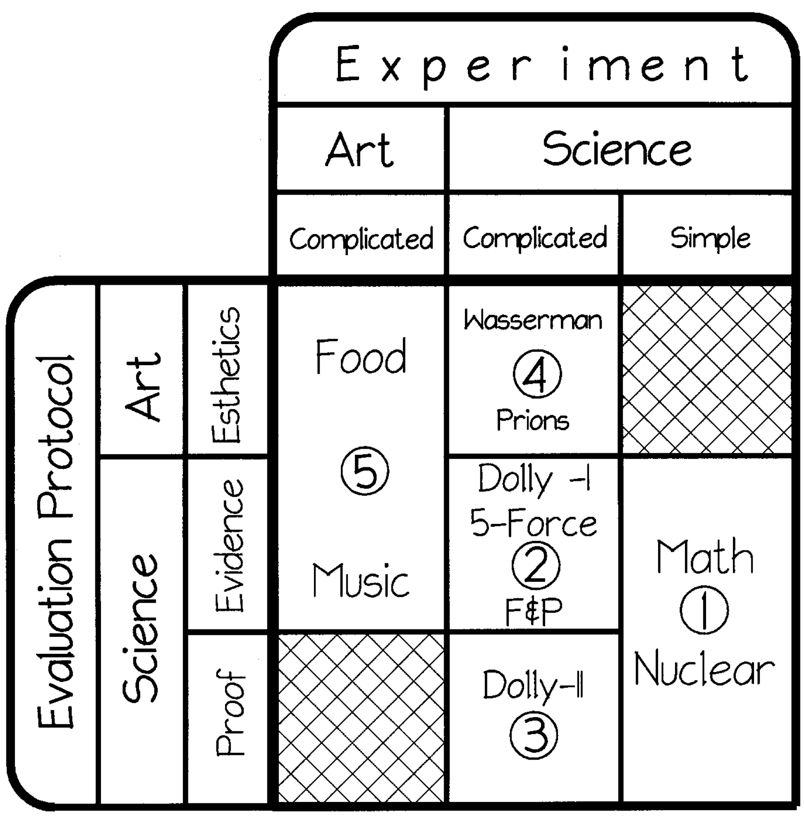

FIGURE 12.1 Variations of protocol for the evaluation of experiments in the arts and sciences with some examples.

ment, and its evaluation. The rectangles labeled 1–5 are experimental disciplines in science and esthetics. Their placement shows the relationship between the kind of experiment undertaken and the protocols that are available for their evaluation.

The word *simple* is used in the figure as the complement to Ramsey's *complicated.* In practice, simple means the natural steps are known which lead from cause to effect. The evaluation category named "proof" includes ready reproducibility or other incontrovertible outcome for an experiment. The category named "evidence" includes laboratory notebook records, measurements data collected from the experiment, publications, testimony of the scientists involved, corroboration in other laboratories, and statistical analysis of the outcome's significance. This follows the most conventional of scientific procedures.

Figure 12.1 identifies five categories of experiment by means of the numbers, 1–5, inside circles. Each has its category of "evaluation" and "experiment" protocols. Category 1, the experiment is described as a simple experiment in science: the experiment has a recipe or formula. It may connect a mathematical experiment and a required evaluation protocol that, in this case, is a proof: a theorem demands a proof and that proof is reproducible according to a recipe (derivation). Category 1 also includes nuclear physics or, at least, a significant part of it. Its pairing up with mathematics is attributable to both fields of study using ideal (perfect) elements (e.g., particles or numbers) in their experimental research, circumstance that enables both fields to routinely require proof. The extension of category 1 upward in Figure 12.1 allows for complicated nuclear experiments that might be subject to evaluation by evidence rather than proof.

In category 2, three examples are given of complicated scientific experiments. The sequence of steps between cause and effect are not known, and at the same time no means is known for achieving an (absolute) proof of the experimental outcome. Evaluation is by means of evidence, not proof. Examples given are the first announcement of a mammalian adult clone (Dolly-I, February 1997) (biology), the experiments involving research about a possible fifth fundamental physical force (physics), and the Fleischmann and Pons generation of anomalous power (chemistry). In these cases, proper methodology calls for the available evidence to be used to evaluate the experimental outcome.

In category 3, Dolly-II represents the Japanese experiment (August 1998) that had available through DNA a comparison of the source and the clone to provide proof. In category 4, I place evaluations that involve some degree of "aesthetics." This includes the Wassermann test for syphilis and the claim that prions cause disease. Specialists of the Wassermann Test considered the scientist's "serum touch" to be as important as the need to follow a recipe. It might be argued that there is only circumstantial evidence available in the case of prions.

The final category is 5, that of non-science, or *art,* where the validations are aesthetic in nature.

The Basic Rule

Most surprisingly, the cold fusion episode triggered a regression of protocol from the modern age back 400 years to the end of the Aristotelian age, before Francis Bacon. At the Utah announcement, Fleischmann emphasized the relatively few neutrons that were measured when compared with the amount of anomalous power reported. The number of neutrons was nine to twelve orders of magnitude too low to represent the source of the quantity of heat claimed. This constituted a direct conflict between a well-measured observation and contemporary nuclear theory.

In their Preliminary Note, referring to the considerable amount of power they had measured, they state "It is inconceivable that this could be due to anything but a nuclear process," and a few paragraphs later, "It is evident that [conventional nuclear] reactions are only a small part of the overall reaction scheme and that other nuclear processes must be involved," and lastly, "the bulk of the energy release is due to an hitherto unknown nuclear process. . ." They have nothing more to offer about such processes in the Note. Their full paper of July 1990 takes the same position. I take these statements to be an *hypothesis* about the source of the heat power, although Fleischmann and Pons do not use that word.

The outspoken physicists demanded that the disparity between the claimed measurement of power and the lack of evident nuclear process be resolved first. Only then, it was said, could the measurements be considered valid. The following quotes illustrate the level of argument used to turn the scientific community away from a focus on the quality of the heat measurements *as a matter of protocol.* This demand was expressed as follows.

- Conventional nuclear physics was declared invalid in metallic lattices by fiat.[26]
- Although the McKubre experiment is considered by many advocates to be the premier evidence for excess heat, no nuclear reaction products were reported![27]
- We were told that many people are finding heat. We were told that people are finding neutrons, tritium, and helium. But what was not said is that individual experiments do not see heat AND neutrons, or tritium,

or helium in amounts that would be required if a nuclear process is going on.[28]

- In the case where the fusion products were reported to be many orders of magnitude less than the excess heat, the excess heat was assumed to be due to an unknown nuclear process. This point of view was first stated by P&F. Their assumption that the reported excess heat was due to some unknown nuclear process puts the responsibility on them to delineate the characteristics of such a process.[29]
- So, delta E [energy] equals [delta] MC^2; where is the delta M [mass]? This is your redundant test. You can't identify a new science without being consistent with known . . . that is as solid as we can get in science. It is based on thousands of experiments. Serious efforts will include looking for the delta M part of the equation.[30]
- You cannot talk about having a nuclear process if you've not seen the nuclear product and the x-rays which must be present and all of these experiments do not see the nuclear product nor do they see the x-rays and, therefore, it makes no sense to talk about these reactions without seeing product.[31]
- Findings [of nuclear products] of the order of 10^{12} nuclear transmutations per watt should be relatively easy, and ,if found reproducible, would prove the claims beyond all doubts.[32]
- Fleischmann has publicly admitted that when excess heat is found, there should be a commensurate amount of nuclear ash. *The time has come to hold him accountable for this equivalence.*[33] [emphasis in the original]
- Cold fusion ". . . contradicts the foundation of nuclear science.[34]

This exclamatory language seems unquestionably reasonable, but it is not because it is the wrong protocol for science. It dismisses out of hand the evaluation that is required by protocol. It turns science away from its duty to evaluate the quality of the anomalous power measurements. To find the unrecognized reaction or the ash that results from an unknown nuclear reaction could well take nuclear physicists a generation to accomplish. In fact, it is not reasonable, it is a crippling digression.

Science embraces the perpetual task of explaining the world about us, at least that part of the world composed of matter and energy. Typically, science must search for its cause or source. There would be wasted effort in this search if the observation was mistaken, so the protocols of science require that the observation be correctly made. That confirmation is the stock in trade of experimental science.

Once upon a time, observations were discarded as illusions if their cause were not apparent. It would seem perfectly sensible to deny the observation, at least until such time as its source or cause is established; only then can one be sure. Perhaps such a wholly sensible course of action was precisely what happened during the millennia prior to the establishment of modern methodology. This was certainly the way contemporary academics responded to Galileo's description of the mountains on the moon.

Does this mean that any claim of observation must be accepted as worthy of scientific study? It does not. It means something quite different. It means that *the controversy must center about the quality of the measurements and not about the source or cause of the phenomenon.* It means that to turn the head of science away from its duty to evaluate the observation on grounds that the source is not evident is a violation of modern protocol.

At first glance, it might seem that such a protocol does not allow the left hand of science to know what its right hand is doing. Not quite. The proper concern is with the course of action to take when there is a conflict between experiment and theory. Karl Popper provides an escape from the difficulty of accepting into science an observation that went contrary to established theory. He asserts that science advances, not by proving theories correct or by defending them to the ends of the Earth, rather, by accepting (not adopting) experimental outcomes that contend with theory. His example was that the observation of a thousand white swans does not prove that all swans are white, but the observation of a single black swan undoes forever a theory that says all swans are white. More formally, a single contrary experiment proves a theory wrong forever.

Popper teaches that an experiment must not be refused admission into the inventory of science simply because its well-measured outcome runs contrary to theory. Such refusal would abrogate the canon of falsifiability. If conflicting data is prohibited from contention, then theorems are no longer falsifiable. Were science to enable such practice, it would evolve into a secular theology.

The claim for anomalous power generation in the Fleischmann and Pons cell needed no miracles and none were sought. Nor did their claim portend violation of the law of conservation of energy. Claims could be verified by replication in independent laboratories, preferably with several different cell designs, and several different types of calorimeter. This was properly done by those with expertise in electrolytic cells and in calorimetric measurements. Therein resides the correct evaluation protocol. In such a manner, the anomalous power claimed by Fleischmann and Pons could follow a legitimate validation procedure. If it were then so validated, a new field of scientific study would begin, a new science that was not beholden to theoretical suppositions about the source of the heat power. The new area of scientific research will have been established in its own right—by claim of measurement.

CHAPTER THIRTEEN

Without Exception

A single event is, in principle, sufficient to bring about a revolutionary outcome. It follows that each claim of well-measured anomalous power, *without exception,* must be undone to dismiss the claim. This is a severe criteria, but it is nevertheless what follows from the modified Ramsey premise.

It is not our purpose in this investigation to see if everything done under the rubric of cold fusion can be ratified. Our choice of the anomalous heat claim, the presenting empirical factor, and the only claim so far to hold our concern, greatly limits and simplifies the task. Its correct measurement constitutes the prime requirement, and that requirement must be met also in an independent laboratory.

The anomalous heat phenomenon belongs to an informal category of experiment that I refer to as *complicated.* The word implies that there are uncontrolled factors within the Fleischmann and Pons cell that largely influence its performance. If several cells are set up identically in accordance with the then current practice, identical processes or reactions may not occur in each of them. The chemical experiment announced by Fleischmann and Pons involves surface-catalyzed electrochemistry. The palladium cathode harbors chemical reactions on, near, and inside its surface. To some extent surface variations determine what reactions proceed, pause, or reverse. Those factors include the electrical potential at the surface and the concentration there of each species of chemical. Their concentrations will vary depending on how fast the reaction proceeds. This indeterminacy is part and parcel of cold fusion research as it is to other fields of science.

Reconsider the words of Ramsey: "Ordinarily, new scientific discoveries are claimed to be consistent and reproducible; as a result, *if the experiments are not complicated,* the discovery can usually be confirmed or disproved in a few months."[1] The Fleischmann and Pons cell involves reaction complexity at and near the surface of the cathode. Learning how to make these cells perform the reactions for which they were designed requires time and skill. It is a tough laboratory drill.

I interviewed several professors who had tried for over two years without success to get cells to function for more than a couple of weeks. A "gunk" accumulated on the electrodes after two weeks that impeded current flow and brought cell operation to an halt. In Chapter 4, p. 49, Dr. Caldwell was quoted as saying that a very minute fouling of a surface could drive a reaction in a totally opposite direction than anticipated. She remembered that for several decades there was a battle between the "big guns" of the surface-science community over whether gold was a hydrophobic or a hydrophilic metal (i.e., does the surface tend to reject or attract water molecules).*

> The two possibilities became two schools of thought. They were just at odds all the time, and these were good scientists. What happens when you strip a gold surface is that immediately it takes on material. It is fouled from air by hydrocarbons or by carbon dioxide. Whatever first comes in touch with that surface fouls that surface. The surface then acts as either a hydrophobic or a hydrophilic surface. So I am not offended by the fact that their [Fleischmann and Pons's] results were not reproducible.[2]

Allen J. Bard, Chairman of the Electrochemistry Department at the University of Texas, Austin, spoke keenly to these difficulties during 1989.

> The Utah researchers made it seem like such a simple experiment, a freshman chemistry experiment. The fact is the more you probe and the more you look at it, it's very subtle. I wasted my first two weeks by not looking closely enough.
>
> There have been times in my career when I couldn't reproduce someone else's results for a long time. So the fact that you can't reproduce it doesn't say it's wrong.[3]

And Caldwell again, "To just say that because they cannot reproduce what they are doing they are not scientists, is wrong because there are certain

* Gold is important to experimental science because it is an inert metal. Cold fusion experimenters use it for a cathode to form a control cell where they can be sure no special reaction occurs at the cathode. However, there are some claims to have experienced reactions on gold surfaces.

phenomena that are very difficult to reproduce."[4] The insistence that science requires a general reproducibility represents a kind of duck-pond thinking.

Textbooks emphasize the complex reactions that can exist on a surface like that of a cathode. For instance, a 1981 textbook on surface chemistry warns the scientist that the chemical properties of the reaction depend intimately on surface preparation. A great deal of experimental evidence indicates that each type of surface site may have a different chemistry. Contamination by a single layer of molecules is almost instantaneous: after cleaning "the surface may be covered [with contamination] in a fraction of a second . . . The [contamination] may impart to the surface unique chemical properties by blocking sites or changing the oxidation states of surface atoms . . . The constant presence of the [contaminate] layer may influence the chemical, mechanical, and electronic surface properties."[5] One might wonder how such a surface could be used at all.

There were other problems with cathodes. The metallurgy of the cathode was also critical to cell performance. The presence of micro-cracks inside the surface may have allowed deuterium gas to escape so that adequate loading was not achieved. These cracks may have formed from the pressure of the initial loading if the metal was too weak. Different batches from the supplier may have had quite different properties in that particular regard.

Complicated Experiments

In February 1997, a laboratory in Scotland tried their experiment with mammal cloning more than two hundred times before achieving success.[6] Clearly, there were unknown factors in the chemistry and biology of that experiment which contributed to the outcome and made success difficult. Fortunately for them, the scientific community followed conventional procedure in its response: their numerous failures along with the failure of other scientists to replicate the feat were largely ignored. In August 1998, a Japanese group reported that they had replicated the feat of cloning a mammal. Pending review, that report was accepted as validation of the Dolly claim. In the Japanese case, the use of DNA testing made proof accessible to the experiment. Easy or hard, the scientific community handled the matter properly. Science accepted the observation of a successful cloning event even when each step that causes or helps the cloning process to happen was not yet fully known.

I mentioned earlier that there was an analogy between the perfect particles of nuclear physics and the perfect numbers of mathematics. The contrast between the perfection of nuclear particles and the chaotic structure of a chemical surface was clear. A similar dichotomy of discipline exists within physics. Wolfgang Pauli, a renowned physicist, spanned this dichotomy at the peak of his career in 1932. He was thoroughly versed in the nuclear physics of the day, but he was also one of the founders of solid state physics. He expressed the anguish of the scientist who moves from the neat world of nuclear physics to the sordid world called semiconductors. “I don’t like this solid state physics . . . though I initiated it. . . . One shouldn’t work on semiconductors, that is a filthy mess; who knows whether they really exist.”[7]

Can any experiment be validated if it is in such a primitive state of development that its details are not known? The chemist, the biologist, the geologist, will generally answer that it is done, though it is not easy. Initial difficulty of replication is not uncommon in scientific specialties outside of nuclear physics.

Caldwell offers this outlook.

> We cannot be too moralistic in terms of judging Stan and Martin for experiments that were irreproducible, because they were inherently working with very difficult material, that is, the electrode surface. If you poison the surface with one leachable material or another (e.g., from the glass flask), you would just have different results.[8]

Experiments involved with outcome determining factors that were uncontrolled appear frequently in laboratory work. Fleischmann put the matter directly, “Getting catalytic reactions to work is always a struggle; but we do it all the time.”[9] Allen Bard seemed to agree, “So the fact that you can’t reproduce it doesn’t say it is wrong.”[10] These views were in keeping with our picture of experimental science, at least that part of it which differed from that of using perfect pieces like atoms or neutrons or numbers, and from working in the more perfect environment of a hard vacuum or with a blackboard. Bearing this in mind, scientists must ultimately track down those additional factors and eventually bring them under experimental control.

Excess power sometimes happened in a burst whose time of occurrence could not be predicted. Figures 4.1 and 4.2 in Chapter 4 recorded such an event. This spontaneous behavior was not a barrier to a claim of their existence. Scientists long knew how to observe and record spontaneous events whose timing could not be predicted. The measurement techniques were well developed. Automatic monitoring techniques recorded those events. For example, lack of predictability did not prevent scientists from observing and measuring seismic events.

A Fleischmann and Pons type of electrolytic cell is set up and left running with the recording equipment attached. After many weeks one looks at the data and, with a little bit of luck, there will be a burst of output power on the record. But it may still be necessary to explain to the naïve scientist why a burst cannot be scheduled for his Thursday visit.

The government Panel did not review the burst phenomenon even though it was known about as early as May 1989, nor did it provide for its evaluation by others, either then or later. Two power burst reports are included in this book, one by Fleischmann and Pons (Chapter 4), and the other by McKubre (Chapter 14). The bursts's behavior—turning sharply on and off during a continuously running measurement—constitutes a secondary, but important, form of anomalous power corroboration based on its dynamics. The observer can see in the tracing its sharp "on" and "off" characteristic. Skeptics of the anomalous power claims will have to suggest other interpretations of this phenomenon if they wish to prevail.

When planning laboratory work, the scientist envisions the whole process that will unfold during an experiment. The expected magnitudes of each measurement are estimated so that the instruments are ready to make the desired measurements. What temperatures will the experiment produce? Those temperatures must be measured without undue degradation of accuracy. If there is ionizing radiation expected, will it be dangerous? In 1989, those who assumed that the primary signature would be neutrons were mistaken and disappointed. The experiments carried out prior to the Utah announcement established that the signature of the cell was heat energy flow (power).

The high cost of cold fusion experimentation is another important consideration. The definitive experiment runs for three months. Each year allows enough time for approximately three successive experiments in which the results from one can inform the design of the next. The work on high temperature superconductivity allowed more than two dozen experiments to be completed over an extended weekend, while the same number of cold fusion experiments might require eight years. Such experimentation is not only time consuming but expensive.

The corroboration and eventual validation of a complicated experiment proceeds by replication in independent laboratories. If no one is able to accomplish replication after the passage of several years, the original claim is usually dismissed as an aberration of some unknown sort. The claim is tentatively validated when replication is accomplished in an independent laboratory. Only tentatively, because the independence of one laboratory from another cannot be taken for granted. Both might follow a similar mistaken procedure, for example. Validation requires that the replication be truly independent.

As the number of corroborations add up, validation takes on the aspect of certainty. Each corroboration must be shown to be in error, if it is to be discredited. If those successive corroborations utilize a variety of cell designs, and if their respective calorimeters utilize a variety of techniques, the prospect of disqualifying them by finding an error that was common to them all becomes vanishingly small. This variety is shown in the next chapter when actual heat generating experiments are examined.

The cold fusion experiment consisted of the most ordinary kind of laboratory science. Its new aspect was that of emphasis on packing a lot of deuterium into the palladium and keeping it there. A high order of technical skill was needed to build and run the cell in that mode, even though it was a standard piece of chemistry equipment. Most of those who jumped into the field after the March 1989 announcement were not prepared to work for two or more years to master cell peculiarities. McKubre and Bockris and other electrochemists came to the field so prepared.

Calibration

While Chapter 1 provided a qualitative view of anomalous power (page 6), it was pointed out there that the evidence was independent of whatever calibration technique might be adopted to put that evidence on a quantitative basis. In Chapter 4 there was an example of excess heat accompanied by detailed measurements of the quantity of energy. The instruments that are used to get such measurements always need to be calibrated. They must be referenced to absolute standards that are maintained by the Federal government. They must also be checked from day to day in case their settings have drifted. How is the power output of a cell determined?

An overview of one method is given here. But with any calibration system, it is important to use several different measurement methods so that an error in one will not be hiding in all of the readings. The interested reader will find many more details in the references.

The Fleischmann and Pons type of cell design passed its heat energy through the flask walls into the bath water by means of radiation, not by conduction. The bath was held at a constant, lower temperature by circulating the water through a temperature regulating unit, by keeping it well insulated, and by stirring it.* The electrolyte was held at a uniform temperature by the vigorous bubbling action of cell operation, by the narrow inside diameter of the cell, and by the insulating effect (from conduction) of the Dewar's hard vacuum.

A control cell is one that does not generate anomalous power and, therefore, will demonstrate an equal amount of power in and out. How much goes in is known from the power supply current and voltage values with an allowance made for the electrolytic action. The temperature of the cell is continuously measured and that of the bath is known. The transfer factor of cell heat into the bath is calculated from the two temperatures and the known output

* The bath is held to a temperature in the vicinity of 303.15 Kelvin and is uniform throughout to within ± 0.01 K except within 0.5 cm of the bath's surface as reported by Fleischmann and Pons.

power (equal to the input power). This measurement technique was augmented to satisfactorily accommodate inadvertent energy leakages and so as to remain stable over many months.* The two temperatures and the calibration factor are used with an active cell to calculate the generated excess power.

If the heat out is larger than the heat in, then the cell is generating anomalous power, the Fleischmann and Pons phenomenon. In one sense, the measurement is an easy one because the exact magnitude is of little importance; the overriding question is whether excess heat exists *as a natural phenomenon.* Namely, is there a difference other than zero between the operation of a control cell and one that claims to be generating excess heat? That question places a minimal burden on the measuring technology.

In the planning of an experiment, it is important to set up control cells in such a way that their behavior can be compared with the active cells. Control cells have to meet two somewhat conflicting requirements. The control should be as nearly similar to the experiment as is possible, but sufficiently dissimilar that it is free of the effect to be explored. The Fleischmann and Pons experiment made such planning problematical because the specific source of the heat phenomenon was not yet known. That there is a large difference between heat generating cells and quiescent cells is shown in their first publication of April 10, 1989. Three of the cells shown there perform in a distinctly different manner from the fourth cell. This difference is referred to as the generation of anomalous power in the three cells. The fourth cell may be considered a control on the other three.

Wilson correctly argued that if the experimenter achieved an energy balance when anomalous power was not evident, a suitable calibration was achieved. Fleischmann suggested the use of a spent palladium cathode in an otherwise complete cell to make it into a control cell. Another approach was to use a platinum or gold cathode. In principle, any of these techniques will make an adequate control cell.

The following characteristics of the different kinds of cells should be kept in mind:

1. Some cells do not have a thermometer in the cell as their calorimeter does not require it. These cells are not dependent on uniformity of temperature within the cell.
2. In some experiments, the degree of recombination of the gasses is monitored and reported.

* Other important heat losses result from heat conduction to the bath, from conductive loss through the exposed top of the cell to the atmosphere, evaporation into the cell's top space from the electrolyte's liquid surface, and from the evolving gasses and the electrolyte's vapors that carry heat out of the cell as they egress.

3. In a closed cell a recombiner is used within the cell so that the oxygen and hydrogen gas molecules are deliberately recombined into water.

The phenomenon under consideration can also be recognized somewhat more simply as the difference between a failed cell experiment and a successful one. That difference is the generation of excess power. For example, at the NSF/EPRI meeting Fleischmann reported on 8 active cells and 13 control cells in which no anomalous power could be detected. Another 23 experiments in the same group showed generated power greater than 20 mw, with 15 above 100 mw. The Fleischmann and Pons phenomenon is exhibited as the difference between these two sets of experimental outcome.

Chapters 1–13 provide the background needed to understand how to interpret the Fleischmann and Pons cell performance. In Chapter 14 we look at seven examples of excess heat corroboration.

CHAPTER FOURTEEN

Validation

The intrepid pilgrim has arrived at the holy grail. The alarums and diversions of failed experiments, failed recipes, and failed theories are defeated. The demand for proof is seen as an invitation to preemptive failure of the investigation. A cautious attention to the norms of protocol is rewarded. The most significant claim of Fleischmann and Pons—anomalous power—is now to be validated.

Validation is an ongoing process that becomes more secure with each successive corroboration. Protocol ordinarily allows the original experiment full confirmation if it is successfully replicated once. That corroboration was properly done in the fall of 1989 by Oriani who submitted his report of anomalous power corroboration to *Nature* magazine, but the submission was refused for wrong reasons.[*,1]

With confirmation, an experimental observation is admitted into the company of mainstream science even if it conflicts with theory. During that admissions process, the confirmation must include a full consideration of possible systematic error, error that may be common to every trial. For example, the observation of extraordinary cosmic expansion may conflict with other data and with theory, yet those observations are allowed into the discourse of science.

* Two reasons were given in *Nature*'s refusal letter of January 26, 1990: the lack of evidence of nuclear ash in Oriani's experiment, and the difficulties with replication of such experiments in general.

Replication

Seven examples of replication will suffice for this investigation. A reference list of additional replications follows them for those readers who would like to study this essential topic further. This replication record includes the first six years, 1989 through 1994. Successful replications continued, and will continue, but they have in them little to add to the overall account.

The first two examples concern replication only of the data reduction calculations because that is such a difficult and important topic. The unevaluated time-voltage-temperature series of data in these two examples came from Fleischmann and Pons's cells. Their data was then reduced (evaluated) by an independent scientist. Following these two examples, five experimental replications are selected from five independent laboratories that reported generation of anomalous power.

W. N. Hansen

The first evaluation was done by Professor Wilford N. Hansen, Physics Department, Utah State University, Logan, Utah.* The Utah State Fusion/Energy Council commissioned him to do an evaluation of some Fleischmann and Pons's calorimetric work and he delivered his report in the spring of 1991.[2] Hansen received eight sets of data that Fleischmann and Pons recorded from eight cells. His task was to complete an independent data reduction to answer one question: did the data set demonstrate that anomalous power had been generated in the cells?

For each of eight cells of the general type shown in Figure 1.1 and 3.1, the data series consisted of three columns of numbers: time, cell temperature, and cell voltage (potential). These three values were recorded every few minutes from the moment the electric current was turned on as shown in Figure 14.1. The experiments ran for less than two weeks during the winter of 1989–1990. These cells used the improved Dewar flask with a harder vacuum and silvering about the neck.

Two of the eight cells were control cells identical to the active cells except that platinum replaced the usual palladium for the cathode, while one of the two used heavy water and the other used light water. The cells reported on here had palladium cathodes and used heavy water electrolyte. Hansen was also given ancillary information about the individual cells such as the cathode size, the nature of the electrolyte solution, the amount of current at which the

* In Chapter 9, p. 116, a summary of a part of Hansen's work was given in a review of the various critiques of Fleischmann and Pons's calorimetry.

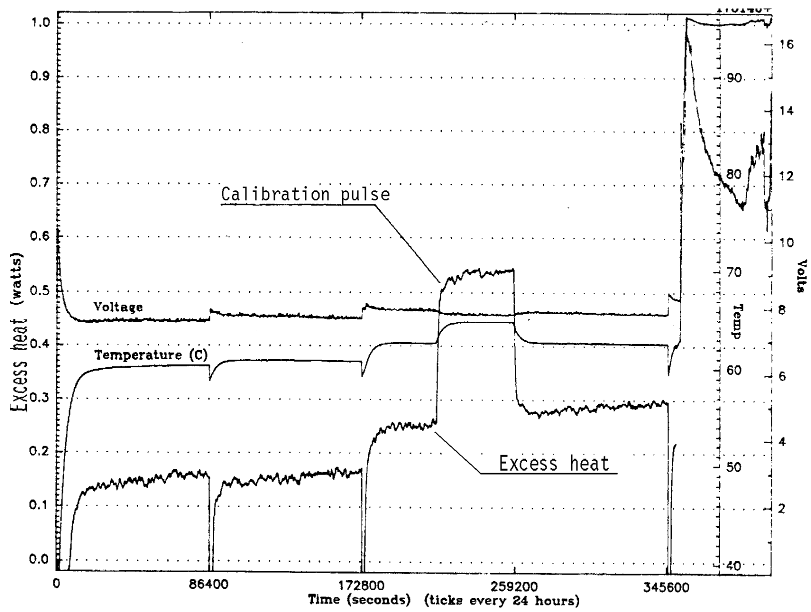

FIGURE 14.1 Hansen, Utah State University, Logan, reported anomalous power of 300 milliwatts with a 27 milliwatt reference pulse. Data was taken from a set of Fleischmann and Pons cells. (Ignore the data after the last 24 hour time tic.)

cell was operated, the bath temperature and the value of the pulses of heat introduced into the cell for calibration purposes.

Hansen had his choice of several methods of reducing the data, and by using more than one, he was able to compare the effects of different methodologies. After filtering the data,* and choosing initially to use the Fleischmann and Pons heat emission formula, he tried various methods of applying the calibration information. At least one cell showed significant anomalous power generation for each of several calculation methods. The control cells showed a slightly negative heat generation, indicating that the formulae resulted in an underestimation of generated power.

Figure 14.1 displays the data from Fleischmann and Pons's cell number five. The calibration for this illustration was determined by adjusting the calculations to set the height of the calibration pulse to 27 milliwatts, the value of the calibration pulse power. The tracing labeled "excess heat" is the amount of power being generated by the cell at each point in time. It may be anomalous or it may be a calibration pulse. It is shown for four days starting at 150 milliwatts for the first day and increasing to 300 milliwatts. Hansen estimates

* Hansen used the mathematical method known as a Kalman filter.

the accuracy of his computations at ±2%. The data noise level can be seen from the figure. The amount of energy involved is expressed in units of electron-volts.* Hansen gives his conclusion for cell number five.

> Just for the two days [out of the cell's operating period] this corresponds to 45 electron-volts (eV) [of generated energy] per palladium atom. This amount is already an order of magnitude larger than the energy it would take to vaporize the entire palladium electrode. We have thought of no other self-consistent explanation [other] than that the excess heat is real and very significant.[3]

Analysis of cell number two reached the conclusion that, "The integrated excess heat is . . . about 1,700 eV per palladium atom. This is about 400 times the vaporization energy of palladium for the electrode of cell 2!"[4] For cell number six, he said, ". . . there is about 6,000 eV per palladium atom of excess energy, or over a thousand times the energy required to vaporize the electrode. To put it this way, . . . we are not dealing with known chemistry or metallurgy. At issue is a profound energy source."[5] An independent scientist came to this conclusion after conducting a detailed analysis of several sets of cell data that had been collected by Fleischmann and Pons.

R. H. Wilson

R. H. Wilson et al. at General Electric published a critique of the initial full length paper by Fleischmann and Pons which we discussed in Chapter 9, p. 117.[6] Wilson comes into this court (of validation) as a reluctant witness, brought to the bar by the bailiff: he and his cohort insist there is no such thing as excess heat.

In 1991 Wilson et al. recalculated the cell performance as presented by Fleischmann and Pons to take into account what they felt were several technical oversights in the original paper. Wilson still found that one Fleischmann and Pons cell generated approximately 40% anomalous power compared to the power put into the cell. This amounted to 736 milliwatts. This level of anomalous power was more than ten times larger than the error levels associated with the data.

But the cases of Hansen and Wilson are only the reworking of Fleisch-

* The electron-volt is a measure of energy used by scientists at the atomic level of calculation. Hansen calculated the excess heat generated for each atom of palladium in the cathode. Chemical energy levels are about four electron-volts maximum.

mann and Pons cell data. Now a look at replication of the entire experiment is in order.

M. McKubre

Michael McKubre achieved confirmation of anomalous power during the period from August 1990 through February 1991. Because he was a central figure in the cold fusion story, a view of his background will offer some perspective.

Michael McKubre started his university education in Washington, DC, when his father was with the New Zealand embassy. He went to high school there, and then for a couple of years to The George Washington University. He was back in New Zealand to complete his Bachelor's degree, Master's degree, and Ph.D. in Chemistry, Geophysics and Electrochemistry. As he explained it, "All during my Ph.D. studies, particularly in electrochemistry, scanning the literature and attending the conferences, it became clear that of all the places in the English speaking world, the University of Southampton was the clear leader in electrochemical research." In Chapter 2, p. 31, McKubre appears as a student in Fleischmann's electrochemistry department. It was with considerable trepidation that he entered the graduate program in chemistry at Southampton in 1977. Here was a boy from the sticks of New Zealand preparing to compete in the big arena.

The department of chemistry at Southampton was quite large. Its preeminence was due to the presence of two individuals. Graham Hills (later Sir Graham Hills) was McKubre's post-doctoral supervisor and mentor. Martin Fleischmann was the chief electrochemist there, and one of considerable global recognition. At that time, the department was considered the leading academic institution in Europe for electrochemistry.

McKubre was accepted in the graduate program, and spent, ". . . a delightful two years . . . Two glorious years at Southampton learning a great deal about electrochemistry and the philosophy of science in the real world." From there, he went directly to SRI International, Menlo Park, California, a private research institute, where he has spent his working career.

SRI International is a well respected commercial research firm near the campus of Stanford University. At SRI McKubre came to cold fusion studies with a running start. The group working there through 1994 was essentially the same group that had worked there in 1988 prior to the Fleischmann and Pons announcement. They were then developing a palladium wire sensor to detect hydrogen in the cooling water of a nuclear CANDU reactor at the Canadian nuclear facility in Chalk River, Ontario, Canada. The measurement of hydrogen levels in the water was accomplished by observing the change of resistance in the wire as the palladium took up hydrogen (deuterium) from the heavy water. By March 1989, the group was already well versed in the technology of palladium hydrides.*

"We understood the electrochemical interface which controls the uptake of deuterium into the metal," explained McKubre. His group understood the means by which one can measure high loading of deuterium into palladium. They reasoned that since others had worked reasonably intensively with the chemical system before and observed no unusual behavior, if the Fleischmann and Pons phenomena did exist, it must be at the highest loading levels. These levels must be greater than 0.6 D/Pd, which had not been obtained previously, at least as reported in the open literature.

In McKubre's words,

> We set about it in a different way from most of the more famous people, other people whose experiments are now well known. We didn't attempt to reproduce the Fleischmann and Pons experiment as understood by close examination of newspaper clippings, for example. We didn't build a cell like theirs at all.
>
> What we did was take the hypothesis that under conditions of high loading in an electrochemical environment, the deuterium palladium system could be made to give off heat and possibly nuclear products.
>
> Given that hypothesis, how would you go about testing it? We devised an experiment that we believed would achieve those conditions independently of any knowledge of the electrochemical apparatus and cell geometry of Fleischmann and Pons. So the first experiments we did were at elevated pressure and reduced temperature, both of which favor achieving the high loading conditions. We didn't use open cells. It was an electrochemical cell with a modest over pressure of deuterium. We worked at about a thousand pounds per square inch of pressure of deuterium (D_2).
>
> We started working with the elevated pressure and reduced temperature experiments and obtained good loadings that were reasonably reproducible. In our first experiments we saw what we thought was evidence for excess power.
>
> By this time the furor was starting to erupt. Other people had done experiments and not observed results. We sort of forced ourselves to make haste slowly. We didn't want to be the first to repro-

* When a metal has absorbed enough hydrogen to affect its physical properties, it is referred to as a hydride.

> duce Fleischmann and Pons's results. We wanted to be the first to understand, a somewhat vain hope looking back these six years.[7]

No one in the McKubre team was a calorimetrist. McKubre says that he really did not understand the calorimeter that initially had been somewhat hurriedly put together, "We were reasonably confident of the result, and simply sought to test that result in a somewhat improved calorimeter." The strategy was what scientists call trying to prove your experiment wrong, which is a most important step. They designed a heat power measuring system of the type called a mass flow calorimeter. That took a great deal of time, perhaps two or three months, in which they did nothing but think about calorimetry.

McKubre was one of the first to appreciate that heat was the primary signature of the Fleischmann and Pons experiment. Here is the account of their exploration into the measurement of heat power.

> Calorimetry is a somewhat medieval discipline. It has not been practiced seriously by any large numbers of individuals since the 1950s. One of the things necessary, and a very positive consequence of the cold fusion experiments that have been performed, is that the quality of calorimetry in electrochemistry is [now] much improved. We have dragged calorimetry into the twentieth century.
>
> Computers had never been used in calorimetry up until we and others started using them. Calorimetry was performed by strip chart recorders. You calibrate on day one; you perform your experiment on day two; you calibrate on day three; you interpolate your results, and that's how you do it.
>
> One of the attempts we made to validate our method of calorimetry was, in fact, to take the design and the results, including excess power results, to an annual calorimetry conference and expose it to the experts. We said: We are farm boys, we don't know what we are doing, but we did this, we saw this result; tell us what is wrong with it.
>
> They didn't tell us that anything was wrong with it by and large. Did we receive acclaim? No. But a rather grudging acceptance of the mode of calorimetry that we had developed. As yet nobody has ever told me of an issue or problem with the mode of calorimetry that we developed. Which is one reason we stick to it.[8]

McKubre had developed a different kind of calorimeter from that of Fleischmann and Pons. It was an isothermal flow calorimeter that operated at a constant temperature and was insensitive to temperature gradients within the cell. This was quite different from the isoperibolic calorimeter of the

Fleischmann and Pons cells. By using a different kind of instrument, McKubre hoped to avoid systematic errors that might lie hidden in the isoperibolic design.

McKubre and his team performed four experiments with a high deuterium loading in the palladium metal. They observed anomalous power in three out of four closed cells. This, in itself, corroborated the core of the Fleischmann and Pons claims. Unfortunately, it took place after the scientific community had closed its eyes on the subject. McKubre commented on the fourth cell, "We didn't get what we now know to be acceptable loading, and didn't observe any excess power."

Figure 14.2 shows data from one of McKubre's cells (P12) after 53 days of operation.[9] It started to generate excess heat energy on day 53, and continued to do so for 12 days. For 7 of these days it generated 0.9 watts of power continuously, as read on the left-hand scale in watts. This added up to 0.54 megaJoules of energy over the twelve days. This amount of energy would operate a 1500 watt stove burner at a cherry-red setting for 6 minutes.

Once the electric current is started in a cell, it may be adjusted in amplitude from time to time. In Figure 14.2, the solid line of straight segments marks the value of current density as read on the left scale in amperes.* On day 53, the current was "ramped" steeply upward from the previous value of 0.04 amperes. The current was then abruptly dropped to the original value and ramped upward again, and so forth. In this experiment, the loading amounted to approximately one deuterium atom for each palladium atom (D/Pd = 1).

Figure 14.2 also labels the "uncertainty," in watts, in measuring the anomalous power. The amount of power measured was much greater than the uncertainty in its measurement.

McKubre was not given a recipe for the experiment. He perceived that deuterons had to be jammed into the palladium, and he had to know how to run a cell for seven weeks without the cathode gumming up. Neither of these criteria lent themselves to recipe type formulation. Both came from training and experience.

The McKubre team went through a dry spell, as did Fleischmann and Pons. There was a whole series of experiments that did not yield excess heat, and it took a fair amount of strength of character to persevere. McKubre explains it:

> One of the reasons this group of people are in it, is that we were well positioned in the first instance. That was luck. But the group of peo-

* Current density is the value of current entering each square centimeter of the surface of the palladium cathode. Total, or actual current, is the current density multiplied by the cathode surface area.

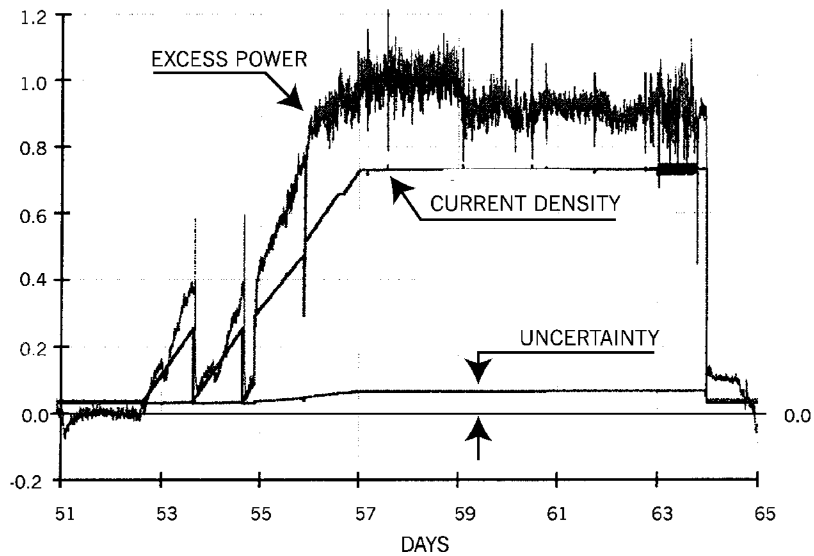

FIGURE 14.2 McKubre reports an experiment showing an excess heat generating burst.

> ple I have here including two New Zealanders, an Englishman, and Tanzella are a pretty tough minded group of guys. These were people who were confidant of what they had seen with their own eyes, and really didn't care that much about non-scientific criticism. We tried to concern ourselves very much with scientific criticism, and to this date I've never had a substantive criticism of our calorimetry. Not only has no one ever followed one of our publications with a refereed criticism, but neither have we had the precursor to it: a polite person, having such criticisms would approach you saying, I have these questions. How do you answer this?[10]

McKubre's technique used the most conventional kind of electrochemistry, exactly the sort of thing he was taught how to do at school, except that he pushed hard on a couple of the margins. His work was done entirely within the bounds of conventional science. Only the results were new. He concluded that excess power was observed when three conditions were met. The average loading of deuterium in the palladium cathode was a ratio of about one, e.g., one atom of deuterium to one atom of palladium. The loading was held for a long time, and then current was applied in excess of a threshold value.

Figure 14.2 confirms the anomalous power claim of Fleischmann and Pons. What they had done in Utah was now replicated in an independent laboratory in California using a different kind of cell and calorimeter. McKubre continued this confirmatory experimental work for nearly eight years. During that time there was no criticism of his calorimetry practices directed at him or published in the scientific literature.*

R. A. Oriani

McKubre's results were not an isolated verification of the Fleischmann and Pons phenomena. Richard A. Oriani is a professor emeritus at the University of Minnesota, Minneapolis. His experiments were done in the summer of 1989.[11]

Oriani introduced the innovation of a cylindrical glass partition between the palladium cathode at the center and the platinum anode wrapped against the inside wall of the flask. This glass was perforated with fine holes that allowed the electrolytic action to take place while separating the oxygen and hydrogen bubbles in order to ensure that any residual recombination was negligible.

When you are looking for experimental validation, it is essential that a common fault not reappear in different laboratories. It is of special importance that Oriani used a Seebeck-effect calorimeter. The Seebeck design surrounds the cell with more than a thousand thermocouples (tiny metallic devices that respond to temperature differences) connected electrically in series, each of which registers temperature differences from inside to outside by generating a small voltage.

One advantage of this kind of calorimeter is that it is not affected by the distribution of temperature inside the cell. That is, the measurement of heat power was independent of the cell's spatial temperature distribution. Calibration of the Seebeck measuring system was reported to be correct to within plus or minus 0.3% or ±40 milliwatts, whichever was greater.

Figure 14.3 displays input net power against the Seebeck calorimeter's output voltage. Points that lay along the diagonal line record equal input and output power, a normal condition for a control cell, and it means that the energy is fully accounted for. If the experimenter did his work well, quiescent operation will lay at a point exactly on the diagonal line, thus providing a fine control of the experiment.

The seven tiny "x" marks are visually located by the short, straight line that points to each. They represent a run that used ordinary (light) water and, as expected, they lay on the diagonal over the full range from a power level of a few hundred milliwatts to nearly 18 watts.

* During these years, McKubre was funded by the EPRI. Their interest expired with the vast changes in the electric utility industry's regulatory environment in the mid-nineties. He and his group continued to work in cold fusion under other sponsors, but with more diverse objectives than measuring anomalous power.

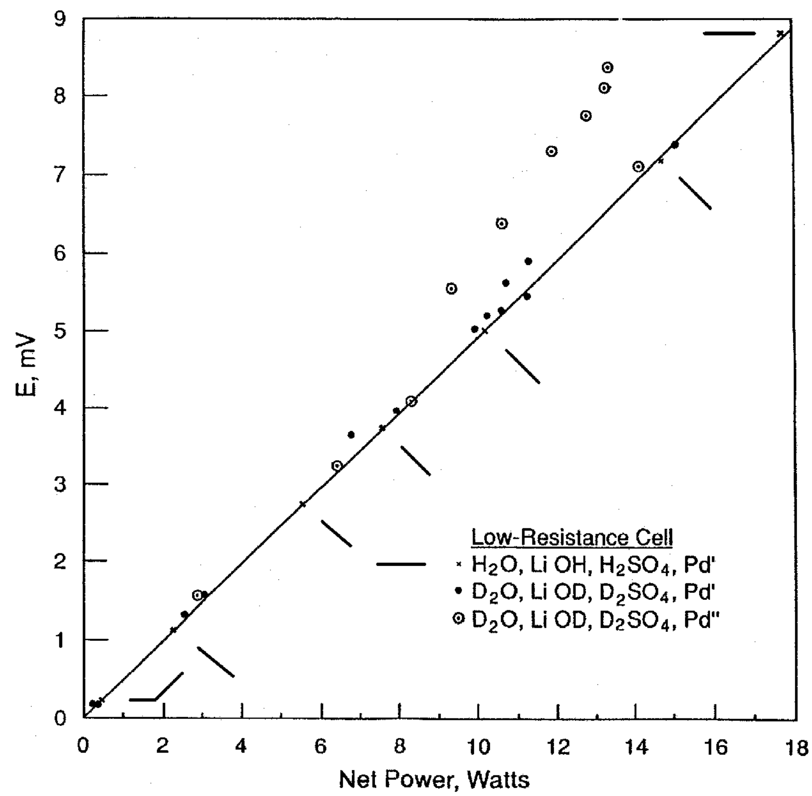

FIGURE 14.3 Oriani, University of Minnesota, reported excess power in a heavy water/palladium cell (dots inside circles) that achieved 3.6 watts for 150 minutes (top circle), or 106 W/cm^3.

If a point fell above or below the line, then one had either a serious error or a scientific revolution. A point below the line would mean that energy was somehow disappearing from the cell, and a point above the line would mean that energy was somehow appearing in the cell. Either case was an apparent violation of energy conservation. Oriani reported that the many points above the diagonal were a generation of excess power that cannot be accounted for: the anomalous power of Fleischmann and Pons.

Two runs using heavy water are shown as a sequence of points on Figure 14.3. The more interesting one, indicated by a dot within a circle, displays substantial amounts of power generation at various levels of input power. Oriani considered the hypothetical possibility that the energy might have come from chemical (storage) activity, and found that such activity would have produced a point below the diagonal while it was storing up the energy that was to be released later. No such negative intervals were found.

The highest point reached during the run was held for 150 minutes and signifies the generation of 3.6 watts ± 0.2 watts of anomalous power in the cell.* The calculated energy generated during that time was 32.4 kiloJoules. The energy density was 106 watts/per cubic centimeter of palladium. Total energy generated during the run was 200 kJ.

The size of the dots in the figure correspond to the amount of uncertainty in dot location. Notice how the "x" marks which define control cell operation are tight against the diagonal. Six of the excess heat's open circle dots are well separated from it. This separation demonstrates a good signal to noise ratio in the data.

R. A. Huggins

Robert A. Huggins, professor of materials science at Stanford University, started early with experiments designed to generate anomalous power. Like so many others, his first work in 1989 was fraught with difficulties and these were widely reported by the skeptics.

Like McKubre, Huggins was well qualified at the time of the Utah announcement. He was working with electrolytic cells to investigate the properties of solids, and hydrogen in palladium by using the metal as a membrane through which hydrogen and lithium could pass. He also had available "glove boxes" so that materials could be handled uncontaminated by the hydrogen in the water vapor of the air. He used a metallurgical arc-melter device that was immediately put to work purifying the only palladium they could get their hands on, a quantity used in earlier work and thoroughly contaminated with other material. His sample preparation was unique—multiple remelting in a high purity argon gas environment to remove dissolved gasses from the palladium.

While McKubre aimed at a high level of loading, Huggins tried a fine grain structure in the palladium cathode. He achieved this by forming a round ingot into a flat electrode by pounding on it with an instrument called a hammer.

He took some time to move up the learning curve, as did other experimenters. His team did a number of experiments and was fully convinced that something of interest was happening at least some of the time. He saw excess heat in three of the first four cell runs. He was not put off by the failures in other laboratories.

* This cell used a palladium cathode of 99.999% pure metal, and a solution of lithium deuteroxide with sulfuric acid (made with heavy water).

His first calorimetry technique was to compare the performance of two cells, one with heavy water and the other with light water. The measurements were not good because of the multiple differences between the two systems. Huggins thought he was seeing excess heat in the heavy water cell but it was not a satisfactory approach.

A new cell and calorimeter design was ready by the fall of 1989. It was delightfully simple in concept and execution.[12] The principle on which it operated can be seen by examining Figure 14.4. Huggins's calorimeter used two aluminum cylinders with the electrolytic cell located inside the inner cylinder. The cell's heat passed outward to the inner cylinder raising its temperature

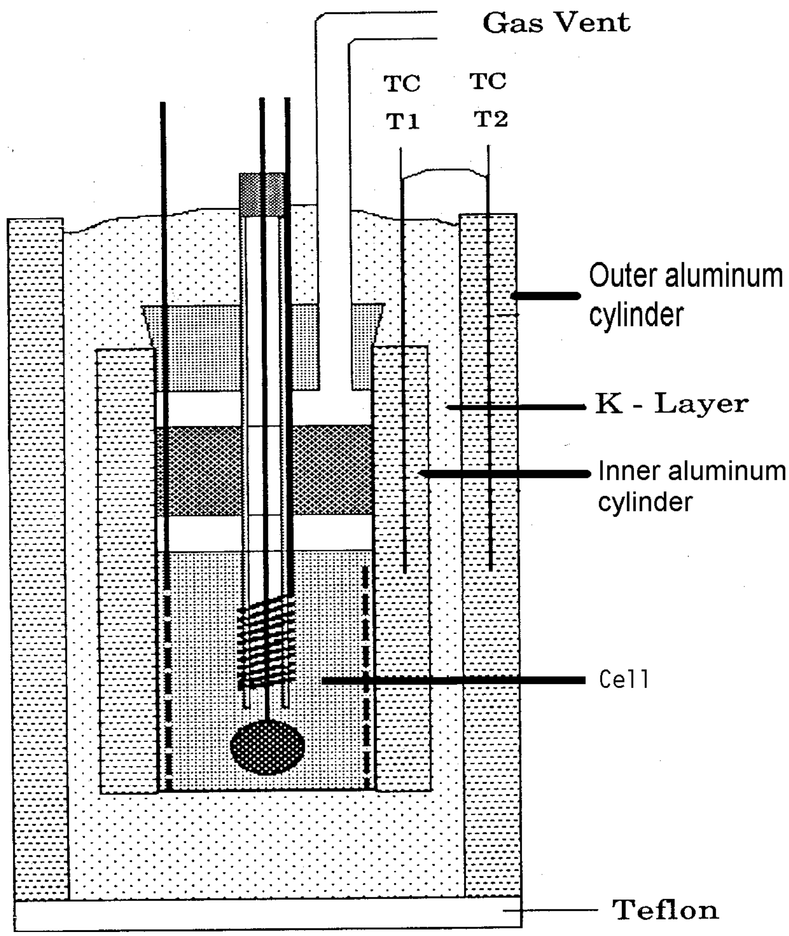

FIGURE 14.4 Huggins, Stanford University, used a calorimeter with two aluminum cylinders to carry heat away from the cell.

(T1). Then it passed through the K-Layer of alumina (aluminum oxide) to the outer cylinder of temperature T2. Between the two cylinders, heat flow followed Newton's law of heat conduction in which the rate is proportional to the temperature difference. (This was in contrast to the Fleischmann and Pons calorimeter in which the heat escapes by radiation.) Once again, a different kind of calorimeter was involved and one that required a different set of calibrations and calculations and, therefore, would not be subject to earlier error sources.

Heat transmission could be readily calibrated and measured with this design. The outward flow of heat power was calibrated using three different methods, and was precise to one half of one percent over the time and temperature range of the experiment. The resulting amount of energy conveyed from the cell to the outside air did not depend upon the distribution of temperature within the cell or upon a need to stir the cell.

Another unique characteristic of the Huggins experiment was the application of constant power, rather than constant current to the cell. This nice fillip was accomplished by continuous computer regulation of the electrical power source that operated the cell. The Huggins cell also treated the two gases in an unusual manner. Above the electrolytic liquid was located a recombiner catalyst (the dark grey area in Figure 14.4) consisting of a platinum mesh which causes the two gases to recombine into heavy water that dripped back down into the electrolyte solution. Inadvertent recombination of the gasses was thus not a concern.

Huggins's results are seen in the Figure 14.5. Note that the left scale is used for power (watts), temperature (C), and anomalous power (as a percent of input power).[13]

The tracing of solid diamond shaped points marks the input power of ten watts during the entire 120 minutes shown. There was a sudden temperature rise of about six degrees after about one hour as shown by the open squares. This rise was caused by a surge of anomalous power that is shown by the hollow diamond markers, and is read as percent on the left scale. The excess power reached 56% of the input power, or about 5.6 watts maximum.

The calibration was determined to be accurate to ±0.5% at power levels to 22 watts, cell temperatures to 60C, and over several weeks time.

While this data were ready for presentation at the first cold fusion conference the following spring (March 1990), it was too late for consideration by the DOE Panel that had finished its work the previous November.

M. H. Miles

Melvin H. Miles was a research scientist at the Naval Weapons Center, China Lake, California. His calorimeter also was different from those described pre-

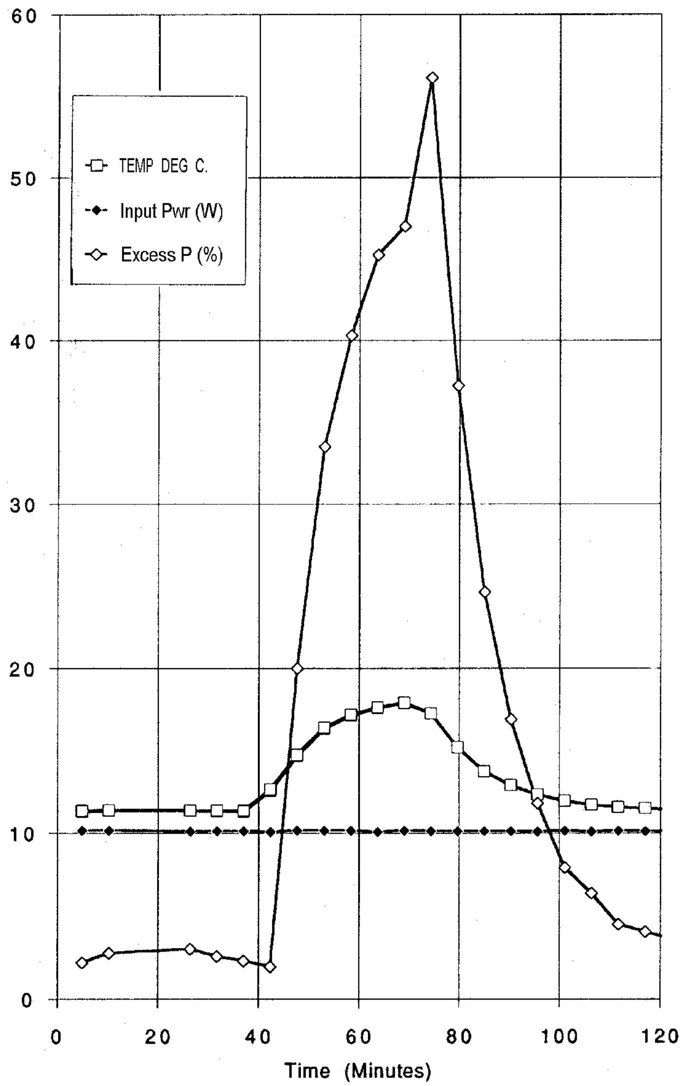

FIGURE 14.5 Huggins reported anomalous power generation. Its value is read from the left scale as power times 1/10 or as percent. The peak power is 5.6 watts or 56%.

viously.[14] An inner cylinder of water collects the cell's heat and releases it through an insulator to a surrounding water bath. The cell does not need either stirring or a thermometer. Temperature readings are taken from the inner cylinder of water and from the bath.

His first tests were disappointing, and when he was contacted by the DOE Panel he told them that he had not detected excess energy. He studied

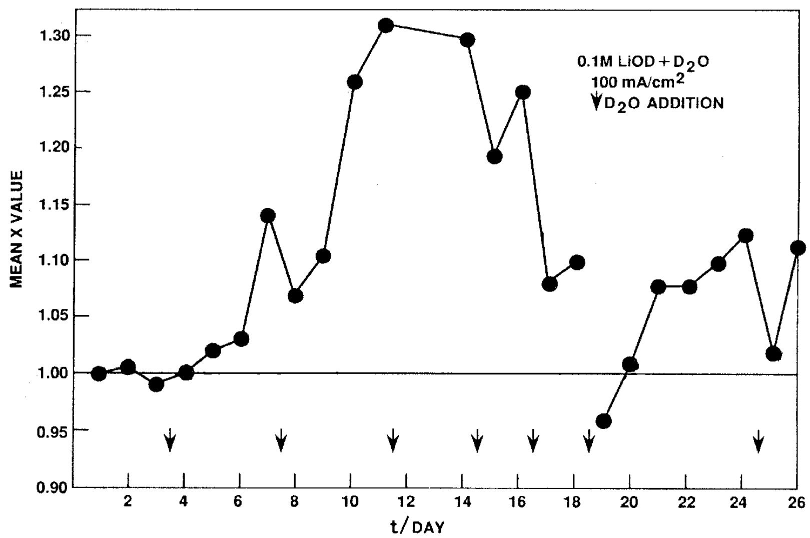

FIGURE 14.6 Miles, Naval Weapons Center, China Lake, CA, reported generation of anomalous power. "X" is excess power. It is expressed as output-power/input-power.

many different samples of palladium that had been processed in different ways in order to select the best candidate for a cathode. By the end of 1989, when he experienced some success in measuring excess power, he contacted the Panel to inform them of this change of fortune but his calls were not returned.

In 1991 Miles's cell design was conventional except that he sorted the palladium carefully. By doing so, he experienced a 50% rate of success in obtaining excess heat. Electrolysis lasted for 26 days. He checked for possible recombination of the gasses, and found that it did not happen to within an accuracy of 1%.

Figure 14.6 shows his results in the solid black dots. On day eleven, the power output is 30% greater than the power input. The estimated accuracy of this power reading is ±20 mw or ±1% of the input power, whichever is larger. Its average over 11 days was 14.5% excess power. The average excess power was 140 milliwatts, and the total excess energy was 110 kiloJoules. Miles stated that his excess power results for at least one of his runs was significant at the 99.95% confidence level.

Y. Arata

Possibly the most ingenious of experiments in this field was accomplished between 1991 and 1994 by Yoshiaki Arata and Yue-Chang Zhang, both at

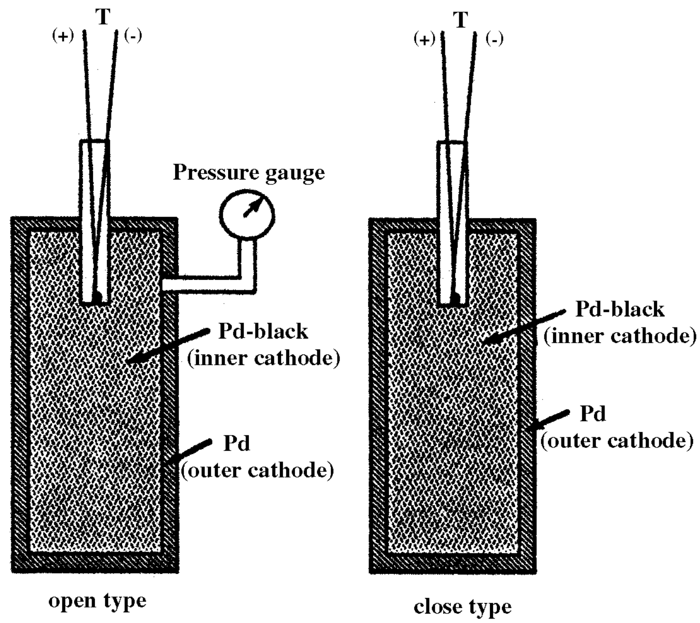

FIGURE 14.7 Arata, Osaka University, shows two versions of the the special double structured cathode he used. Its inside space holds palladium black, a powder form of palladium.

Osaka University, Osaka, Japan.[15] Much of Dr. Arata's experience was in high-power plasmas and lasers. He was one of the first to develop powerful CO2 lasers. Professor Arata is an Academician of the Japan Academy, Recipient of the Arthur Schawlow Award (1985), and recipient of the Emperor's Award (1997). A major building on the Osaka University campus is named in his honor. But their results in cold fusion research did not come quickly. They experienced repeated failure before learning how to generate excess power. Arata explained that "A long trial and error period of over two years was required before success was achieved."[16]

Arata configured his cell with a "double structured" cathode, two variations of which are shown in Figure 14.7. Its shell is made of palladium, and the inside holds palladium-black, an extremely fine powdered form of the metal. The powdered palladium* can be seen inside the cathode which is welded closed. About 3 to 5 grams of powder proved to be sufficient for the production of hundreds of megaJoules of heat.

The cathode has a temperature sensor, sometimes a pressure gage, and a connection to the negative terminal of the power source that runs the cell.

* The particle size is about 0.04 ±0.02 microns in diameter.

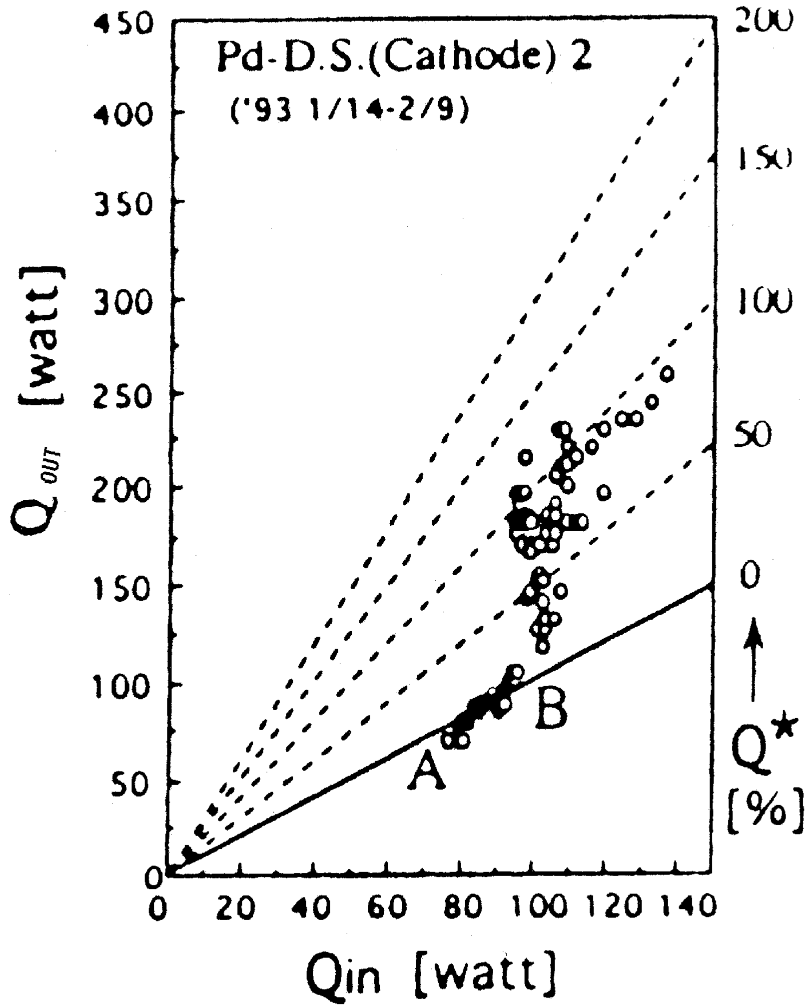

FIGURE 14.8 Arata shows power output as a function of input power. At 125 watts input power, there is almost 250 watts of output power. This calculates to 125 watts of power generated by the experiment.

Placed in a vacuum chamber and out-gassed, the cylinder is sealed by means of electron beam welding. Heavy water (and a lithium salt), and a platinum anode complete the cell's electrical circuit. Arata anticipated that the heat generating reaction would take place in the powdered palladium.

During electrolysis, deuterium (from the water) is deposited on the surface of the cathode, to take the form of single atoms or, possibly, single nuclei. These pass through the wall of the cylinder to enter the interior space free of contamination by other atomic elements. Only the three forms (isotopes) of hydrogen can pass freely through palladium. The palladium black absorbs the deuterium to a high concentration and generates excess heat. Arata was the first to show that palladium black exposed to deuterium gas was highly active.

Figure 14.8 shows the results of one of his experiments from the period 1992–1993. The interval A-B is the "incubation," period during which no excess heat is generated. That also may be considered a "control" period. The distribution of the dots shows the kind of signal obtained without excess heat ($Q^* = 0$).

After B, the power output increased from that zero line up to twice the input power level showing the generation of 100% excess heat power. It generated more then 200 megaJoules of anomalous energy over 3,000 hours at an average rate of 14 to 28 watts, and produced a maximum power generation of 125 watts, which lasted for several weeks.[17] From Figure 14.8, it can be seen that this set of data displays an excellent signal to noise ratio.

In a related experiment, two cells were connected electrically in series with one using heavy water and the other using light water. Excess heat was generated by the first and none by the second.[18] Arata considers the data from his hollow-cathode experiments to be 100% reproducible.

Electrochemistry

Planning of the experiments and the laboratory data collection and reduction that are presented in this chapter was accomplished by researchers who were chemists in all cases except for W. N. Hansen and Y. Arata. But Hansen had spent two years doing electrochemistry in a post-doctoral fellowship where he worked with Professor Heinz Gerischer, at the Technical University, Munich, Germany. Arata emphasizes that two years of trial and error were necessary for him. All of these scientists enjoyed considerable expertise in electrochemistry and calorimetry. These were the relevant areas of expertise for exploration of the Fleischmann and Pons phenomenon.

My own acceptance of the reality of anomalous power came from reading McKubre's many reports supplemented by a visit to his laboratory where I found him quite willing to show me whatever I asked to see and to answer at length the most critical of questions. The person, the laboratory, and the reports added to my present opinion that he was doing good science at SRI, and that his results should be given serious consideration.

Validation Summary

Our review of the most significant claim of the three Fleischmann and Pons claims of March 1989 is now complete. The original claims of nuclear product production were immature and, in at least one case, wrong. Although the claim of deuterium-deuterium fusion was exciting, even more exciting, as well as more important, was the claim for anomalous power along with its concomitant property—the lack of significant radiation. The scientific community, led by nuclear physicists, was diligent in evaluating the evidence for neutron particles but woefully lax when anomalous power needed evaluation. The chemists, acting as a professional community, or acting as individuals in the dress of independent referees, chose to play no role whatsoever in responding to the Utah claims. The most surprising aspect of the entire episode was that the scientific community concluded that the claims could be adjudicated in a mere five weeks.

There is no overlooking the fact that the 1989 evaluation can best be characterized as frenetic. The appalling turn that events took when *ad hominem* arguments were introduced along with the concomitant and indefensible shut down of communications with those working in the field constitutes an historic blemish on the American scientific community.

Though major American corporations were not doing cold fusion research, many were keeping a weather eye on its evolution. A senior engineering executive from a major international company gave a paper at the ICCF-5 conference in the spring of 1995. He reported the results of an informal survey carried out over the previous eighteen months. This overview included substantive visits to several laboratories. The following quote from that report expresses his view of the field: "The first [premise] is that the cold fusion effect in its various forms is real. There exists sufficient experimental evidence at this time that this issue no longer needs to be addressed. It is not justified to devote additional resources to demonstrate the existence of the effect."[19] The "effect" referred to here is anomalous power. This quotation parallels the conclusions reached by this writer at about the same time and constitutes a rational response to the state of the art of cold fusion research at that time.

These seven corroborations prior to the end of 1994 are presented as scientific validation of the announcement by Fleischmann and Pons of their observation of anomalous power in their electrolytic cell. In 1999, McKubre summarized the evidence for excess heat, basing his views on many years of cold fusion research.

> The evidence in my view for the appearance of an anomalous unaccounted excess heat in the deuterium-palladium system is essentially overwhelming. There is something there. It is larger by more than

SUMMATION

Validation by Independent Laboratories

After 1989, those willing to commit time to the field gradually brought forth increasingly well designed experiments. The seven cases described in some detail in this chapter plus two cases described in Chapters 1 and 4 offer confirmation of Fleischmann and Pons's claim to have observed anomalous power.

Dr. Edmund Storms collected reports of anomalous power corroboration in March 1995, at the end of the first six years of cold fusion research.[1] Each had been reported in a published technical paper—an unusual protocol in science where only the second occurrence was ordinarily needed to confirm a claim of discovery. (The listed decimal number is the reported excess power maximum value in watts.)

Researcher	*Year*	*Country*	*Max.power*
Aoki	(1994)[2]	Japan:	205.–
Appleby	(1990)[3]	USA:	0.049
Bertalot	(1991)[4]	Italy:	0.08
Bertalot	(1992)[5]	Italy:	3.–
Bush	(1991)[6]	USA:	6.0
Celani	(1994)[7]	Italy:	05.0
Fleischmann	(1990)[8]	Japan:	2.8
Gozzi	(1991)[9]	Italy:	12.8
Guruswamy	(1989)[10]	USA:	8.–
Hasegawa	(1992)[11]	Japan:	0.5
Hugo	(1994)[12]	USA:	23.–
Hutchinson	(1990)[13]	USA:	3.–
Kainthla	(1989)[14]	USA:	1.1
Lewis	(1990)[15]	Sweden:	1.0
Okamoto	(1993)[16]	Japan:	7.–
Ota	(1993)[17]	Japan:	11.3
Storms	(1992)[18]	USA:	7.5
Takahashi	(1992)[19]	Japan:	130.–
Yang	(1990)[20]	Taiwan:	12.9
Yun	(1991)[21]	Korea:	0.24
Zhang	(1990)[22]	China:	0.015

Experimental corroboration of anomalous power was now well advanced. The variety of the experiments made any attempt to refute these reports a daunting task as it must be done *without exception.* Such an undertaking would only be meaningful if presented as a full-length report published in a peer-reviewed journal. The preponderance of experimental evidence was now in support of anomalous power because of its successful replication in many independent laboratories.

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one order of magnitude, in some cases by more than two orders of magnitude, than the sum total of all possible chemical reactions.[20]

Our examination of excess heat claims during the years 1989 through 1994 is now complete. From this point forward our narrative will recognize the existence of a new natural phenomenon, anomalous power. The science of anomalous power will mature as the many fundamental and unknown aspects of it are gradually filled in over the years. These are obviously of an order that requires the best minds and facilities for an indefinite period of time. It is not reassuring that an understanding of the mechanism of superconduction remained out of reach for forty-six years, from 1911 to 1957.

These first fourteen chapters have tried to explain and witness *the genesis and development of a scientific fact:* the anomalous power phenomenon that is exhibited in the Fleischmann and Pons's experiment.

CHAPTER FIFTEEN

Posthumous Heat

The fourteen preceding chapters are dedicated to the empirical evidence for a field of science called cold fusion studies, and how it emerged and developed during the years 1984 through 1994. With that purpose behind us, the story from this point can range more broadly. This chapter and Part Four following it, tell of the further aggregation of empirical evidence for excess heat and the unrecognized or unknown nuclear reaction that powers it.

First, a number of different types of electrolytic cells will be briefly reviewed. Some experiments that follow operate at high levels of power. Finally, there is the emergence in several laboratories of what I call posthumous heat, heat generated after the electrolytic cell current is extinguished.

There were many types of electrolytic cells tried in the course of the first ten years—-almost as many as there were research groups working in the field. My account of the history so far has carefully limited itself to the Fleischmann and Pons cell. It is now time to look at other designs.

A number of laboratories have claimed to detect excess energy in cells using light water for the electrolyte and using nickel rather than palladium for the cathode. The status of this sub-field is summarized in a 1995 survey by Dr. Edmund Storms: "Although most studies (with a nickel electrode) used primitive calorimetric methods and open cells, proof-of-principle evidence for excess energy production is sufficiently strong to warrant further study."

Robert T. Bush and Robert Eagleton, both professors of physics, California State Polytechnic University, Pomona, California, have been working with cold fusion electrolytic cells since the summer of 1989. With them, the generation of excess heat is a routine accomplishment of many years standing.[1]

They use a potassium salt to make the light water into an electrolyte, and, for a cathode, they use varieties of nickel, such as foil, screen, and mesh. They achieve power gains of up to 1.40.

The purpose of their laboratory work is in support of R. Bush's theoretical studies looking towards the source of the power

Mitchell R. Swartz, JET Energy Technology, Inc., Wellesley Hills, MA, and MIT, has measured excess heat in nickel/light water electrolytic systems for several years. One reported experiment in 1998, produced a power gain of 1.38 at its optimum operating point.[2] He finds that excess heat does not occur with cathodes of iron, aluminum, or platinum.

Swartz shows that nickel/light water systems have an optimal operating point for excess heat generation. Increasing the cell current beyond that point causes a falloff in power generation as does lowering the current. By operating more optimally, the cell gives a more reproducible excess heat phenomenon. He suggests that the failures to produce heat may be due to operation far outside the optimal operating range.

Most people are aware that electric current consists of the movement of electrons in metals. A less well known type of electric circuit uses protons moving in ceramic materials. Dr. T. Mizuno, Hokkaido University, and Dr. Richard A. Oriani, University of Minnesota, have applied varying electrical voltages to ceramic materials under the conditions of a high temperature (250C) in a deuterium gas atmosphere.[3] Mizuno prepared the ceramic samples* and supplied them to Oriani.[4]

The ceramic obtained its needed protons by absorption of deuterons from the deuterium gas. The deuterium nucleus, a positively charged deuteron, may move through the ceramic from the positive to the negative terminals as an electrical current. Both scientists claimed a measurement of excess heat. The (generated) excess power measured is from ten to one hundred times greater than the excitation power applied to the ceramic. Oriani also found that the ceramic electrode in deuterium gas operating at a high temperature spontaneously generated anomalous power, without concurrent electrical excitation. That the electrical excitation was not needed was in keeping with other cell designs using gaseous deuterium, and with the possibility that in liquid cells the electrical current is used to load deuterium into the palladium rather than to participate necessarily in the heat generating reaction.

L. C. Case

Leslie C. Case, Sc.D. MIT, has been practicing and teaching chemical engineering for many decades. Much of his professional work involved the use of

* The preferred ceramics were of a perovskite type, $SrCe_{0.9}Y_{0.08}Nb_{0.02}O_{2.97}$.

catalysts to augment industrial chemical reactions. He became attentive to the field of cold fusion studies when the episode broke open and was much influenced in 1993 by the Yamaguchi experiment where helium and an enormous amount of heat were reported in a vacuum bell jar[5] by exposure of palladium to deuterium gas. In his basement laboratory at Newfields, New Hampshire, he explored by trial and error the available commercial catalysts for a possible reaction with deuterium gas, and found a variety of promising candidates.

Case traveled about central (formerly Eastern) Europe, until he found that the nuclear laboratory at Charles University, Prague, was willing to work with him to make the necessary nuclear measurements for his experiments. He was granted an international patent in November 1997 for his experimental device. He authored an article at the ICCF-7 in Vancouver in April 1998 to explain what he had been up to for so many years.[6] The professional scientist who follows the given reference will be disappointed in what he finds. Case is not functioning as a scientist in this endeavor, but as a development engineer, which is what he is professionally. This referenced paper, which is all we have of his solo work, is only a program manager's progress report; it is not a scientific paper.

Case's experiment is quite simple. It consists of a metal canister of about one liter capacity. In it he places a few 10s of grams of the selected catalyst and, after cleaning it with several fills of hydrogen (H_2), finally fills it with deuterium [D_2] gas at about three atmospheres pressure. It is heated to about 200C by means of an external electrical heater. Immediately after reaching temperature the catalyst activates itself to begin the reaction process in which additional heat is generated raising the temperature an additional ten or twenty degrees.

Case has explored the reactions of a broad array of commercial catalysts when exposed to deuterium gas at modest temperatures and pressures. Most exhibit no reaction. Those that do have a reaction, offer a range of parameters much narrower than is the case with chemical catalytic reactions. The generally successful catalyst is metallic and is supported on a "fluffy" carbon substrate. He reports that the platinum series of precious metals work: platinum (Pt), iridium (Ir), rhodium (Rh), and palladium (Pd). Ruthenium (Ru) has not yet been tried. Of these, palladium seems to work best. It is present as minute particles on the carbon support at about 0.4% by weight for best activity. But the performance is sensitive to the amount of catalyst used; too much or too little squelches the reaction.

In his laboratory practice, Case applies the same amount of heat to an identical (second) flask that is loaded only with hydrogen (H_2). He notes that the temperature of the deuterium loaded canister moves higher than that of the second flask to indicate the generation of excess heat when deuterium is used. The level of generated anomalous power is estimated at 5 to 10 watts.

Reproducibility is excellent at a level of about one success out of two attempts. We will look at this experiment again in the next chapter for its nuclear effects.

Most disciplines in science recognize that an experiment is validated by reproducing it in another laboratory and involving other technicians. McKubre, at his laboratory in Menlo Park, California, replicated the Arata experiment from Osaka University, and the Case experiment from Newfields, New Hampshire. The staff were in frequent consultation with both of the original researchers throughout the experimental period, but all the work done in McKubre's laboratory was done by his people, Arata and Case not being present during that period.

Arata's results (Chapter 14) proved sufficiently robust that his experiment was selected for reproduction at SRI in 1998. He supplied two of his specially designed cathodes and they were subjected to electrolysis, one in heavy and the other in light water. For these instances, abundant excess heat was generated with heavy water, and no excess heat was generated with normal water.[7] In particular, McKubre obtained 64 MJ (17.8 kiloWatt-hours) of excess heat from the cathode provided by Arata when it was electrolyzed in D_2O. Our imagined 1,500 watt stove burner would run cherry red for eleven hours on this amount of energy. The Arata experiment thus appears to be reproducible in other laboratories.

McKubre duplicated the L. C. Case experiment using stainless steel flasks of about one liter capacity.[8] To two of these were added the carbon supported palladium catalyst. One was filled with hydrogen ($^1H_2$) gas and the other with deuterium ($^2D_2$) gas. They were heated to between 170 and 250 C and were filled with gas at from one to three atmospheres (15 to 45 lbs/in^2) of pressure. Calorimetric measurements showed that the flask filled with deuterium gas generated excess heat and the one filled with hydrogen gas did not.

The Case experiment to generate excess heat has been successfully operated at Charles University, Prague, and at the Cold Fusion Technology Laboratory, Bow, New Hampshire, as well as at SRI International, Menlo Park, California.

Higher Power and Temperatures

It is important to look at the high power levels a few experiments have achieved. The part played by calorimeter errors changes dramatically as the power level of operation moves higher. That is, the large temperature excursions due to high levels of anomalous power were more easily measured. The fact that excess heat was claimed at these higher levels was an important source of corroboration for the reports that were presented in the previous chapter.

There we have seen that Arata reached excess heat output levels of 125 watts in the experiment of Figure 14.8.

A principal task of Fleischmann and Pons at their laboratory in southern France (1992–1995) was to achieve higher excess power levels than had been seen earlier. They reported in 1993 on an experiment that ran for four weeks in which the temperature was driven to boiling and the electrolyte was boiled away in the last few minutes. They reported power levels, at temperatures near the boiling point of water, of 140 watts excess sustained for several hours. The power density in the cathode under these conditions was 3,700 watts per cubic cm. of the palladium electrode,[9] a power density greater than that experienced in the fuel rods of a nuclear reactor. The generated power was reported as four times greater than the input power.

Replication of this experiment was begun by Dr. G. Lonchampt at the French Atomic Energy Commission facility at Grenoble, France, after Fleischmann and Pons published their results. Lonchampt published his initial report in 1996 saying "We confirm the results published by Fleischmann and Pons, more particularly in the boiling regime."[10] This paper was primarily a confirmation of the heat measurement techniques. They affirmed the correct performance of the calorimeter at the boiling temperature of water.

Successful accomplishment of this limited confirmation required two years of scientific effort in a national laboratory and frequent consultation with Dr. Pons, who provided the cathode.

Dr. Pons reported a 1996 experiment that operated at high power levels with a new type of cell design, one that operated continuously with the electrolyte liquid at the boiling point.[11] The cell was a cylinder insulated to limit heat loss. It had a lower section where the boiling takes place, and an upper section that acts as a reflux condenser, where the steam condenses into water and falls down into the lower section to be boiled again. One experiment that ran for 158 days generated 294 megaJoules of energy. A power level of 100 watts excess was maintained for 32 days, and produced enough energy to run our hypothetical stove burner (1,500 watts) for twenty-four hours.

One of the most enticing revelations in cold fusion research came from the evidence for bursts of energy. Sources of possible error change radically for a burst. Wilson, for example, mentions that their criticisms had little effect on values associated with bursts.

The existence of bursts was first mentioned by Fleischmann and Pons at the May 8, 1989, meeting of the Electrochemical Society in Los Angeles. Several other researchers had introduced data showing power bursts at the Santa Fe meeting held at the end of May, 1989. The DOE Panel virtually ignored the topic.

In a preprint, "Our Calorimetric Measurements of the Pd/D System" (March 1990), Fleischmann and Pons gave a quantitative measurement to a burst phenomenon.[12] The particular burst they studied continued for 19 days during which it generated 2.5 MJ. This amount can also be stated as 16 megaJoules for each cubic centimeter of cathode rod. The average power level during the 19 days was 1.5 watts.

Heat After Death

In 1992, Fleischmann and Pons experienced a new aspect of their cell's behavior. Instead of replenishing the electrolyte level, they let the cell run dry. The electrical current passing through it went to zero, of course, because the circuit was broken (opened). One would expect the cell to become quiescent and the generation of heat to become zero. With the use of ordinary light water, that was precisely what happened. But when the electrolyte was made with heavy water (D_2O) then an interesting phenomenon took place as is shown in Figure 15.1. It can be seen that the cells remained at an high temperature for three hours after the cell boiled dry.[13] The Kel-F plastic support in the base of the cell had partially melted, indicating temperatures above 300C. They refer to this effect as "heat-after-death."

Fleischmann and Pons's measurement techniques throughout the experiment give assurance that there was no storage of energy during the course of the experiment to provide the heat-after-death energy release. The accuracy of their calorimetric measurements was estimated to be about 2 to 3% at the cell dry point. Apparently, within the Fleischmann and Pons cell there is a mode of operation that permits the generation of excess heat without the need for a source of electrochemical excitation. I refer to this mode as posthumous heat.

Tadahiko Mizuno, Dr. Engineering and Professor, Department of Nuclear System Engineering, Hokkaido University, Japan, spent eight months building a closed electrolytic cell that had 1 cm thick stainless steel shell, a 1 cm thick Teflon lining, and was a little larger than a pineapple. It was designed to sustain several hundred atmospheres of pressure at 150C. He used a large cathode that was one cm in diameter and 10.5 cm long to be operated at 6 Amperes. The electrolyte was LiOD in heavy water of exceptional purity. The cell's operating cycle would be three weeks of electrolyzing and six weeks rest, repeated twice.[14]

The experimental run of interest started on March 24, 1991, a Sunday, when Mizuno brought the cell up to 75C by turning on an external heater coil that used 60 watts from a stabilized power supply until turned off. The electrolysis current provided 6 Amperes at 4 volts, or 24 watts, to raise the temperature further to 100C. In three days the pressure stabilized at seven atmospheres indicating a cathode loading of 0.95 D/Pd. The action of interest started two weeks later when, without intervention, the temperature slowly

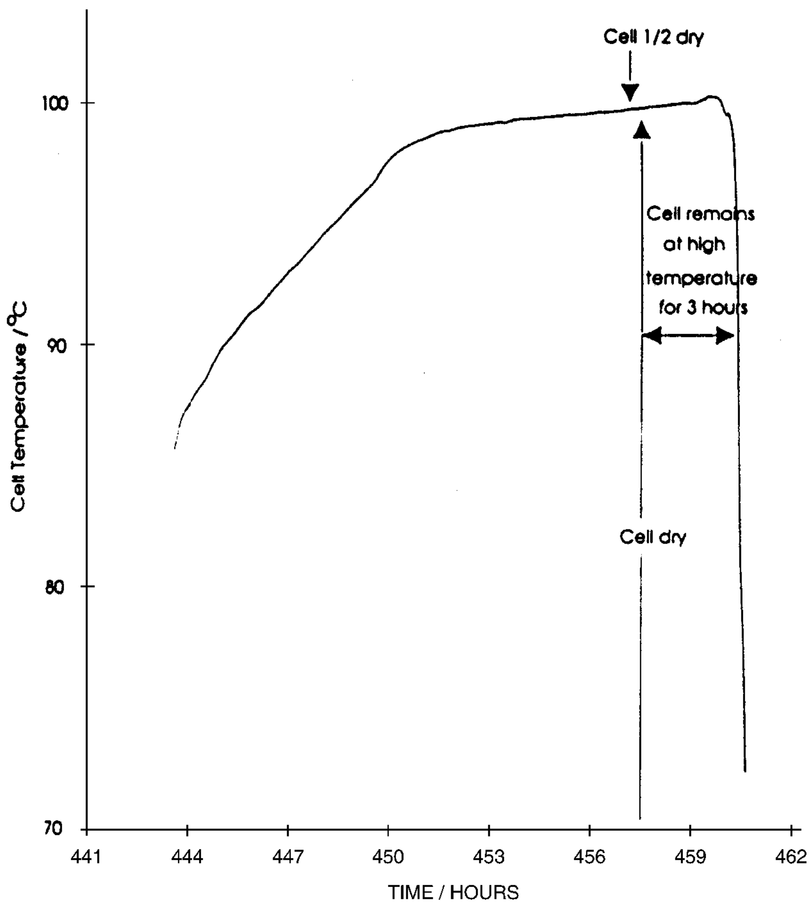


FIGURE 15.1 Fleischmann and Pons let this cell boil dry thus interrupting the current. It continued to generate heat for three more hours. They refer to this effect as "heat after death."

rose from its quiescent value of 100C to between 105 and 110. On April 22, a Monday, he turned off the electrolysis current leaving only the external heater coil running. Three days later, on Thursday the 25th, he saw that the temperature, rather than subsiding to 75C where the heater would hold it, it was at 90C and soon rose to 100C.

An assistant said, "Maybe this is the cold fusion effect that everyone is talking about." "It can't be," Mizuno replied, "the electrolysis current has been turned off for three days. Even cold fusion doesn't do that." The cell was exhibiting the behavior we call posthumous heat.

Mizuno turned off the external heater, but the next day, Friday, the temperature had not dropped. He put the cell in a bucket of water, and after an hour its temperature had dropped to 60C. On Saturday, he came in to check the cell and found the water had evaporated, the bucket empty, and the temperature up to 80C. He found a larger bucket and put 15 liters of water in it so as to completely submerge the cell. He checked three days later, on April 30, to find that this water had evaporated too.

Mizuno refilled the bucket with another 15 liters, and on each of the next two days he added 5 liters to it. Four days later, on May 7, the water was half gone and the temperature subsided to 35C. He calculates that from April 30 to May 7 the cell evaporated water to the tune of 8.2×10^7 Joules. That energy would keep our 1500 watt stove burner running on high for 15 hours.

This example of Mizuno's is the only occasion in this book where we have presented a limited type of experiment. The data was not sufficiently well documented to be published in a journal.

Giuliano Mengoli, Instituto di Polarografia, CNR, IPELP, Padova, Italy, by operating his cells at 95C, responded to an earlier Fleischmann note that higher temperatures facilitate the onset of anomalous power generation. He operated the cell and its bath at that temperature initially to enable cell temperature excursions above 95C allowing a measure of excess heat generation.[15] His design was similar to Fleischmann's, it being of similar size with a Dewar cell and palladium sheet cathode in a heavy water electrolyte. One difference was that Dr. Mengoli used an external source of gas bubbling through the cell to assure adequate mixing when the current was set at values much lower than those used by Fleischmann and Pons.

Figure 15.2 shows, partially, the result of one such run in 1995. The figure is labeled in watts of excess heat and in minutes from the point at which, after five days of electrolysis, the current was reduced to 1.5 mA/cm^2. After about 45 minutes the current was switched off. The amount of generated (excess) heat then increased to a level of 0.82 watts, about double its earlier value. The cell continued at that power level for 3.3 hours as shown in the figure, and for an additional 24 hours that are not shown. During these 27.3 hours, there was no electrical excitation applied to the cell. Furthermore, the excess energy generation stopped only because the experiment was shut down by turning off the thermostatic bath and letting it assume room temperature.

Dr. Mengoli reports one run in which the excess heat after current cut-off continued without cell excitation for 150 hours.[16]

Dr. M. Miles, China Lake, CA, received an appointment to the New Hydrogen Energy (NHE) Laboratory, Sapporo, Japan, where he performed an experiment that ran for 70 days, from December 1997 to February 1998. The cathode for his cell was an alloy of 0.5% boron in palladium made at the Naval Research Laboratory, Washington, DC, especially for this purpose. The

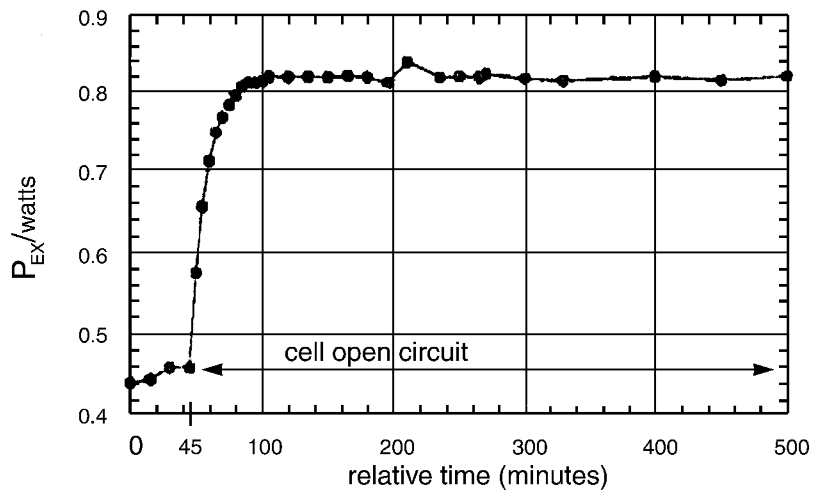


FIGURE 15.2 Mengoli observed his cell, operating at 95C, to continue to generate heat for 27 hours after the current circuit was interrupted.

data acquired during the run was thoroughly evaluated in a report published by the NRL[17] from which this summary is prepared.

The cell was allowed to run dry on day 69 to produce, afterwards, the phenomenon of heat-after-death (or posthumous heat) that lasted for approximately one hour during which time the excess heat being generated by the cell increased from 1 watt to approximately nine watts.[18] Fleischmann and Pons, Mizuno, Mengoli, and Miles all obtained a substantial increase of power after excitation was turned-off. Oriani found that excess heat could be generated from the start without electrical excitation.

This display of posthumous heat enables a more intuitive appreciation of the Fleischmann and Pons phenomena. *No longer is it necessary to subtract the input from the output power to determine the amount of excess heat.* The measured heat is all excess heat.

With this chapter, we end our devotion to anomalous power, the principle presenting symptom of an unknown nuclear reaction in solid matter. From the beginning of this episode, some scientists, correctly convinced that the excess heat announced by Fleischmann and Pons in March 1989, would prove true, started to search for the nuclear products that must be produced by that reaction, no matter what the nature of that reaction might be. Part Four is committed to that purpose.

Part Four

LOW-ENERGY NUCLEAR REACTIONS:

NUCLEAR PRODUCTS

CHAPTER SIXTEEN

Helium-Four

The object of the series of experiments presented in this chapter is to measure the correspondence between the amount of energy generated in a cell and the consequent production of nuclear products, or ash, and in particular, helium as isotope four (He-4, or ^{4}He).

Nothing in this book denigrates the need to search for the energy source of the anomalous power phenomenon. Clearly, if a nuclear reaction generates anomalous power, then some nuclear ash must form. Nuclear reactions occur in discrete increments: for example, when two deuterium atoms merge into one new atom, they form helium-four.* The number of atoms of helium created during each second of time should be proportional to the amount of excess heat power being measured in the cell.

Park, Close, and Huizenga mistakenly demand that such evidence is necessary to affirm the Utah claims for anomalous power. Why, then, does the topic get postponed to this later chapter, it being such a basic part of our interest? It is because inappropriate motives fed the demands of the skeptics who wanted evidence for nuclear ash. They were angry at the two trespassers: Are establishment physicists to be taught potentially new physics from the likes of two chemists from a place called Utah? Also, the skeptics much wanted to avoid the need to develop a working knowledge of electrochemistry and calorimetry.

The skeptics also argued that if the claims were true three miracles were required while, at the same time, they refused to enter the chemistry labora-

* The various forms for writing the isotope name are used for stylistic variation: helium-four, He-4, ^{4}He, $^4_2$He.

tory. They acted towards Fleischmann and Pons like an only child when the new baby arrived. Their assertion of the need for miracles was only another put-down of the new field of study, as was their use of the word "believers." Such statements were effective in distracting the professional chemist or physicist from his duty to properly evaluate the anomalous power measurements. Was the demand for evidence of nuclear products a means to evaluate the heat claims? No, the heat claims would have to sustain themselves under thorough examination. The expert in the calorimetry of electrochemistry experiments must find no procedural error in the detection and measurement of the heat flow.

The search for nuclear products, however, was the science that followed indirectly from the claim. It was the science whose fulfilled purpose would bring increased understanding to a well-measured observation. We report in this chapter on the evidence for helium-four, and in the next chapter for tritium and helium-three. I recommend to the interested scientist the 1995 book by Nate Hoffman, *A Dialog on Chemically Induced Nuclear Effects,* as an extensive text on the range of nuclear evidence and activity for the years 1989 through 1993.[1]

Those scientists who settled into early cold fusion studies explored the cell's presenting symptom of anomalous power and the difficulties experienced in its replication. Other scientists, however, moved directly to the search for the source of the energy. Some took the more conservative path and looked for nuclear effects only during the production of heat, and others tried to create nuclear effects more directly, i.e., by temperature cycling of deuterium loaded titanium.

The practitioners of this nuclear research found themselves trying to do in a few years the research that might well occupy a generation. Their purposes were noble: they were trying to release the field from its bound of ghetto walls. Their efforts proved marginal during the first six years.[2] Nevertheless, the search for the origin of the heat energy accomplished some good science during those years. Even here, only a glimpse of what has been done can be offered.

The search for products (nuclear ash) of an unknown process constitutes a herculean task, but a search for helium (He) could be more focused.* In particular, the reaction

$$\text{D} + \text{D} \rightarrow \text{Helium-four} + 23.8 \text{ MeV (as lattice heat)}$$

conjectures a fusion reaction that is without neutron or gamma radiation. As Huizenga points out, "One couldn't wish for a more attractive source of fusion

* Helium is second in the table of elements, placed after hydrogen. It has two electrons orbiting the nucleus and two protons in the nucleus. Helium-three has one neutron and helium-four has two neutrons in the nucleus in addition to the protons.

energy, free from the normal much more copious sources of neutrons and tritium."[3] The enormous amount of energy emitted by each fusion occurrence of this sort would provide heat as the primary signature of the experiment. In fact, if Fleischmann and Pons had looked for radiation instead of heat, they most likely would have missed their claim of discovery. Clearly, if this were to prove the correct reaction pathway, then anomalous power is the presenting symptom of the phenomena.

First, a note about helium that we will need in this chapter. Air contains a 0.00000522 fraction (5.22 ppm) of helium-four which is always available to contaminate an experiment. Helium-four (^4He) has a mass of 4.0026 atomic mass units (AMU),* but deuterium molecules, D_2 (two atoms of deuterium), have a weight of 4.0282 (AMU). These close values present a demanding separation requirement for the instrumentation. A high-resolution (quadrupole) mass spectrometer is used to distinguish between the two values.†

Stanley Pons claimed to recognize that his cells were generating helium-four (^4He) as early as December 1988, a position based on his own limited experiments.[4] Pons and Hawkins had earlier informed two of their associates in the chemistry department that they had used a mass spectrometer to analyze the gasses and found in them substantial amounts of helium-four. On Monday, April 17, 1989, he held a press conference to announce the measurement of helium in the off-gasses from a cell that was generating 0.5 watts of excess heat. Pons presented this as further evidence of a nuclear source for the excess heat. Pons planned to make a scientific presentation of his limited helium data at the Los Angeles meeting of the Electrochemical Society on May 8, 1989. The hostility generated the previous week in Baltimore, however, precluded any such casual sharing of information.

In the meantime, N. S. Lewis, at Caltech, had called to interrogate him as to the exact procedures used. Lewis used an identical model spectrometer to replicate the measurement and concluded that Pons had measured, not helium from his cell, but helium from the air. It seems that the amount of helium generated by 0.5 watts is not sufficient according to the above formula to be measured by the particular spectrometer they both were using.[5] Shortly thereafter Fleischmann and Pons retracted their claim of helium measurement.[6] Pons never turned his attention again to finding gas-entrained helium.

Out of the Los Angeles meeting came a promise by Pons to check for helium embedded in the body of the palladium cathode, a search which also arrived at a null conclusion.‡ Pons provided electrolyzed cathode samples and

* Atomic mass units (amu) with hydrogen as 1 amu is a measure of the mass of a proton.

† In 1992, a quadrupole-mass spectrometer (ULVACIII-RESOM 2SM) became commercially available. It was effective in providing sufficient resolution to separate helium-four from deuterium (both are gasses) in the effluent gas flowing out of cells that are generating anomalous power.

‡ This episode of the cathodes analyzed by Johnson-Mathey is reviewed in Chapter 8, p. 105.

TABLE 16.1 Summary of Round-Robin Helium-Four Analysis[a]

Laboratory	As Received ($\times 10^{13}$ atoms)	Electrolyzed ($\times 10^{13}$ atoms)	Factor Increase
#1	2.0	6.3	3.15
#2	1.25	7.0	5.60
#3	0.31	2.4	7.74
#4	0.37 & 0.35	1.5	4.32 & 4.57
#5	0.84	4.6 & 8.3	5.48 & 9.88

a. Worledge, David H., "Technical Status of Cold Fusion Results," (The First Annual Conference on Cold Fusion, March 1990, National Cold Fusion Institute, SLC, Utah), p. 252, Table 4.

Morrey, John R., Marc W. Caffee, N. J. Hoffman, B. M. Oliver, et al., "Measurements of Helium in Electrolyzed Palladium," (*Fusion Technology,* vol. 18, December 1990), p. 659.

samples as received from the supplier to EPRI for evaluation. EPRI forwarded the samples to ETEC/Rockwell in a double-blind test. ETEC/Rockwell distributed the samples to the laboratories and collected their analysis reports.

All the laboratories found an increase factor of 3 to 10 in the amount of helium-four in the cathodes after they were electrolyzed. It is further reported that this amount of helium corresponds approximately to the small amount of anomalous power reported for the electrolyzed cathodes. The results are shown in Table 16.1. The considerable helium-four present in the "as received" material prevents further refinement of this interpretation of the data.

There are in chemistry many different kinds of electrolytes. Professor Bor Yann Liaw, et al., of the University of Hawaii experimented in 1990 with molten salt electrolytes that were operated at about 400C. They had previously reported excess heat seven times greater than the input excitation power. In two active experiments, palladium electrodes were embedded in deuterium saturated electrolytes.* Their corresponding control experiments used hydrogen in place of deuterium. After electrolysis the experiments were analyzed for their helium-three and helium-four content.[7]

The amount of helium-three in all active and control samples remained almost constant throughout the analysis. This implied that those known nuclear reactions that generate helium-three or tritium were not active in this experiment or were active below the sensitivity level of the instrumentation.

Slightly enriched helium-four in the deuterium saturated sample was detected from all four specimens, of which one was fourteen standard deviations above the background noise level in the measuring spectrometer, while those saturated with hydrogen showed an opposite effect. On the basis of this data, the enrichment of the palladium with helium-four was recognized as an anomalous event—it was without scientific explanation.

* The electrolyte was a LiCl-KCl mixture saturated with with LiD.

Liaw postulates that most of the generated helium-four escaped with the effluent deuterium gas. The suggestion is also offered that there is a remote possibility of atmospheric contamination in this experiment based upon an analysis of the experimental conditions.

A team of graduate students, led by Dr. Bockris in the Chemistry Department, Texas A&M University, electrolyzed a palladium cathode for three weeks in the Autumn 1991. The run was deliberately interrupted when measurements showed it was generating tritium, and the cathode was rapidly (within one second) removed from the cell and immersed in liquid nitrogen to hold the cathode's chemical condition unchanged for analysis. Later, the cathode was quickly cut up into small pieces, packed in dry ice, and sent to Rockwell International for analysis of its contents. In Table 16.2, the series of pieces labeled "a" and "c" were cut from the surface of the cathode. The "b" samples were from cuts made away from the surface, from the core of the sample, without any of the near surface material.

Table 16.2 shows the result of the analysis above a background value of 0.5 billion helium-four atoms measured during the mass spectrometer's calibration procedure using non-electrolyzed samples of palladium. This value is subtracted from the actual measurements on each of the six samples.

Excess helium-four was observed in nine out of ten samples from electrodes that produced tritium. No helium-four was observed above background in the non-electrolyzed palladium (not shown) from the same virgin stock or in the platinum anode material.

This early work was inconclusive, but it is not without merit. It inspired other scientists to search for helium products that might result from the generation of excess heat. These products or "ash" might be found either in the effluent gasses or in the body of the palladium cathode.

TABLE 16.2 Helium-4 in Electrolyzed Palladium Cathodes[a]

Sample	Sample: mass/mg	^{4}He/10^9 atoms
5a-1	30.92	3.8 ± 0.3
5a-2	39.70	166.8 ± 3.3
5a-3	42.37	3.4 ± 0.3
5a-4	20.22	2.1 ± 0.6
1b-1	27.79	1.9 ± 0.3
1b-2	30.01	2.5 ± 0.3
1b-3	23.85	0.4 ± 0.3
1b-4	33.05	1.7 ± 0.6
5c-1	44.63	1.9 ± 0.5
5c-2	30.40	−0.1 ± 0.5

Note: the average background value is 0.5×10^9 ^{4}He atoms.

a. Bockris, John O'M., et al., "On an Electrode Producing Massive Quantities of Tritium and Helium," (*Journal of Electroanalytical Chemistry,* vol. 338, 1992), pp. 189–212.

Heat Correlated Helium-Four

Dr. Benjamin F. Bush, Department of Chemistry, University of Texas, Austin, Texas, applied in 1990 for a postdoctoral position at the China Lake Naval Weapons Center. This led to conversations with Melvin H. Miles (at China Lake) regarding cold fusion type experiments. A collaborative effort was initiated in the fall of 1990 to accomplish the first formally structured experiment for the detection of helium in the effluent gasses of a heat generating cell.

Benjamin Frederick Bush grew up in Sacramento, California, and attended the University of California at Berkeley. "At Berkeley I took every laboratory course possible. While the process of understanding was itself of interest to me, I strove to master laboratory practice. I wanted to participate in the adventure of the laboratory." Bush received a B.S. degree in chemistry in 1981 from Berkeley.

From there, he went to the University of Texas at Austin where he received a Ph.D. degree in inorganic chemistry in 1988, and continued there with post-doctoral work afterwards. He described his reaction to the Utah announcement.

> The cold fusion announcement came within the next year and I was hooked on its possibilities. I made contact with Dr. M. H. Miles at the Naval Weapons Center, China Lake, CA, during the summer of 1990 and discussions of collaboration began soon thereafter. By this time Miles was successful at obtaining excess heat. He would run the electrolysis, conduct the calorimetry, and collect the off-gasses. I would provide the flasks ready to accept the gasses and arrange for the mass spectrometer analysis of the resulting gas samples at the University of Texas.[8]

After working successfully in tandem with the laboratory at China Lake, he accepted a one year post-doctoral appointment there, later extended for a second year, and joined it in February 1991, where he developed the all stainless steel gas handling system there that eventually produced fine data.

In the March of 1993, his two-year appointment at China Lake ended, he moved to SRI International, Menlo Park, California, with a temporary, post-doctoral appointment to work with Dr. McKubre.* At SRI, Bush set up

* Bush was invited to reproduce his heat vs. helium experiment under EPRI sponsorship. He participated in a program intended to encourage and allow visiting scientists to reproduce results at SRI, and with SRI supervision, experiments successfully performed elsewhere. In some cases, and this was the case with Dr. Bush, these scientists were paid a stipend to support living expenses. In all cases, these appointments were of defined and limited duration to allow other scientists to participate in the program.

the Seebeck calorimeter and all-metal, off-gas sampling system he had been mastering. The first set of three data points showed helium-four generation from heat generating cells at a rate commensurate with the release of energy from deuterium-deuterium fusion.

Bush moved back to the chemistry department at Austin in early 1994 where he got his heat generating and gas collection instrumentation working in about two years. He began getting new data in 1998 that supported the earlier work by Bush and Miles at China Lake to demonstrate helium-four entrained in the effluent gasses as the nuclear product of the Fleischmann and Pons phenomena.

Melvin H. Miles was born in the small town of St. George, Utah, and was raised as a member of the Mormon Church. He attended a two-year community college, Dixie College, and graduated in 1957 as valedictorian of his class. In a manner customary in those parts, he served as a church missionary for two and one-half years in northern Germany. From there he went directly to Brigham Young University and graduated in 1962, with a B.A. in chemistry and a minor in mathematics. He did graduate work at the University of Utah and received his Ph.D. in physical chemistry in 1966 with a minor in physics. He won a one-year NATO post-doctoral fellowship to work at the Technical University at Munich in electrochemical kinetics under Professor Heinz Gerischer, a preeminent electrochemist.[9]

He joined the technical staff at the Navy laboratory in Corona, California, in January 1967, but when that was closed, he left the Navy to teach at Middle Tennessee State University for nine years. He joined the Naval Weapons Center laboratory at China Lake, California, in 1978 to work there on electrochemical programs such as thermal batteries for missile applications.

When the announcement emerged from the University of Utah, Miles was engaged in using the hydrogen in palladium system for reference electrodes in electrolytic systems. From there, he was able to move quickly into cold fusion experimentation on a part-time basis. But he found that the experiment was not an easy one, even for someone with his experience with electrochemistry and hydrides. His first publication on this topic reported no excess heat and was cited in the DOE Panel report of November 1989. More than six months would pass before the first excess heat registered on his calorimeter. Miles agreed with Bush that it would be worthwhile during 1990 to search effluent gasses for the atomic products of heat generation and, in particular, for helium. They recognized that other parties had already mentioned detecting helium in the off-gasses from cold fusion experiments. But even more compelling, helium is the nuclear reaction product of fusion thus helium may be considered diagnostic of fusion.

Precious few product atoms are expected when compared with the voluminous bubbling off of the oxygen and deuterium gasses, with which they are entrained. But helium is a noble gas—it does not combine with other elements; it does not have a chemistry. Various chemical traps can be designed to remove other gasses coming from the cell and helium will remain as the survivor. This helium is then transferred to a mass spectrometer where it can be identified by its mass of 4.0026 AMU.

A plan was devised for the fall of 1990 wherein Miles would generate the excess heat, do the required calorimetry, and collect the helium samples in flasks provided by Bush (at Austin). (Miles also would look for x-rays, radiation, and neutrons.) The flasks initially contained only boil-off gases from liquid nitrogen, void of detectable helium. Miles would fill the flasks and Bush would have them analyzed.[10] The helium flasks would be shipped to Austin and to independent laboratories for a blind measurement of the quantity of helium; Bush did not know the heat generation associated with the helium sample. In a somewhat more logically structured formulation, the experiment was to correlate the presence of helium to the generation of excess heat. A control was provided by performing helium analysis on samples collected when the generation of excess heat was zero.

Table 16.3 shows the results of eleven gas samples that were collected by Miles when the calorimeter showed excess heat generation. These samples from active cells were collected between October and December 1990. Experiment 12/14/90-B was lost when its flask broke during shipment.[11] By inadvertence 12/17/90-B was allowed to lose electrolyte until the electrodes were exposed, thus permitting recombination to appear as excess heat, so it was omitted from the analysis process. These two samples were not considered meaningful for inclusion in the analysis.

Experiment 10/17/90-A showed too little excess heat to generate detectable amounts of helium and none was detected.

The designation P_{ex} stands for excess heat in Watts, P_{out}/P_{in} is the power increase factor of the cell.

Miles filled six flasks in January 1991 from experiments using palladium and light water, a condition that never generates heat, these flasks served as control samples. Testing of the gas in the flasks was done at the University of Texas as before.

The designation of large (lge), medium (med), and small (sml) refers to the amplitude of the waveform shown by the mass spectrometer which was always set to its maximum sensitivity for these measurements. The values of excess power were those measured during the time period when collecting the gas. The measured amount of helium-four, column four, is displayed as the number of helium atoms per 500 ml collection flask for the electrolysis gasses.

Six experimental runs of control cells in January 1991 (not shown) exhibited no excess heat and also measured no helium. This result is strong indication that atmospheric helium was not a contaminant. In each case, those eight

TABLE 16.3 Helium-4 Gas Entrained, Series-I

Period from 1990–1991[a]				Revised[b] in 1992
Sample date (mo/dy/yr)	P_{ex} (W)	P_{out}/P_{in}	He-4 atoms/ 500 ml.	He-4 atoms/ 500 ml.
12/14/90-A	0.52	1.20	10^{14} lge	10^{15} lge
10/21/90-B	0.46	1.27	10^{14} lge	10^{15} lge
12/17/90-A	0.40	1.19	10^{13} med	10^{14} med
11/25/90-B	0.36	1.15	10^{14} lge	10^{15} lge
11/20/90-A	0.24	1.10	10^{13} med	10^{14} med
11/27/90-A	0.22	1.09	10^{14} lge	10^{15} lge
10/30/90-B	0.17	1.12	10^{12} sml	10^{13} sml
10/30/90-A	0.14	1.08	10^{12} sml	10^{13} sml
10/17/90-A[c]	0.07	1.03	$<10^{12}$ none	$<10^{13}$ none
12/14/90-B[d]	—	—	—	—
12/17/90-B[e]	0.29	1.11	$<10^{12}$ none	$<10^{13}$ none

a. Bush, B. F., Miles, M. H., G. S. Ostrom, and J. J. Lagowski, "Heat and Helium Production in CF Experiments," (Proc. ACCF-2, The Science of Cold Fusion, Como, Italy, 1991), p. 366.

b. Miles, M. H., Bush, B. F., et al., "Anomalous Effects Involving Excess Power, Radiation, and Helium Production During D_2O Electrolysis Using Palladium Cathodes," (*Fusion Technology,* vol. 25, July 1994), Table II, p. 481.

Miles, M. H., and B. F. Bush, "Search for Anomalous Effects Involving Excess Power and Helium During D_2O Electrolysis Using Palladium Cathodes," (*Frontiers of Cold Fusion,* ICCF-3, U. Academy Press, 1993) Table 2, p. 192.

c. While this experiment did show some excess heat, the level was insufficient to produce helium above the minimum detection level of the spectrometer.

d. The collection flask for this experiment broke during shipment.

e. A calorimetric error is anticipated here due to low D_2O solution levels that exposed the electrodes to allow recombination of the gasses in the cell's head-space.

flasks of gas were found to contain helium when the respective cell exhibited excess heat. In summary, Table 16.3 shows eight instances of helium and heat, and there were six instances of no heat and no helium.* These helium quantities were later revised (column five) to account for the effects of diffusion of helium into the flasks.

It was ultimately necessary to measure the diffusion rate of atmospheric helium into the 500 ml Pyrex flasks. While this study of helium diffusion was of importance and permitted a reevaluation of the results in Series-I, it is not presented here. It was found that helium from the air diffused into the glass flasks at a measurable, but predictable and tolerable rate. In 1991–1992 Bush

* No helium-three was detected in any of either the active or control experiments.

and Miles found that the diffusion rate of airborne helium into the flask was reduced by one-quarter if nitrogen in the flask was replaced by deuterium. The outward diffusion of deuterium apparently hinders the inward diffusion of helium. The diffusion rate was found to be linear within useful limits.

These studies of helium diffusion into Pyrex flasks now indicate that the approximate amounts of helium observed for Series I was incorrect and that the number of helium atoms per 500 ml should be increased by one order of magnitude. The revised values for Series I are given in column five of Table 16.3. For these revised values, yielding 10^{15} helium atoms for each 500 ml of released gasses, the rate of helium creation is 10^{11} to 10^{12} atoms of helium-four per second per one watt of excess heat.[12]

At China Lake, Bush and Miles continued their collaboration but were unable to reproduce the excess heat effect during much of 1991. Eventually, a second series of experiments succeeded in producing a useful excess heat effect. These experiments (Series II) were run during December 1991 and January 1992, and precise helium analyses were performed by Rockwell International Corp. These are presented in more detail in Hoffman's book starting on page 176. He says of them, "No definite conclusions can be drawn concerning these observational levels . . ." of helium-four. Hoffman did not have the corresponding anomalous power values. They were given to a third party and held until the helium measurements were in. Hoffman was not in a position to draw conclusions, but his measurements support the Bush and Miles reports.

The results are shown in Table 16.4. The number of helium atoms produced per second per watt of anomalous power is shown as 1.9×10^{11}, 2.5×10^{11}, and 5.2×10^{11} atoms. These are to be compared with 2.6×10^{11} helium atoms per watt value for the ordinary deuterium-deuterium fusion reaction cited earlier. While the results of Series II are in the ballpark, so to speak, they are limited to one decimal place by the excess power measurements. Successive measurements of helium taken over a period of weeks allowed the rate of in-diffusion of helium to be measured, so that back calculation to the helium in

TABLE 16.4 Helium-4 Gas Entrained, Series-II[a]

Sample date and flask used	P_{excess} (W)	P_{out}/P_{in}	^{4}He atoms/ 500 ml	^{4}He per second per watt excess heat
12/30/91-B/F5	0.100	1.08	1.34×10^{14}	1.9×10^{11}
12/30/91-B/F3	0.050	1.02	1.05×10^{14}	2.5×10^{11}
01/03/92-B/F4	0.020	1.01	0.97×10^{14}	5.2×10^{11}

a. Miles, M. H., Bush, B. F., et al., "Anomalous Effects Involving Excess Power, Radiation, and Helium Production During D2O Electrolysis Using Palladium Cathodes," (*Fusion Technology,* vol. 25, July 1994), p. 478.

the flask at the time of collection (time zero) was possible. The study was designed to define the in-diffusion rate of helium in the sampling flasks.

Although diffusion of helium into glass was under some control, obviously the experiment ought to give a starker result if glass were avoided. Bush constructed metal flasks designed to preclude ingress of atmospheric helium during the later part of 1992. After his appointment expired in March 1993, Miles continued these experiments alone as Series III in mid-1993 and 1994 using Bush's metal flasks rather than the Pyrex flasks of Series I and II.[13]

Five experiments that produced no measurable excess heat were used as controls (they were palladium cathodes with D_2O and LiOD electrolyte). These yielded, for the level of background helium in their cell apparatus, gas collection, and distributed helium-measuring system, a mean value of helium for the five runs of 4.4 $\pm$0.6 parts per billion (ppb) or 5.1 $\pm 0.7 \times 10^{13}$ atoms per 500 ml of electrolytic gasses.

Seven experiments produced significant excess heat using Pd and Pd-Boron cathodes with heavy-water electrolyte. They produced the helium values shown in Table 16.5, where they are correlated with the measured excess heat. Column one gives the date of the experiment and shows the letter assigned to the particular electrochemical cell. The right-hand column shows the calculated number of atoms of helium created per second of time per watt of excess heat.

In summary, Bush and Miles completed two experimental series in 1990–91, and 1991–92 using glass flasks, and Miles completed a third in 1993–94 at China Lake, while Bush completed a third at SRI, both using metal flasks.[14] Though Bush and Miles worked cooperatively in the first two series, the

TABLE 16.5 Helium-4 Gas Entrained, Series-III, (Miles)[a]

Sample date and flask used	^{4}He(ppb) in flask	P_{ex} (Watts)	^{4}He/500 ml above bkgnd	^{4}He/sec per Watt
05/21/93-3/A	9.0 $\pm$1.1	0.055	5.1×10^{13}	1.6×10^{11}
05/21/93-4/B	9.7 $\pm$1.1	0.040	5.8×10^{13}	2.5×10^{11}
05/30/93-1/C	7.4 $\pm$1.1	0.040	3.3×10^{13}	1.4×10^{11}
05/30/93-2/D	6.7 $\pm$1.1	0.060	2.4×10^{13}	7.0×10^{10}
07/07/93-1/A	5.4 $\pm$1.5	0.030	1.0×10^{13}	7.5×10^{10}
09/13/94-2/A	7.9 $\pm$1.7	0.070	3.9×10^{13}	1.2×10^{11}
09/13/94-3/B	9.4 $\pm$1.8	0.120	5.6×10^{13}	1.0×10^{11}

Note: Background (bkgnd) is 5.1 $\pm 0.7 \times 10^{13}$ of ^{4}He/500 ml.

a. Miles, M. H., B. F. Bush, "Heat and Helium Measurements in Deuterated Palladium," (*Trans. Fusion Technology,* 26 (1994) p. 159, Table III.

Miles, M. H., personal correspondence, June 6, 2001, providing data not previously published for column four and for the 1994 experiments of the last two rows.

experimental work of operating the apparatus, performing the calorimetry measurements, and collecting the electrolytic gasses into flasks was done by Miles. Three laboratories were used to measure the gas samples: University of Texas at Austin, Rockwell International at Los Angeles, and DOI, Bureau of Mines, at Amarillo, Texas. All three sets of experiments gave helium production rates for each cell that lay between 0.7×10^{11} and 1.0×10^{12} helium-four atoms per second for each watt of excess heat being generated by the cell. These values are to be compared with the theoretical value of 2.6×10^{11} helium-four atoms per second for one watt of excess heat. Funding for this program ended in 1995.

During the spring and summer of 1993 after he had arrived at the SRI laboratory, Bush put together a version of the experiment which included for the first time his own cell and calorimetry instrumentation as well as the gas collection system.[15] His calorimetry used the Seebeck method, a technique used much earlier in the Oriani paper that was submitted to the journal *Nature* in August 1989.

Bush used an all metal collection manifold and collection flasks. The results are shown in Figure 16.1 for the three samples analyzed. The vertical axis is labeled in watts (energy per second) of excess heat being generated at the moment of gas collection. The horizontal axis is calibrated in the number of

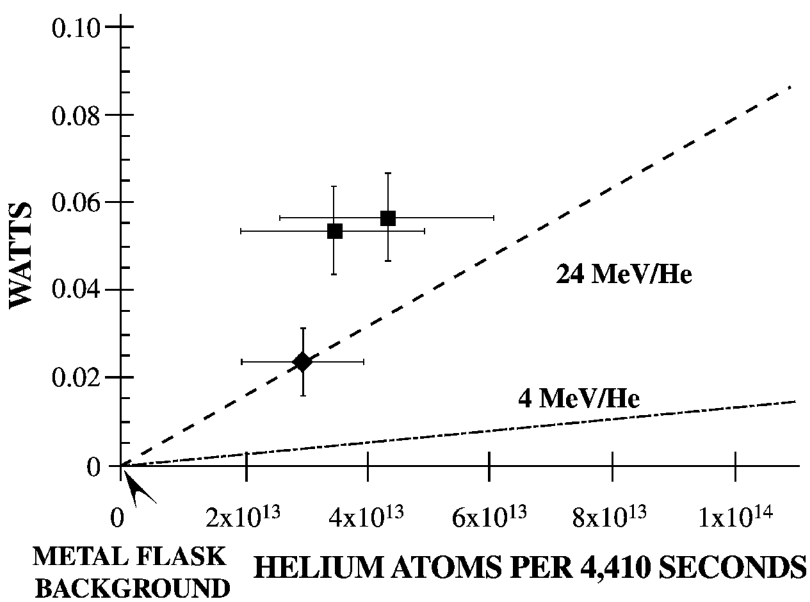


FIGURE 16.1 Bush reported the energy level generated per each helium atom detected from a cell exhibiting the excess heat phenomenon. The 4,410 seconds is the time required to generate 500 ml of electrolytic gasses at a normalized electrolysis current of 525 mA.

helium atoms produced during the time period of 4,410 seconds required to generate 500 ml of electrolytic gasses at a normalized current of 525 mA. The two diagonal reference lines show where the data points would fall were the heat and gas measurements perfect and each nuclear reaction produced a helium atom while releasing either 4 MeV or 24 MeV respectively of heat.

Bush ran these three experiments in addition to a number of control runs in the SRI laboratory. Some of the helium atoms, apparently, did not get out of the cathodes and others did not get through the traps. This experimental limitation gave a higher value for the ower per helium atom ratio than expected for the energy assigned to each of the measured helium atoms. Figure 16.1 is evidence that (1) Bush found and counted the nuclear products from heat generation, and that (2) the source of the excess heat is a nuclear reaction whose outcome is similar to that of deuterium-deuterium fusion with the resultant energy release appearing as heat in the palladium lattice rather than as a 23.8 MeV gamma ray as in two-body hot fusion reactions.

These same months of 1993 saw Miles, at China Lake, also working with an all metal gas collecting system. In due course he gathered seven data points from gas collected while generating excess heat. These points are shown on Figure 16.2 (●), and they appear to be clustered about the 23.8 MeV per atom of helium reference line.

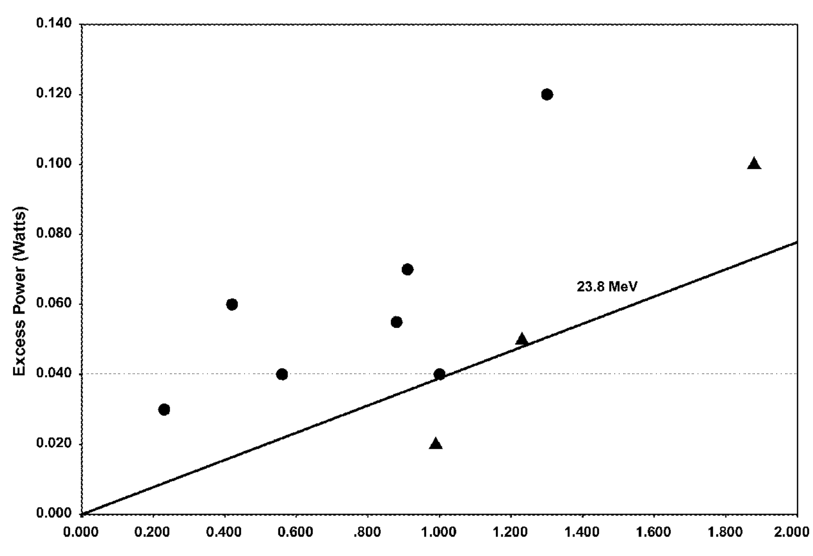


FIGURE 16.2 Miles reports ten data points to show correlation of helium-four with excess heat at a rate of approximately 23.8 MeV per atom. These points are taken from Tables 16.4 (▲) and 16.5 (●). Power measurements limit the accuracy of these points to one significant place.

So the nuclear product, demanded by the skeptics for so many years, had at last been, at the least, tentatively found. In these instances, it was not located inside the cathode rod, but was formed at the surface and was entrained with the bubbles of deuterium and oxygen gasses leaving the cell. Thus two experimenters working in separate laboratories obtained values of helium-four generation commensurate with a nuclear reaction process of deuterium-deuterium fusion. Their results are best summed up in their own words.

> Three sets of heat and helium measurements have yielded similar results. Our first experiments (1990 to 1991) using Pyrex glass flasks resulted in eight experiments that yielded heat and helium, and six experiments that gave no excess power and no detectable helium. Our second set of experiments (1991 to 1992) also used Pyrex glass flasks and involved three experiments that produced excess power and helium. Our final set of measurements (1993 to 1994) used metal flasks. Six experiments produced no excess power and only background levels of helium. Seven experiments yielded excess power and helium production.
>
> We report 18 experiments with excess power and elevated helium levels, along with 12 experiments showing no excess power and no excess helium. To our knowledge, there are no experimental errors that can explain these results.[16]

The reports resulting from the several years of endeavor by Bush and Miles were severely criticized by Steven E. Jones, professor of physics, and Lee Hansen, professor of chemistry, BYU, in the *Journal of Physical Chemistry,*[17] although the original papers were published elsewhere. The journal's editor did not follow conventional protocol by placing Miles's defense in the same issue. Rather, after much pleading with the editor, it appeared three years later.[18]

There is a sense of futility in this mention of the Jones criticism. Jones does not allow that a record of anomalous power exists in the scientific literature. He must, *a priori,* find that helium is not a product of anomalous power generation because (1) he is convinced that anomalous power does not exist in cold fusion experiments, and (2) the evidence for anomalous power generation is far more extensive than the evidence for helium. These arguments critical of the helium-four measurements have been debated earlier, yet they are presented in the referenced paper as though they had not been previously mentioned. It would not serve any pedagogical purpose, therefore, to present Jones's arguments here in detail as was done earlier with Wilson's critique. The interested reader will have to resort to the referenced literature.

This writer agrees with Jones's assessment that the Miles series of experiments does not offer "compelling evidence." But I disagree with Jones when he asserts that the evidence is "far from compelling." The evidence is strong—strong enough to be intellectually and scientifically interesting. It is sufficiently so that one wants to see this experiment continued (1) in an integrated laboratory with the necessary instrumentation at hand, (2) at anomalous power levels an order of magnitude higher, and (3) with a much larger power out to power in ratio. The posthumous heat mode of the previous chapter might provide this requirement nicely.

The record shows that there were several suggestions before the fall of 1990 that helium had been detected in the cell's off-gasses. Such claims would continue during the ensuing years to come from increasingly well-designed experiments to detect the presence of helium created in the cell. The Bush and Miles three part series presented above, dedicated as it was to the quantitative and timely correlation of helium and heat, is a major advancement over what preceded and inspired it. The several experimental runs that detected helium from a heat producing cell, and the several that could not detect helium from cells that were not producing heat, taken together, were a scientifically significant set of measurements. The quantitative correlation between heat and helium, such as it was, identified the nuclear reaction pathway as some new variation—presumably a collective and coherent variation—of nuclear fusion.

Experiment Transport

The review of scientific methodology in Chapters 10–12 concluded that the most common type of scientific confirmation for an experimental result was that it could be reproduced in a different laboratory and with different technicians from those of the laboratory where it was first accomplished. We look in this section for the characteristic of interesting experiments to produce their expected result when performed in a new setting.

Among first examples of an attempt to transport an experiment will be the Arata experiment presented in the previous chapter. We will look at how it fared when moved from Arata's laboratory at Osaka University, Osaka, Japan, to McKubre's laboratory at SRI International, Menlo Park, California.

The SRI International relationship with EPRI had ended by 1998 and a series of new sources of funding for the laboratory took its place. All of these involved a wider variety of scientific activity, but all of it within the scope of cold fusion research. McKubre's laboratory evolved into a laboratory that duplicated the work of other laboratories so as to validate their experiments as scientifically corroborated procedures. They attempted to replicate the results of the Miles, Bush, Arata, Case and other experiments. One larger purpose was to further correlate anomalous power with helium production.

McKubre designed a series of experiments to accomplish this purpose. Seebeck type calorimetry was adopted, but the cells were now closed to the atmosphere to avoid possible atmospheric helium contamination. The off-gasses were recombined inside the cell into water that was returned to the electrolyte, but provision was made to collect gas from the head space over the electrolyte. The cells were made of metal, as was the collection system.

Three runs were accomplished in the fall of 1998 with cells that were generating a statistically significant level of excess heat. The number of helium atoms produced, however, amounted to only 76% of what would be expected if 23.8 MeV of heat energy were given off for each helium atom created.[19] Three other runs that demonstrated excess heat gave marginal readings of the quantity of helium as did several control runs. McKubre interpreted the helium results (in the presence of excess heat) as indicative of a delayed release of helium to the off-gas in the head space. In a later series of experiments, he found that this was indeed the case. He tentatively confirmed the Bush-Miles relation of helium and heat, but it is clear that much work remains to be done if this correlation is to be firmly established.

During this same period, researchers in Japan, the U.S., and Italy were also measuring helium-four in the effluent gasses of experiments that were generating excess heat. Their experimental results will be presented as it was accomplished in their own laboratory. Following that, we will look at the degree of success achieved in McKubre's effort to replicate their results in his laboratory at Menlo Park, California.

Recall that Arata's double structured cathode produced hundreds of megaJoules of excess heat (see Figure 14.7). The hollow cathode is filled with palladium powder of 0.04 micron particle size and welded closed under high vacuum. When it is submitted to electrolysis, deuterium diffuses through the wall of the cathode and enters into its interior space to achieve a considerable pressure. The excess heat reported in previous chapters is generated in the palladium particles located there.

The necessary quadrupole mass spectrometer (QMS) instrument was available in Arata's laboratory and was dedicated to the experiment. He reported that the helium produced in the Pd particles mostly remained locked within the lattice. Only a small fraction of the helium produced entered the gaseous state directly during the course of the experiment. This is in contrast to the work of Miles and Bush, where helium was apparently generated on the surface of the cathode and more than half of it released into the gas-stream effluent. This different result may be explained intuitively by noting that the helium produced in the inner cathode space was physically separate from the gasses boiling off from the cathode's outer surface. Also, diffusion of helium

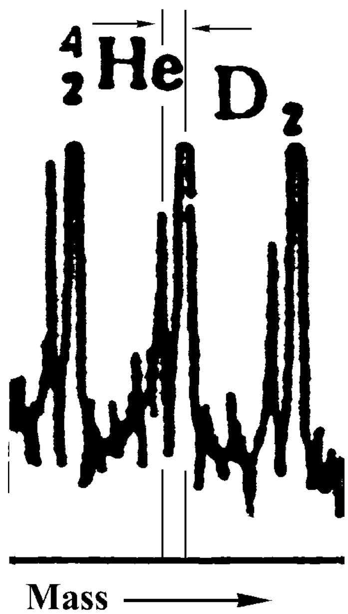


FIGURE 16.3 Arata shows the presence as well as the separation of He-4 and D_2 in a quadrupole mass spectrometer (QMS).

out of the cathode would be a slow process compared with the diffusion of deuterium into the cathode.

After the experimental run was complete, the cathode was carefully opened. The palladium powder was removed from the cathode and baked in a vacuum at temperatures as high as 1000C to force the helium out of the palladium lattice.

The QMS generated graph shown in Figure 16.3 gives evidence of the presence of helium-four (three cycles of output reading are displayed).* The two peaks (for each cycle of the QMS) are due to the presence of helium and residual deuterium residing cheek by jowl, helium at 4.0026 and deuterium at 4.028 amu.[20] The dip in the tracing between the peaks shows how well these two values can be separated in the QMS instrument.

While "much helium-four" was found, there was no evidence of either helium-three or tritium being also present.

The scientists in Italy had moved quickly into cold fusion studies in the

* The QMS repeats the waveform continuously on the strip-chart. In the figure, three successive and somewhat overlapping scans of the QMS are shown.

spring of 1989, led by Dr. Scaramuzzi. Since then several groups have been doing research while others were involved in entrepreneurial activity. The second “annual” Cold Fusion Conference was held at Como, Italy, in June 1991, and ICCF-8 was held in Lerici, Italy, in May 2000, a reflection of Italy’s commitment and contribution to this field of research.

Dr. D. Gozzi, Department of Chemistry, University of Rome, captured helium-four that is entrained in the gasses from a cell which is generating excess power. His purpose was similar to that of Miles and Bush. He first learned to achieve the generation of anomalous power in 1989. His line of experiments began in 1991 and continued into 1997.[21] He begins with a rather conventional cell and calorimeter, but the effluent gasses need to be rather elaborately treated. After getting rid of the preponderance of other gasses, the remainder was stored temporarily. Later, in a mass spectrometer, where the helium-four and the residual deuterium were resolved, he detected and measured helium.

His first experiments indicated helium gas present in large amounts, but they were flawed by air leakage which he detected by monitoring for the presence of neon-20.* He then rebuilt the experiment in such a way that new tests showed the elimination of any significant air leakage. This second round of experiments also showed relatively large amounts of helium. But his attempt to correlate heat generation with helium detection was marred by inadequate flushing of the gas system at the start of the experiment. Gozzi later did a set of flushing exercises and retroactively applied the results to his collected data. The result was clear, though not overwhelming, evidence that helium in amounts corresponding to the measured heat was produced in synchronism with the heat generation after the effects of residual gases were subtracted from the previously collected data.

He planned a third round of experiments that would use an advanced gas flushing protocol, an integral mass spectrometer, and would have monitored neon++ (rather than neon+) for air leakage so that all readings would fall within one expanded spectrometer scan.† Unfortunately in 1997, his funding ended.

Dr. Tullio Bressani, professor of physics, Department of Experimental Physics, University of Torino, Torino, Italy, took his own direction in the field. He picked up on the electromigration work of Alfred Cöhn (1929) that was described in Chapter 2, p. 33. A thin palladium ribbon an inch wide and

* This is the element neon of atomic weight 20. Neon is a well-recognized constituent of air. Monitoring for it provides a check on possible leakage of air into the experimental system.

† Questions of calibration become simpler to resolve if the values of interest lie within one scale range of the mass spectrometer. By doubly ionizing the neon atoms, they will respond as if their mass weight were ten rather than twenty. The values of 4 (helium) and 10 (neon x ½) can be fit on one scale of the mass spectrometer.

eight inches long was loaded with deuterium gas. It was then placed in an atmosphere of deuterium gas while a voltage was applied between the ends of the ribbon. The Cöhn effect was used in this manner to achieve a high deuterium loading into the palladium.[22]

In Figure 16.4, the upper tracing shows the presence of deuterium at 4.0282 (amu) mass in a control sample taken just before the experiment. There is no trace of helium at 4.0026 amu to the left of the deuterium peak.

This peak at 4.0026 (amu) (see the lower tracing) demonstrated the creation of 5×10^{18} atoms of helium-four during the experiment. The helium peak appears after the experiment had run its course. It constitutes strong evidence of nuclear processes at work in the palladium ribbon. There is no assumption here that the purported helium atoms came from a deuterium-deuterium fusion reaction. A reaction that created alpha particles (ionized helium), for example, would also have resulted in a corresponding proliferation of helium-four atoms.

McKubre was also interested in devising his own search for heat and nuclear product correlation.[23] In 1998 he designed an experiment following the electrolytic loading and mass flow calorimetry of his earlier work.[24] His cathode now became a wire rather than a rod and the cell was closed. This set of experimental runs was in their essence replications of earlier ones, but were done with sealed systems.*

Eighty-two kiloJoules of excess heat were generated in one experimental run, but the sample of gas taken out of the head space had a quantity of helium only 62% of the amount needed to match the calculated deuterium-deuterium fusion energy release of 23.8 MeV per reaction. It was concluded that some of the helium generated was sequestered in the palladium cathode, which was then subjected to 200 hours of polarity cycling† to dislodge any retained helium. Two more samples of gas were analyzed and used to calculate all the helium involved in the experiment. The resulting value of the production of helium in the experiment was 104 ±10% of the number anticipated by a deuterium plus deuterium fusion reaction.

L. C. Case Experiment

The Case experiment that displayed excess heat as discussed in the previous chapter was also examined for nuclear products.[25] To review, it consists of a canister in which are placed 10 or 20 grams of the catalyst, a fibrous carbon

* In this instance, his system was no longer isothermal as in his earlier experiments.

† Polarity cycling involves repeatedly reversing the power connection to the electrodes so as to excite the surface of the cathode.

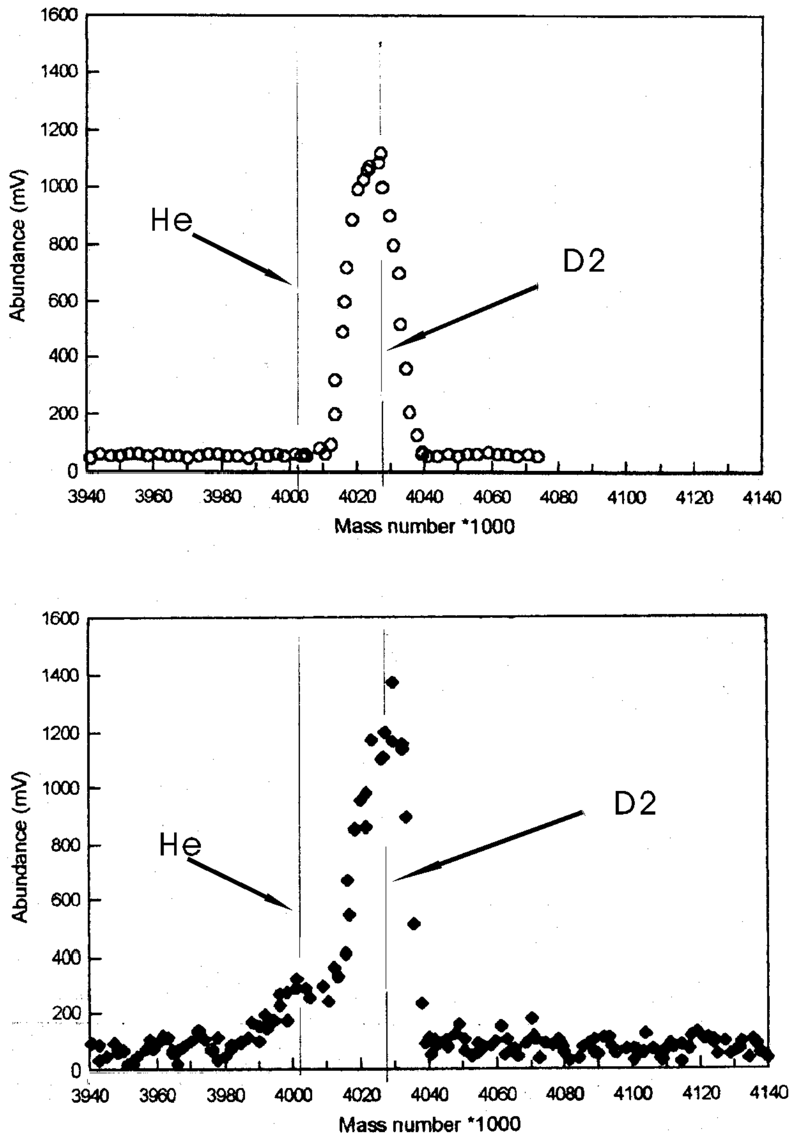


FIGURE 16.4 Bressani reported the generation of He in Pd ribbon. Upper tracing was taken before the experimental run; lower is after. The fine vertical lines mark He at 4.0026 amu. and D_2 at 4.028 amu.

base with 0.4% metallic palladium embedded in it. The canister is pressurized to about three atmospheres (45 lbs/in^2) with deuterium gas.

The canister is warmed to about 200C with an external heater. A mass spectrometer records the level of helium-four atom concentration inside the canister during the month or so experimental run. Case reports that helium is produced in the experiment, but that result has not been published.

The laboratory work done by Case over the past ten years has been of the limited form of experiment discussed in Chapter 4, p. 53. He is not interested in publishing papers, but merely in constructing operating devices and getting rich. We must wait until his work is confirmed in other laboratories that do more formal work and publish it. But at least one careful and well equipped experimenter, using catalyst samples that had previously been used successfully, failed to get any response out of the palladium-carbon catalyst in deuterium gas.[26]

Case allows that the nuclear process might be a catalyzed deuterium plus deuterium fusion reaction to produce helium-four plus 23.8 MeV of energy transferred to the lattice as heat. Case speaks of this reaction hypothesis in properly tentative language. Measurements in the nuclear laboratory at Charles University, Prague, showed no evidence of tritium, fast neutron, or gamma ray generation, only helium was detected. Thus the more common deuterium-deuterium reactions are not candidates.

Case was careful to work only with catalysts available from commercial suppliers. The particular catalyst used in his experiments can be purchased in 55 gallon drums. If the energy producing reaction were as presented above, the fuel cost may be estimated as one one-hundredth that of coal. Large amounts of catalyst would be needed because the heat for each pound of catalyst is low. That is, the heat generation comes at a power density that is disappointingly small.

The Case experimental results as reproduced by McKubre at his SRI laboratory are shown in Figure 16.5.[27] The dashed line in the figure shows the level of helium in the atmosphere at standard pressure, 5.22 ppm.

The figure depicts evidence of a nuclear reaction within the experimental canister. The tracing shows an increase of helium beginning at about day four. The helium level reaches the level that would occur if air were leaking into the pressurized system on day seven. At 19 days, the level of helium in the canister exceeded that in the air outside the canister. At 28 days the level of helium-four reached 11.50 ppm. Identical control experiments using hydrogen did not produce helium. This experiment using deuterium gas has been performed several times at SRI, sometimes producing nothing and sometimes producing similar levels of helium. It appears that the helium can have come only from a nuclear reaction within the canister. This experiment also shows that the Case experiment is reproducible by other scientists in other laboratories.

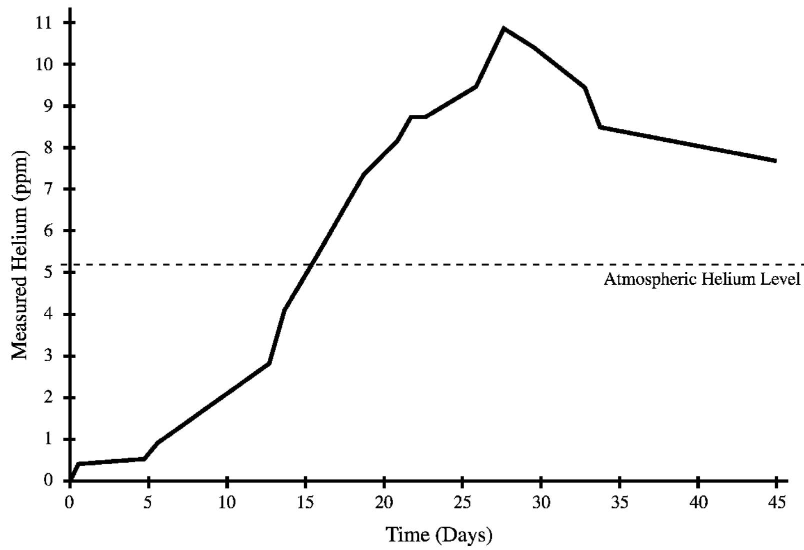


FIGURE 16.5 The Case experiment at McKubre's laboratory showed helium-four generation at eleven parts per million in 28 days, then decreasing at about the same rate.

At 28 days, an anomaly appeared in the data. The level of helium-four abruptly began to decrease. It continued to do so at several different rates until day 45 at which date the experiment was terminated. The reason for the abrupt change from a positive slope to a negative slope has not been explained. Overall, however, McKubre estimates that the carbon base of the catalyst, operating at 200C, ultimately absorbs the generated helium.

Evidence accumulating from Bush, Miles, Arata, Gozzi, McKubre, Bressani, and Case points to the source of excess heat as a nuclear reaction that results in the creation of helium-four and heat energy. The energy released for each helium-four atom is approximately 24 MeV. The presence of that amount of heat implies a nuclear source that is able to pass its energy as heat to its surroundings in the lattice.

Figure 16.6 shows the history of groups involved in this line of research. The vertical dashed line marks the date when a portable mass spectrometer instrument became available that could separate the nearly identical mass values of one atom of helium-four from one molecule (two atoms) of deuterium.[28]

Seven laboratories have reported helium-four gas that corresponds in some degree to the quantity and time of anomalous heat generation. In this fashion, cold fusion research has moved forward the work of searching out the energy source. One can only hope that these experiments will be continued with their task of quantitatively correlating generated heat and nuclear ash.

From the evidence for helium-four, collected in these Bush-Miles experi-

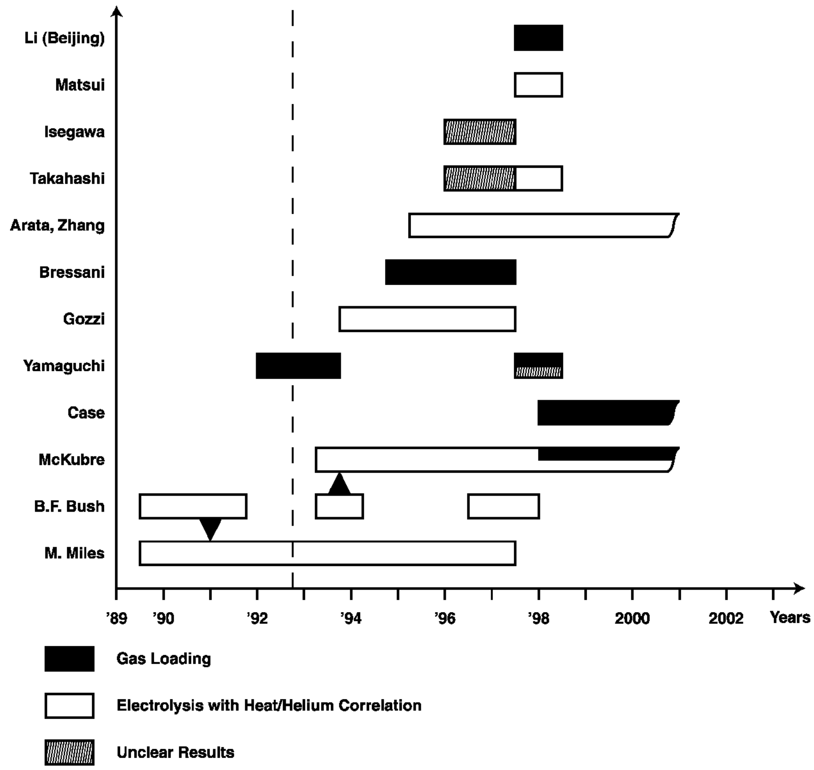


FIGURE 16.6 A listing of those scientists who search for evidence of helium-four atoms created by the reaction used to generate excess heat. The vertical dashed line marks when the portable QMS became available. The triangles indicate cooperative efforts. Drawing by the author, after Bressani.

ments followed by those in other, separate, laboratories, it may reasonably be concluded that helium-four is an established candidate for the sole nuclear product that accompanies heat generation.

The reaction of interest cannot be the two-bodied fusion as known by hot fusion physicists because that reaction generates lethal radiation and fast neutron particles that are not present in cold fusion experiments. It cannot be the reaction of muon-induced (catalyzed) fusion,* because that reaction also generates strong radiation not present in cold fusion data. Several theorists have suggested that the process might be the result of a multi-body or lattice-coherent reaction, one that brings together an ensemble of particles into the reaction process.

* Muon-induced fusion is a scientific effect that is not a basis for commercial power production.

The field of cold fusion research has now approached the criteria called out by Frank Close: the proof of test-tube fusion would occur when its products turned up in amounts corresponding to the excess heat.[29] At this remove, the techniques and the direction of this research seem clear and worthwhile. Miles's funding ended in 1995 and Gozzi's in 1997. One can only hope that other funding will continue, and look forward to the time when a laboratory dedicated to this goal can be established.

The difficulty in resolving the relationship between excess heat and the production of helium-four relates directly to the difficulty of establishing such a laboratory. It would necessarily involve real-time collection of the effluent gasses, system capacity for 100 watt operation of multiple cells with their corresponding calorimetry requirements, and the availability of a variety of QMS instruments as illustrated by the referenced literature, along with support facilities and technicians. There is no question of two points: (1) the phenomenon of anomalous power remains the presenting and identifying characteristic of this field, and (2) the pursuit of the nuclear reaction product can bring substantial intellectual rewards.

Those who are concerned with environmental issues may find these results alarming as they may undermine efforts to restrict individual access to energy for discretionary use. Most environmentalists, recognizing the inherently benign character of this energy source, will applaud these achievements.

Hot fusion gets a 23.8 MeV gamma ray with each helium-four atom, but easily measured gamma rays are not seen. With cold fusion experiments a similar amount of energy, 23.8 MeV per helium atom is released, but that energy appears as heat not as gamma rays. This implies that a different physics results in the reaction when it takes place within a certain kind of lattice structure.

In hot fusion the ratio of both tritium and neutrons to helium-four is about 5 million. Hence, the next two chapters examine evidence for these important deuterium fusion products.

CHAPTER SEVENTEEN

Tritium and Helium-Three

The government has a special interest in tritium, the heaviest isotope of hydrogen, because of its use in nuclear weapons. It is a radioactive gas that has one proton and two neutrons in the nucleus and a half-life of 12.3 years. When it decays, it emits a beta particle to become helium-three. Because of its short half-life, tritium does not appear in high concentrations in nature. Its production in an experiment is important because that is proof that a nuclear process occurred.

Dr. Bockris counted 147 papers by the end of 1994 that claimed generation of tritium in a Fleischmann and Pons type of cell. Six years later, at about the time of ICCF-8 in the early summer of 2000, the frontier of cold fusion research had moved from the detection of helium-four, as illustrated in Chapter 16, to the measurement of helium-three not only as the stable product of the decay of tritium but also as a product of the nuclear reaction of interest. This change is important because, unlike helium-four, helium-three is a rare substance in the atmosphere with a ratio to helium-four of about one to one-million. Its emergence in an experiment in a different ratio would indicate nuclear activity as distinguished from atmospheric contamination. Its detection also might help to identify a particular nuclear reaction. Helium-three is measured in a quadrupole mass spectrometer (QMS) in a manner like that used for the measurement of helium-four. Because helium-three has an atomic weight close to DH (a deuterium with hydrogen molecule), the QMS must be able to distinguish them from one another if the helium-three is to be identified.

Tritium

Tritium is detected by placing a sample in a scintillation (flashes of light) counter that measures its radioactivity as the number of disintegrations per minute for one milliliter (cm^3) of the sample (DPM/ml). The tritium atom can substitute for a hydrogen atom in water, forming HTO or tritiated water. It appears as a contaminant in heavy water and in deuterium gas, both of which are used in cold fusion experiments. As a contaminant, tritium tends to build up over the days and weeks of cell operation, a phenomenon called enrichment.

Dr. Carol Talcott Storms worked with tritium measurements at the Los Alamos Scientific Laboratory for many years prior to the Utah announcement. She was intimately familiar with the methods necessary to obtain accurate, contamination free, readings.* Dr. Edmund Storms, her husband, moved into cold fusion experimentation immediately after the Utah announcement. By summer of that year the two scientists had dozens of electrolytic cells running and were testing them for tritium activity.[1]

One cell produced tritium as shown in Figure 17.1, where two data sets are displayed. The one designated with squares, run number 70, depicts the output of a cell that did not generate tritium, but did have the tritium normally present in the electrolytic solution as a contaminate. The vertical axis is scaled to discount that artifact. It depicts the ratio of tritium activity in cell number 73 to the residual (background) level of activity in cell number 70.

Tritium generation for run 73 started on day three, and rose rapidly to reach a peak at 25 days when the electrolyte tritium level was 380 DPM/ml. This level indicates an amount greater than the sum of the tritium contained in the initial D_2O, its daily replenishment, plus that lost to the effluent gas. My critique of Figure 17.1 is that the curve may be inappropriately drawn with too much detail, but the data points themselves are of interest. This work was finished in the fall of 1989, and the results were available early in 1990.

Other tritium generation experiments were underway at Texas A&M, College Station, Texas, at about the same time. Dr. John O'M. Bockris, distinguished professor of chemistry, promptly entered the cold fusion fray in 1989. His principal doctoral student in this work was Nigel Packham.

Bockris reported on the performance of one type of cell. It was tested for tritium content from September 11 until October 19, 1989.[2] In Figure 17.2, the vertical axis indicates the amount of tritium activity. The solid tracing shows the quantity of tritium activity present in the electrolyte solution, and

* At Los Alamos there is a deep underground natural reservoir whose body of water is estimated to be over 1,000 years old. It is essentially tritium free, and is occasionally used in the laboratory for reference purposes.

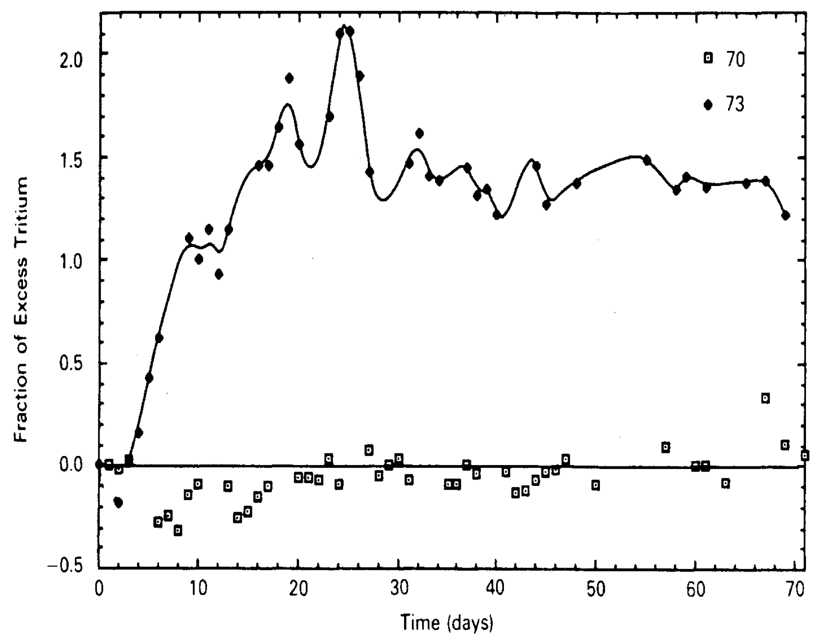


FIGURE 17.1. Storms, at the Los Alamos National Laboratory, reported a clear signal of tritium generation in his cell number 73. The fraction divides the tritium count of cell 73 by the background count as shown in cell #70.

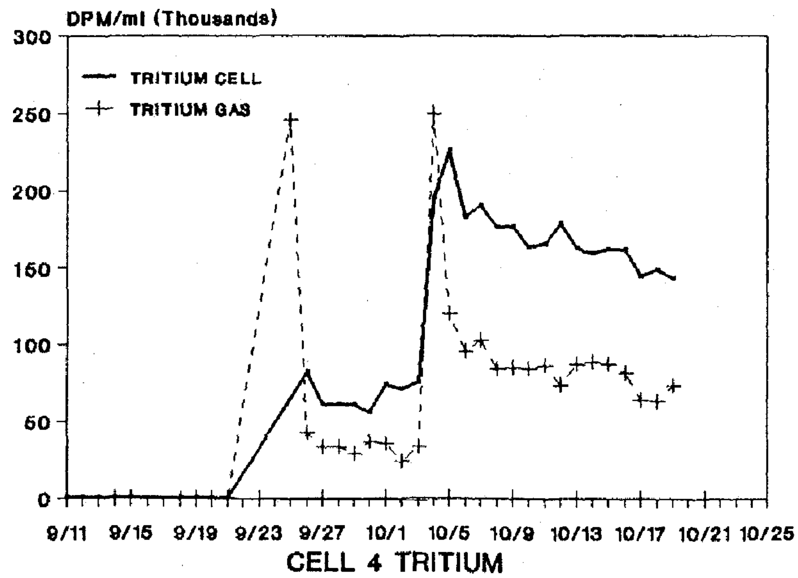
the dashed tracing shows the quantity in the effluent gas. Because the liquid remained in the cell while the emergent gases moved out, the level of tritium in the gas could decrease rapidly from the peak value while the tritium in the liquid decreased more slowly.*

Bockris and Packham observed tritium in 9 out of 13 cells. The measured tritium activity generated in these experiments was ten times larger than the amount shown in Figure 17.2.†

Dr. S. Szpak, a scientist at the Naval Ocean Systems Center, San Diego, California, followed the advent of cold fusion research closely from the early days. He developed a new variation on the Fleischmann and Pons cell by starting with the palladium initially residing as ions in the heavy water electrolyte.

* In 1989 there was published an implication that the reported tritium generation in Bockris's laboratory might be due to the surreptitious addition of tritium (presumably as tritiated water) to the electrolyte. According to Storms's analysis, the level of tritium activity from added tritiated water would stay nearly constant rather than dropping off as it does in Figure 17.2 after 10/4 for the gas, and 10/5 for the liquid.

† Others working at Texas A&M found that there was tritium contamination in their palladium, but that does not appear to have affected this experiment.



FIFIURE 17.2 Bockris reported (Lin, et al.) tritium activity levels from cell 4 in its liquid and gas sectors.

When the current was turned on, it served to deposit palladium on the copper cathode, soon coating it over and then building up a thick layer. Deuterium, also from the electrolyte, was absorbed by the palladium as it came out of solution and onto the surface of the cathode.[3] This procedure permitted control of the palladium material, in contrast to working with solid palladium cathodes of generally unknown impurities and micro-structure.

Immediate generation of excess heat was indicated from the start by the cathode being several degrees hotter than the solution while similar cells using regular water had no difference of temperature. The heating was estimated at 2500 Joules per second and it started after 20 minutes of charging. One cell that was run for ten to sixteen hours experienced a tenfold increase of tritium from an initial reading of 30 increasing to over 230 disintegrations each minute for each ml of electrolyte. No water was added to the cell during this run time. It appears that tritium was produced by the cell during its ten-hour-plus operation.

Dr. Thomas N. Claytor, a physicist at the Los Alamos National Laboratory, Los Alamos, New Mexico, has reported the generation of tritium in glow discharge experiments since 1989.* He did those experiments in a system with

* The term glow discharge refers to the illumination made by an electrical discharge in a partial vacuum, such as in a neon sign.

twin stainless steel containers, one held the glow discharge assembly and the other was used for on-line detection of tritium. The first sealed chamber held a palladium foil as one electrode and after a small gap, the end of a piece of wire constitutes the other electrode. The chamber was filled with deuterium gas at low pressure and a small amount was absorbed into the foil. When electrically excited, a purple glow formed over the foil and extended across the gap to the wire end. Using a sensitive detector in the second chamber, the gas was analyzed for tritium content after the system has run for many hours.[4]

One of Claytor's experimental results is shown in the data of Figure 17.3 that was taken from three experiments run in 1994 (plasmas 1–3). The vertical axis records the concentration of tritium. The current was pulsed (time-varying, non-continuous) to avoid over heating the cathode and anode. Plasma runs number 1 and 2 lie along the base line with number 1 ending at 105 hours and run number 2 ending at 375 hours.

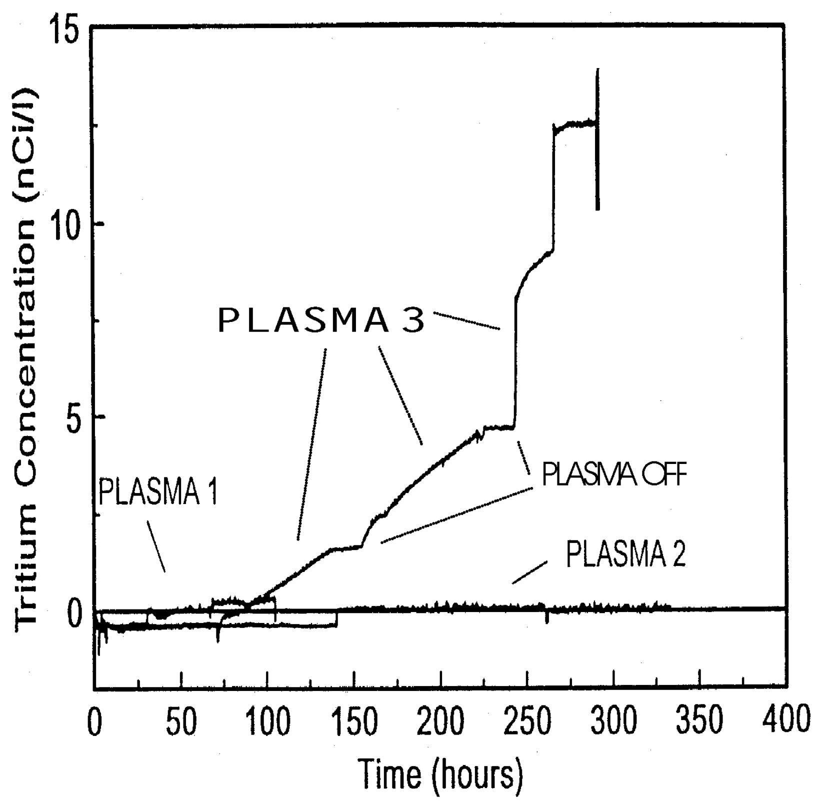


FIGURE 17.3 Claytor reported the generation of tritium in plasma run number 3. Interruption of the plasma interrupted the tritium generation process at two places.

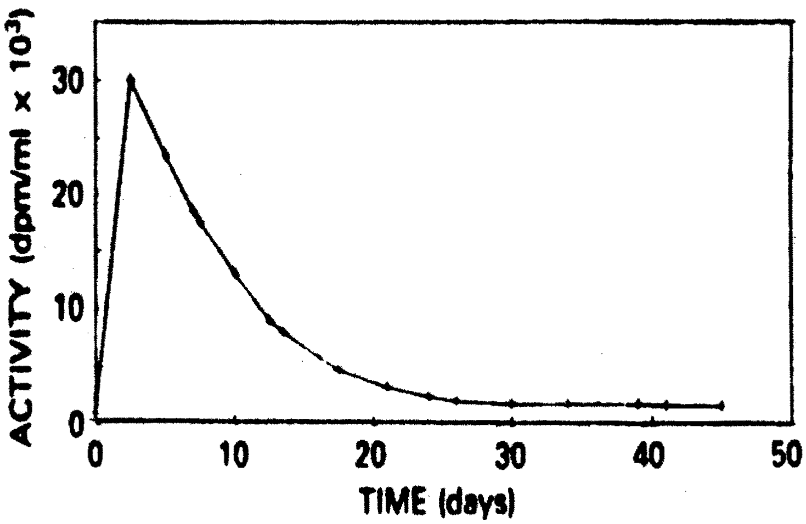


FIGURE 17.4 Scott, Oak Ridge National Laboratory, reported a burst of tritium in one of his cells.

Run number 3 yielded an increased tritium signal. It produced 30 nanoCuries* (nCi) of tritium. The current was turned off for several hours at two points during the run. These are the plateaus labeled "plasma off." Afterwards, the cell was heated to release gas from the electrodes. This produced an additional 52 nCi of tritium not shown in the figure.

Experiments at the Oak Ridge National Laboratory using electrolytic cells reported tritium generation. Bockris recounted this event. "Scott at ORNL reported tritium activity of 25 times the background level in one cell with current densities varying between 200 and 600 Ma/cm.[2] This tritium occurred in a burst lasting 2 to 3 hours."[5]

In Figure 17.4, Scott shows a peak of 30,000 DPM/ml that gradually dissipates from this cell over a period of 20 to 30 days (the curve's rise was not finely plotted). The interpretation of this dissipation curve is that the tritium is in the form of DT gas molecules dissolved in the electrolyte, rather than in the form of tritiated water. The rapid dissipation of the tritium over a month or so was expected.

Critics have pointed out that tritium contamination in the palladium, if there were any, would take the form of DT gas rather than (OT^-) ions in the water resulting in a rapid dissipation of the tritium over a month or so, whereas tritium as ions would remain as tritiated water.

* The Curie is a measure of the quantity of radioactivity. A nanoCurie is a one billionth part, 10^{-9}, of a Curie.

Helium-Three

The normal ratio of helium-three to helium-four to neon-22 in the atmosphere can be used to check quite conclusively for possible contamination of an experiment by leakage from the air. The ratio of helium-three to helium-four (1.3×10^{-6} for the atmosphere) has the further possibility to provide some insight as to the actual nuclear reaction processes that generated it. While working in the chemistry department at the University of Texas, Austin, Texas, B. F. Bush made measurements, Table 17.1, of this ratio in the off-gasses from a cell.

Bush used Calvet (Seebeck) calorimetry to measure for the occurrence of anomalous power. Because heavy water always has a small amount of tritium in it, the gas collection flasks were limited in their exposure to the cell to a couple of hours at most. The amount of tritium in the D_2O that would decay into helium-three during that restricted time would be insufficient to unduly contaminate the helium-three measurement. A crucial step of removing the deuterium and oxygen from the gas stream took advantage of the ability of a copper oxide surface at 450C to recombine the two gasses into water. A liquid nitrogen cold trap then condensed out the water to leave a vacuum holding the small quantity of the two helium isotopes for measurement by means of a mass spectrometer.

TABLE 17.1 Bush Helium-Three Data[a]

Sample	Electrolysis/ma	$^4He/^{22}Ne$	^4He/30ml[b] (Atoms)	$^3He/^4He$
12/04/97	29.3	16.754	5.9×10^{11}	2.14×10^{-6}
12/16/97	9.8	6.056	2.0×10^{12}	2.41×10^{-6}
12/23/97	10.2	7.379	4.1×10^{10}	3.17×10^{-6}
		Additional data[c]		
09/10/97	missing	8.248	5.38×10^{10}	1.66×10^{-6}
09/23/97	18.8	20.199	4.56×10^{11}	2.66×10^{-6}
10/06/97	37.9	14.45	1.72×10^{11}	3.05×10^{-6}
10/09/97	38.1	13.60	1.69×10^{11}	2.85×10^{-6}
10/12/97	38.3	13.64	1.59×10^{11}	3.40×10^{-6}

a. Bush, B. F., and J. J. Lagowski, "Methods of Generation Excess Heat with the Pons and Fleischmann Effect: Rigorous and Cost Effective Calorimetry, Nuclear Products Analysis of the Cathode and Helium Analysis," (ICCF-7 Proceedings, April 1998), pp. 38–42.

b. Columns four and five have been corrected for atmospheric contamination by consideration of the neon found.

c. Bush, B. F., personal communication. August 6, 2001. This material is unpublished. The five experiments use palladium-boron cathodes.

In control experiments (not shown), the ratio of helium-three to helium-four measured out to "about 1.3×10^{-6}," at extremely low concentrations consistent with residual atmospheric contamination. Typical helium-four concentrations were about 100 parts per trillion (ppt) or one-tenth the detection limit of the mass spectrometer used in the quantitative helium-four analysis above. Thus, hydrogen-three decay into helium-three was not a measurement factor even at exquisitely sensitive levels of analysis. This relief came about because the gas collection flasks had limited time exposure to the cell, and the deuterium (and tritium) was removed promptly from the gas sample, after it was collected.

The ratio tabulated in column five may be of help to discern the shape of the nuclear reaction generating the helium. Bush raises the question of how the palladium helps to catalyze the apparent fusion reaction. The deuterium—deuterium reaction can be mediated along its reaction trajectory by electromagnetic interactions if one considers that the reaction emits an electromagnetic gamma ray under hot fusion conditions. Electromagnetic interaction mediated reactions are fulfilled much more slowly than are those under mediation of the strong force, the manner of hot fusion reactions. By happening more slowly, it is possible for the lattice to be involved in the process by absorbing the released energy as heat.

Bush calculates that the helium-three is produced by a deuterium and hydrogen reaction* and the helium-four by a deuterium and deuterium reaction. The former releasing 5.395 MeV and the latter 23.82 MeV of energy. He calculates the relative isotopic ratio according to the formula, noting that the nuclear reaction matrix elements cancel to a coefficient of five for purely electromagnetic interactions.

$$^{3}He/^{4}He = 0.005 \times (5.395)^{5} / (23.82)^{5} = 2.98 \times 10^{-6},$$ [6,7]

This value compares favorably with the ratios as listed in Table 17.1, the last column. This result is most encouraging, if only because the deuterium and deuterium reaction is the most energetic nuclear reaction per weight of fuel for the generation of energy.

We reviewed Professor Arata's experiment, with its hollow cathode design and the copious excess heat produced by it, in Chapter 14, p. 201. Arata reported also the presence of helium-four embedded in the powdered palladium after an experimental run. He also continued this work with the measurement of helium-three. Figure 17.5 is a portion of a QMS printout for this measurement.[8,9] Both helium-three and hydrogen molecules, with one ordinary isotope and one deuteron (HT), are detected with about equal amplitude while the dip in the peak shows the degree of separation achieved.

* The hydrogen-one is provided by the residual presence of light water as .5% of the heavy water.

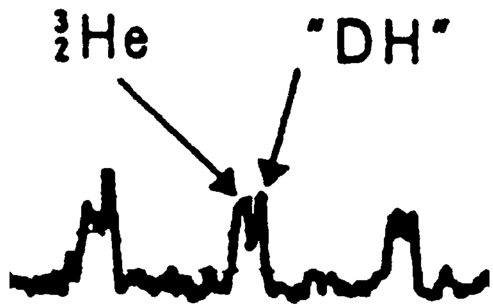


FIGURE 17.5 Arata shows the presence and separation of He-3 and HD in QMS.

Arata reported that inside the closed cathode the ratio of helium-four to helium-three was approximately four, far different from the natural ratio of ten thousand.

McKubre's interest in the reproduction of other's experiments continued in the fall of 1999 when he received two hollow cathodes from Dr. Arata. Each was prepared for electrolysis with about 5 grams of Pd black sealed in its hollow center after evacuation. They were electrolyzed, one in H_2O and the other in D_2O, for three months, after which the polarity on the cells was reversed and they were run for 80 days, the cycle being completed in May, 2000.

The Pd powder that was encased inside a cathode electrode for 90 days generated 64 MJ (17.8 kWh) of excess heat in McKubre's laboratory. At the same time, another cathode was electrolyzed similarly in H_2O and produced no excess heat. Both cathodes were further electrolyzed with their polarity reversed so that the cathode acted in the role of an anode, the purpose being to release helium-four and helium-three, if they are present, from the solid body of the cathode.

Analysis of the results involved the use of McKubre's laboratory, the Pacific Northwest Laboratory in Richland, Washington, and a helium analysis facility at McMaster University, Hamilton, Ontario. There were three components in the hollow cathode to be analyzed: the Pd powder, the retained gasses that might have reached a pressure of 1000 atmospheres, and a residue of water. In addition, the solid cathode body itself would be sectioned and examined.

In September of 1999, at the Pacific Northwest National Laboratories (PNNL) the hollow cathodes were punctured and the gasses collected. The light water cathode's gasses were lost as it had not been properly welded shut, so the light water control experiment was also lost. The cathode electrolyzed in heavy water had its gasses collected, but 90% of them were lost by an equipment failure. The remaining 10% were bottled up and sent to a helium analysis laboratory at McMaster University, Hamilton, Ontario. There, Dr.

W. H. Clarke found no exceptional amount of helium-four in the gasses, but did find significant quantities of helium-three as the daughter product of tritium which then permitted a calculation of the amount of tritium as well as the time at which that tritium appeared in the cathode. The total tritium content in the electrode was approximately 1.8 $x10^{15}$ atoms and calculations of the time of its occurrence indicated that it was produced during the electrolysis run.[10]

Some of the Pd powder in each of the two cathodes was also sent to McMaster for analysis. Again, no significant amount of helium-four was detected, but a considerable amount of helium-three was found.[11]

Tritium seems to have been generated in substantial quantities, but not in quantities sufficient to supply the enormous amount of excess heat detected, assuming the hot fusion of 23.8 MeV of emitted energy per created atom.

Furthermore, an examination of the outer layer of the cathode for helium-four would reveal if it were being produced there for release into the off-gas stream. The report concluded that it was not generated in the solid cathode, at least not by the conventional (Rutherford) deuterium-deuterium reaction.[12]

This effort by McKubre to confirm the Arata detection of large amounts of He-4 and a ratio of helium-three/helium-four of 0.25 resulted in a null result—no confirmation.

In summary, a variety of liquid electrolytic and gaseous glow-discharge experiments enabled the recording of tritium production at significant levels above background. This phenomena continued to be exhibited in various experiments. In hot fusion, tritium production is 5 million times greater than helium production. In cold fusion experiments, this ratio seems to be dramatically smaller, although helium-tritium correlations in the same experiment have not been made. The appearance of relatively little tritium implies that the two-body, hot fusion reaction channel probabilities do not apply to cold fusion reactions.

CHAPTER EIGHTEEN

Neutrons

It is ironic that in this cold fusion episode the rôle of neutrons has proved of only marginal interest after the high authority, even above that of excess heat, it was given by the skeptics in 1989. If the reaction product ratios seen in hot fusion applied to cold fusion reactions, neutrons would be seen in quantities comparable to tritium and roughly five million times greater than helium-four production. So marginal is the place of neutrons in cold fusion experiments, in fact, that twelve years later, evidence of neutron generation seems not only marginal, even ephemeral, a scientific curiosity except for the Mizuno/Takahashi results shown on page 261.

Neutrons, being neutral and unstable, are relatively hard to measure compared to other radiations and to stable nuclei like He-3 and He-4. Neutron measurements are usually made indirectly, for example, by measuring the gamma rays they stimulate.

Some fusion physicists took a paternalistic interest in the claim of small amounts of neutron generation in the electrolytic cell on the grounds that orthodox nuclear physics does not accept the possibility of such an occurrence at room temperature. They quickly showed that the measurements presented in the Preliminary Note were flawed. The neutron generation was far lower than those measurements indicated.

Fleischmann and Pons, however, continued their efforts to measure neutron emission from their cells during 1989. They made what appears to be a recovery from their initial failure by changing detector types and their corresponding data collection pattern.

Neutrons emitted from their cells would strike water molecules in the bath and cause them to emit gamma radiation. Working at the University of Utah during the summer and fall of 1989, the two chemists, using high resolution germanium detectors, collected the gamma signals for more than one hundred days.[1] They reported a sampling technique that allows the long-term accumulation of a weak signal, and their published report appeared in *Il Nuovo Cimento* in 1992. It shows a clear signal for the signature of neutrons interacting with water, at 2,224 keV, n_1, Figure 18.1. The signal is displayed as a ratio to the naturally occurring signal for the 214 isotope of bismuth at 2,293 keV, n_2, used as a reference signal. There were five cells in the bath, and they were turned on at day zero and off at day 205 (see arrow).

After the current was turned off, the signal did not drop completely to zero. They report that this may be due to other cells in other baths that were operating in the same room. The chemists interpreted the amplitude of the signal to indicate a flux of about 5 to 50 neutrons per second for each watt of excess power generated in the cell.

A group of scientists in the Chemical Technology Division at Oak Ridge National Laboratory (ORNL) built and operated a number of electrolytic type cells during 1989.[2] One of these provided neutron flux data for C. D. Scott, as shown in Figure 18.2.[3] He composed a schematic of the data showing the average value with the error bars at the 95% confidence level. He operated a cell at a level of about one half ampere per cm^2. A jump in the excess power was observed as shown at about 1,310 hours into the run. This power is referenced to the left vertical scale. A jump in the average neutron flux at

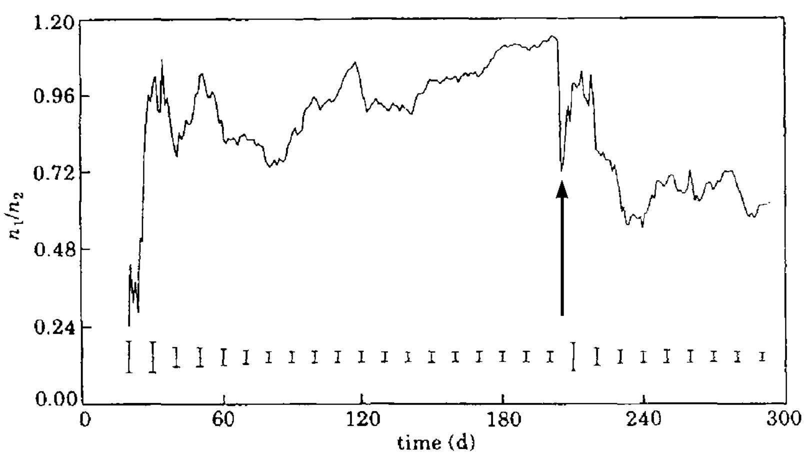


FIGURE 18.1 Fleischmann and Pons reported evidence for neutrons emanating from power generating cells. Error magnitude was depicted by the vertical bars on the graph. The arrow marks the time (205 days) when the cells were turned off.

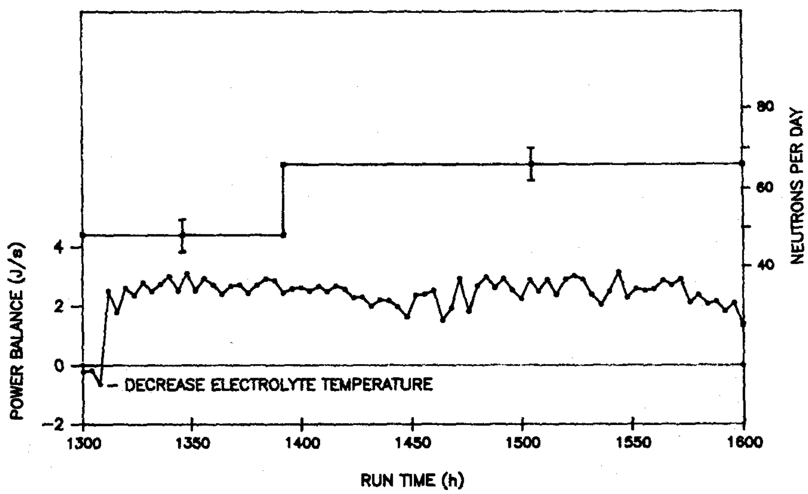


FIGURE 18.2 Scott, ORNL, reported anomalous power and neutrons. Neutrons are depicted as their average value. Excess energy is shown as the saw-tooth shaped tracing.

1,400 hours continued at least until 1,600 hours. He reported that, "Apparent increases in the neutron count rate were also observed in several instances, including one period that is coincident with induced excess power." By "induced," Scott meant that he could cause changes in excess power by wiggling certain operating parameters such as the current level.

There were at least four groups that practiced cold fusion experiments at Texas A&M University. Dr. Kevin L. Wolf, professor of chemistry, a nuclear chemist of solid reputation who worked in the Cyclotron Institute there, led one of these groups. His experiment had three identical, palladium/lithium-heavy water cells wired in series so the same operating current passed through all three.[4] It ran for six weeks during the months of August and September 1992.

A new protocol called for the cathodes to be removed and sanded during a fifteen-minute intermission every seven days. Some boron and aluminum were added to the electrolyte on the 18th day.

Like Takahashi, Wolf was an expert at neutron detection. On September 7, 1992 his experiment produced the neutron flux shown in Figure 18.3. Wolf's experiment also produced the remarkable result that all three cathodes became mildly radioactive. He also detected gamma rays. Both are shown in the figure. Within five hours the neutron count moved from a background level of about 24 counts per hour (cph) up to 150, then leveled off at near 100 cph. At 21 hours, the neutron count returned to the original level and stayed there for the remainder of the experiment.

In a contrary manner, the gamma ray count stayed at background level

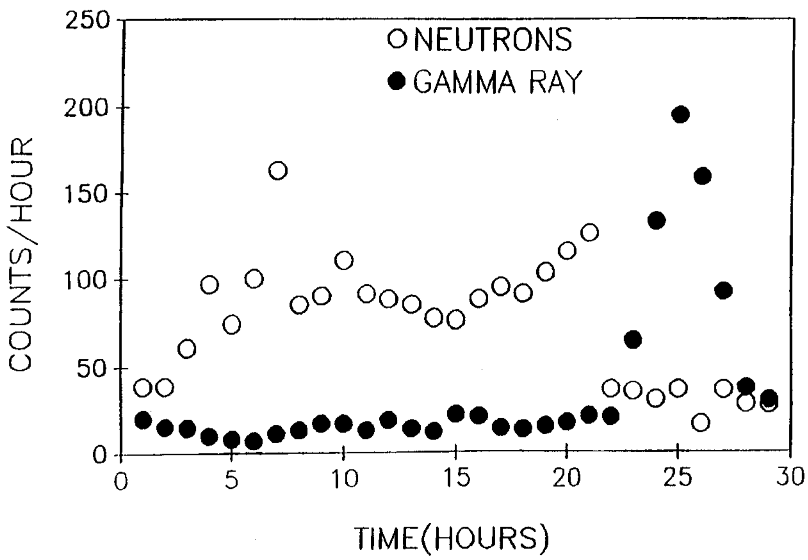


FIGURE 18.3 Wolf, Texas A&M, detected neutron and gamma emissions from Pd/LiOD cells.

until that same twenty-one-hour moment when it soared to near 200 cph and returned within a six hour period. All three cathode rods experienced a similar result.

Professor Akito Takahashi, Department of Nuclear Engineering, Osaka University, Osaka, Japan, an experienced hot fusion scientist, announced an experiment in the spring of 1992 that emitted a great deal of excess heat and low levels of neutrons at the same time. His expertise in nuclear measurements allowed him to obtain a substantial record of the nuclear emissions. The number of nuclear products was too few to account for the amount of heat by a factor of more than a billion. In fact, Takahashi found that increased heat output was accompanied by decreased neutron count.[5]

Neutron counts, Figure 18.4, were separated from background noise by the experimental technique used. The cell current was cycled high to low, changing every six hours, and it can be seen that Takahashi's neutron count followed the cycling cell current in an up-down fashion.* The triangles give the count of the number of neutrons detected during high-current, six-hour periods, and circles give the count during low-current periods. The left-hand

* The experimenter must be careful, of course, that there is no electric or magnetic coupling between the cell excitation current circuitry and the nuclear particle counting system.

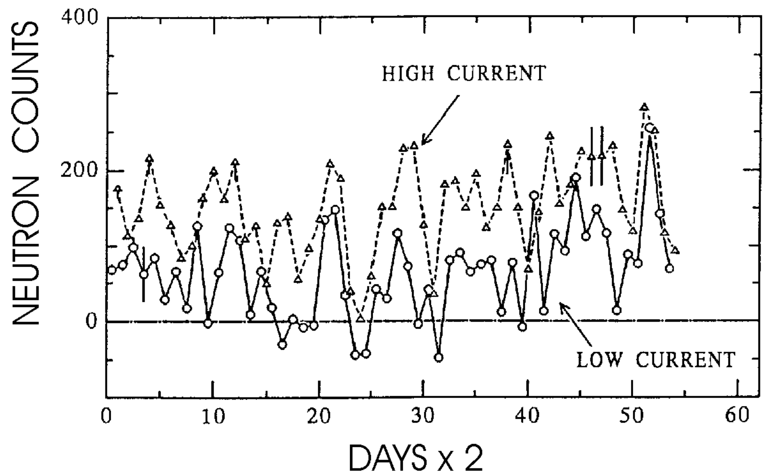


FIGURE 18.4 Takahashi, Osaka University, cycled the cell current up and down and detected the neutron count following up and down.

scale is the count after subtracting away the remainder background count. Error bars associated with this figure may be found in the reference.

Takahashi explained that during the first years he noticed only a small neutron signal, but after two and a half years of experimentation he obtained significant results. The typical duration of one of his electrolysis experiments lasted from two to four months, although Figure 18.4 shows only a 30 day portion. Takahashi has difficulty reproducing these results as did others working with complicated experiments.

In 1994, Dr. M. Srinivasan reported on an experiment performed at the Bhabha Atomic Research Center, Bombay, India. A large commercial electrolytic cell with palladium cathodes was operated at 30 amperes current for two months. Surrounding the cell were 16 neutron detectors that registered bursts of neutrons, as well as the total count of neutrons.[6]

Figure 18.5 shows the average of the total number of counts recorded each day. The cell was inserted into the neutron detecting array on day 17. On day 46, the cell's heavy water was replaced with light water. With continued operation, the deuterium inside the palladium was gradually replaced with hydrogen. This caused the neutron count to slowly fall off to background level.

It was also reported for the interval from day 16 to 46 that about 6% of the neutrons came in bursts of from 20 to 100 neutrons each. During the first 16 days there were no bursts of over four neutrons. The authors concluded that neutron emission from an electrolytic cell was observed, and that a small fraction of these neutrons were emitted in bursts.

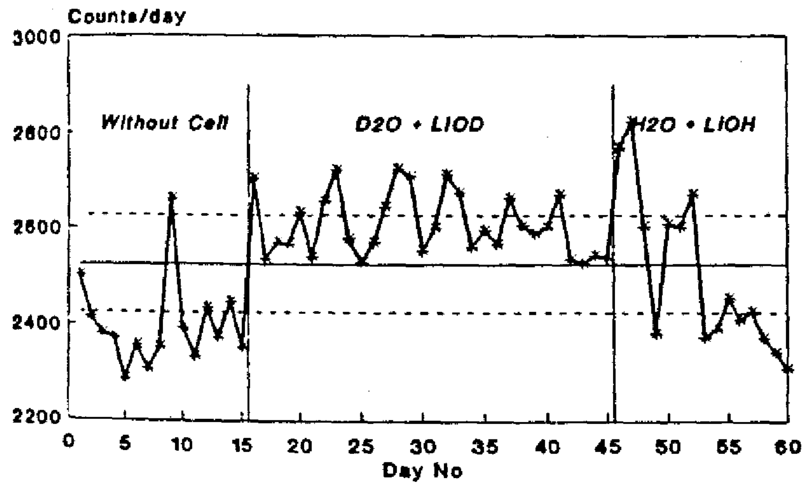


FIGURE 18.5 Srinivasan, Bhabha Atomic Research Center, presented neutron counts and neutron bursts in an electrolytic cell. Changing the electrolyte from D_2O to H_2O caused the count to fall away.

Neutron Bursts

Tadahiko Mizuno, Hokkaido University, in association with Akito Takahashi, Osaka University, determined that the heat generating reaction required more than just the absorption of deuterium into the palladium cathode. A survey of experimental results in the literature showed that light hydrogen absorbed after a period of heavy hydrogen loading was the likely candidate.

The experiment used a palladium rod 1.5 mm diameter and 150 mm in length in an electrolyte made with K_2CO_3 that had been baked at 300C to remove all traces of (hydrogen containing) moisture. The preparation of two flasks provided a quartz flask with extra pure heavy water electrolyte and a Pyrex one with light water electrolyte. Each contained an anode of platinum mesh. The cathode was electrolyzed in the quartz flask at a high current for three hours, and then transferred to the Pyrex flask for further electrolysis.[7]

The instrumentation consisted of three neutron detectors placed 50 cm above and apart from the cell.* The neutrons emerge from the experiment in all directions, of which the detectors will intercept only a small fraction, a number designated as the efficiency of the detectors, in this case 4×10^{-5}.

* The detectors were helium-three type neutron detectors aligned in a row. After calibration with a Cf252 neutron source, one of them was covered with a cadmium sheet to provide anti-coincidence noise reduction.

The detectors were enclosed in a shield to avoid pickup of electromagnetic noise.

Large emissions of neutrons were observed in five out of ten runs of the experiment. These occurred in bursts that continued for several minutes after a triggering event such as a rapid voltage change. A typical burst sequence is shown in Figure 18.6. The total emission for this event was estimated at 1.57 $x10^6$, or one and one-half million neutrons. The figure shows that the experiment enjoys an excellent signal to noise ratio.

The ten experiments are listed in the table of Figure 18.7. Column three gives the cell voltage before neutron emission occurs. Column four is the voltage after the emissions cease, or at the end of the experiment. Columns five and six show the duration of voltage ramp and the total voltage boost, with the ratio in column seven. The remainder columns describe the resulting neutron emission. Half produced no neutron emission, a reproducibility wholly acceptable for much scientific work.

The table shows that bursts occur after a voltage increase or at the start of electrolysis. Neutron emission varied over an order of magnitude, from 100,000 to 1,000,000, and their duration from 2 to 200 seconds. All emission events started with a high peak and decreased from there to the end. Conclu-

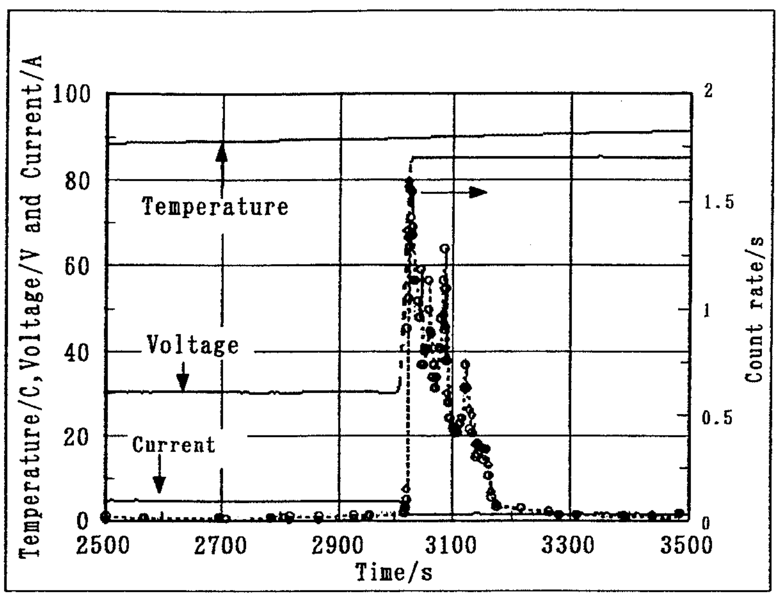


FIGURE 18.6 Mizuno shows a typical neutron burst from the experiment using deuterium followed by hydrogen electrolysis. More than 100,000 neutrons were generated during the 200 second burst.

Sample No.	Cell Temp. (℃)	Starting Voltage	Ending Voltage	Duration of boost (s)	Voltage Boost (V)	Voltage change (V/s)	Peak Count	Total counts	Duration of Burst (s)	Count rate (c/s)	Total neutron burst
No.1	40	0	40	15	40	2.67	7.70	17	50	0.17	438600
No.2	26	0	30	10	30	3.00	3.08	5	2.6	1.92	129000
No.3	40	0	25	5	25	5.00	0	0		0	0
No.4	90	30	83	20	53	2.65	1.54	61	200	0.305	1573800
No.5	95	50	90	40	40	1.00	0.05	3	100	0.03	77400
No.6	90	40	90	20	50	2.50	0	0		0	0
No.7	90	70	90	15	20	1.33	0	0		0	0
No.8	90	20	90	40	70	1.75	0.910	5	135	0.037	129000
No.9	60	0	0	200	0	0	0.025	5	200	0.025	129000
No.10	80	72	92	15	20	1.33	0.460	1	195	0.005	25800

FIGURE 18.7 Mizuno lists his ten experiments to show that five gave neutron bursts and five had null results. these reproducible results indicate that hydrogen (^{1}H) is involved in the nuclear reaction.

sions are that the experiment is highly reproducible and it generates a high level of neutrons. To quote the report, "the [theoretical] models proposed heretofore based upon D + D reactions are inadequate to explain our present results, which involve hydrogen nuclear reactions."

It is clear that the detection of neutrons at low levels was as fraught with difficulties as was the measurement of heat power. Only a small fraction of the neutron measurement reports are included here.

Can the kinds of reports offered in this section show conclusively that the Fleischmann and Pons cell produces neutrons? The answer is No. It will take a large experimental effort to establish conclusively whether there are emissions of neutrons from the cold fusion cell. What can be learned from the effort to date is that in these experiments there is much interesting science to be explored.

CHAPTER NINETEEN

Gamma Rays and Transmutation

The experimental results presented in this chapter represent a considerable difference from the Fleischmann and Pons result shown thus far. They lead to claims for extensive elemental transmutation of the materials initially placed into the experimental vessel. That is, there is experimental evidence for nuclear reactions involving heavy nuclei that are much more complex than fusion of two light nuclei, such as deuterons. With one exception, the reported results have the singular characteristic that the resultant elements are not radioactive.

My principle rationale for including the material in this chapter is that two independent experimenters on two continents have produced results that are astonishingly similar. Therefore, their work is worth including along with some appropriately cautionary words.

Research into the transmutation of elements is redolent of alchemy, the chemistry of the middle ages with its goal the transmutation of lead into gold. The rise of the nuclear age in the twentieth century made such transmutation realizable (though expensive) through the use of large accelerators. That enticing word—*transmute*—has emerged again in the field of cold fusion research. Nuclear change of any sort is of interest as a possible clue to the source of the heat energy. The electrolytic cell shows recurrent evidence of elemental change among light nuclei, although the word *transmutation* is generally applied to heavy nuclei.

Intimations of transmutation were present when the topic emerged at the first cold fusion conference in March 1990. Huizenga speaks of an early encounter with it: "Fleischmann told me that he thought the unknown nuclear process was fission of the palladium." Huizenga did not respond to Fleischmann at the time, but later wrote:

> The answer startled me. Fleischmann was, no doubt, unfamiliar with the fact I had researched nuclear fission for decades and had coauthored a book . . . on the subject. Knowing that the energy threshold for palladium fission was several tens of millions of electron volts, I had to conclude that Fleischmann was either joking (although he appeared to answer in all seriousness) or exposing gaping holes in his knowledge of nuclear physics.[1]

This is the kind of comment that is skeptical rather than critical. Huizenga was not sufficiently curious to ask to see the data that had led Fleischmann to his supposition.

Dr. Wolf's experiment of September 1992 is described in the previous chapter. The three cathode rods were examined with a gamma spectrometer after the experiment was complete. Figure 19.1 shows a portion of that spectrum, from 295 keV to 574 keV, including peaks that ordinarily indicate the presence of isotopes of rhodium, silver, ruthenium, that were not present prior to the experiment.[2,3] The interested reader will have to turn to the references to see the entire spectrum in five frames. Gamma ray spectrum from the three cathodes were similar to within a factor of three in magnitude.* The most active one is shown.

Wolf never wrote up this experiment for publication because he was always waiting first to replicate it, but that never happened. With his death in 1998, its record will necessarily remain incomplete. What we have of it was made available from the project's files by Dr. T. O. Passell of EPRI, the sponsor of the study. Wolf and Passell consider this gamma spectrum taken in its entirety to be evidence that element transmutation occurred in the three cells of the experiment.

Cold fusion experimentation was widely practiced during the 1990s both in Russia and in some of the former member states of the Soviet Union.[4] Alexander Karabut, of the Scientific Industrial Association "Luch," Podolsk, Moscow Region, published a number of papers with Y. R. Kucherov and I. B. Savvatimova, as well as the one upon which this report is based, under his name alone. The work was done in the glow discharge type of experiment that was discussed earlier by Claytor in Chapter 17, p. 249.

In experiments that confirmed his earlier work, Karabut generated excess heat using deuterium, hydrogen, and argon gas with various cathode materi-

* Gamma rays are high energy electromagnetic waves emitted by a nucleus when it undergoes rapid evolution.

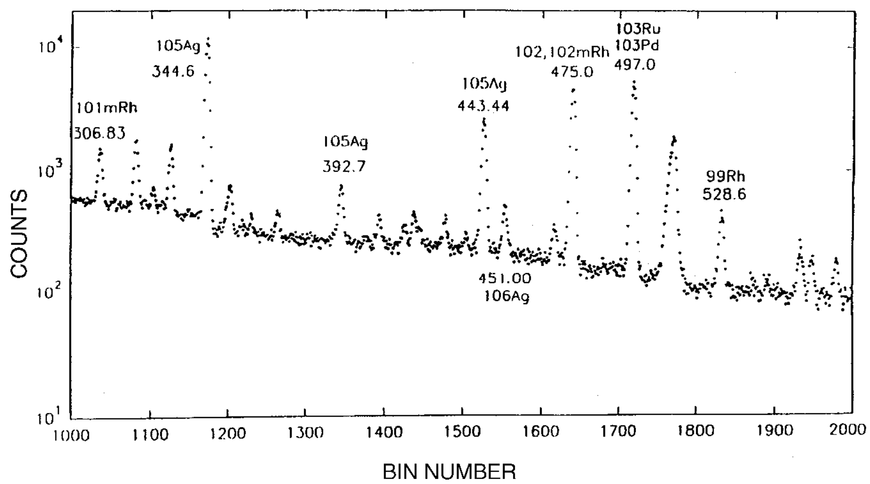


FIGURE 19.1 Wolf (via T. Passell) reports a portion of the gamma spectrum of one of his electrolyzed cathodes that covers the energy spectrum from 295 keV to 574 keV. The experiment proved unreproducible.

als. Some of the combinations used were palladium-deuterium, palladium-hydrogen (with apparently no excess heat), nickel-hydrogen, nickel-deuterium, silver-deuterium, and niobium-hydrogen.[5]

In the case of the palladium-deuterium experiment, Karabut reports the production of a variety of stable isotopes that appear as new "impurities" after the glow discharge period. By "stable" it is meant that the produced elements are without residual radioactivity.

In Figure 19.2, the variety of new elements is displayed by the atomic mass number (amu). The vertical axis indicates the measured approximate number of atoms created during the experiment. Karabut further reports that the quantity of impurity corresponds to the "complete value of excess heat."

He reports further that radioactive nuclides with a low level of activity are formed in many of these experiments. A much higher level of radioactivity is detected during the experiment, but when it ends that activity disappears with the formation of stable nuclides.

Dr. Tadahiko Mizuno, Division of Quantum Energy Engineering, Hokkaido University, Japan, did yeoman work in the cold fusion vineyards over the nine years to 1999.[6] He obtained some of the most interesting results in the field.[7] For example, he measured excess power in proton conductors and those measurements were corroborated by Dr. Oriani at the University of Minnesota.[8]

Mizuno's evidence for the transmutation of atomic elements was shown in a September 1996 report, and is reprinted as Figure 19.3. He ran an elec-

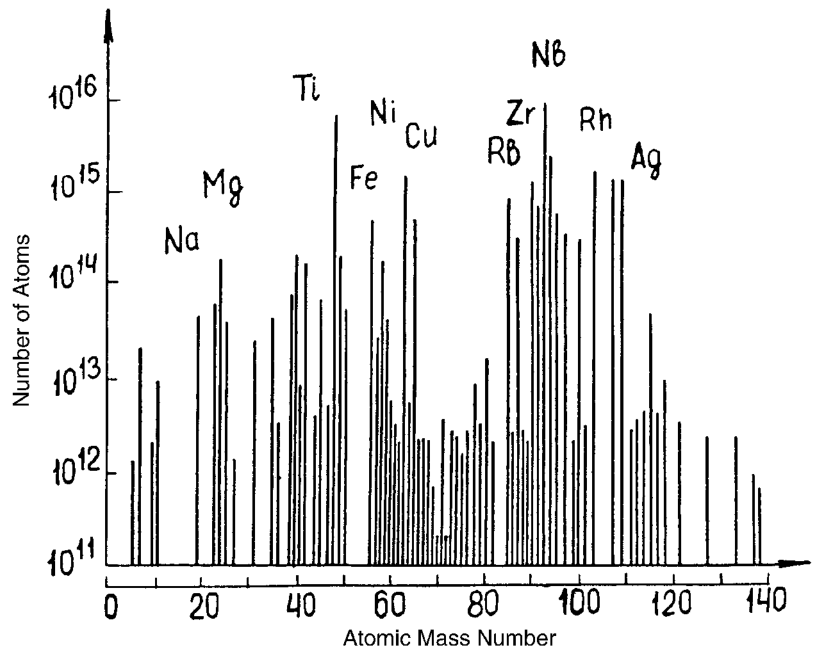


FIGURE 19.2 Karabut reports on the stable isotope "impurities" that appear in a palladium cathode after a glow discharge experimental run.

trolytic cell at high temperature, high pressure, and high current continuously for one month. He used 99.97% pure palladium and distilled the heavy water to reduce the presence of contaminants. Four different kinds of elemental analysis were employed to examine the cathode and electrolyte after the run.[9]

Evidence for the evolution of platinum (Pt), chromium (Cr), iron (Fe), and copper (Cu) occurred near the surface of the palladium cathode to be displayed clearly in the before and after tracings of Figure 19.3. The signal level amplitudes were 10 times larger than the background in some cases. The element concentrations remained much the same after removing one micrometer of the cathode's surface. Large differences from normal were observed in the isotope ratios. In naturally occurring copper, the ratio of copper mass 63 to mass 65 is about two to one. In one experimental sample, the copper consisted entirely of mass 63 isotope with no copper of mass 65.

Mizuno's results produced from several different kinds of instrumentation showed that the evolved elements were distributed roughly into three groups by atomic mass: 20 to 28, 46 to 54, 72 to 82, as shown by the triangle markers in Figure 19.4. Many of these had isotopic distributions substantially different from the naturally occurring element.[10] Mizuno concluded that the nuclear changes occurred during the electrochemical process. He reported that

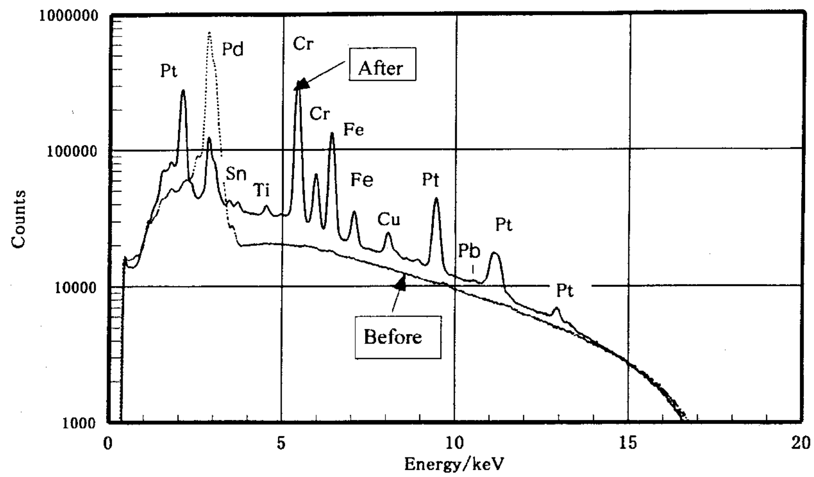


FIGURE 19.3 Mizuno, Hokkaido National University, reported that by using x-ray spectroscopy, there appeared evolution of platinum (PT), chromium (Cr), iron (Fe), and copper (Cu) present in the "after" scan that were not present "before."

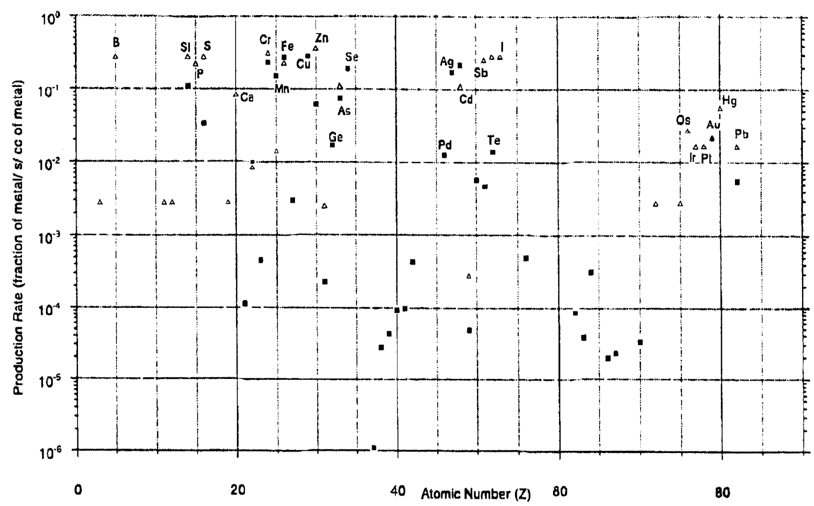


FIGURE 19.4 Miley (■), University of Illinois, and Mizuno (▲) reported a rate of generation of the several elements. Mizuno's rates are arbitrarily normalized to Miley's. Note the elements Si, Cr, Fe, Zn, As, Cd, Sb, and Pb.

five of these experiments were performed with similar results for each run so they appeared to have a reasonable degree of reproducibility for the purpose of scientific evaluation.

James Patterson, a chemist who spent his career at Dow Chemical Company, has provided scientific and strategic guidance for Clean Energy Technology, Inc., (CETI), Sarasota, Florida. He had learned how to coat palladium over tiny plastic beads (spheres) that he had developed for many processes some years before. He was author of 16 U.S. patents on bead and electrolytic technologies.

Patterson developed his version of the Fleischmann and Pons experiment built on that technology. He designed a cell where the nickel-coated balls were used as the cathode in place of the usual rod of Pd. As many as a thousand beads constituted a cathode. These experiments used a light water (1-molar L_2SO_4) electrolyte solution. The cell required 60 milliwatts of electrical power for excitation and yielded about 500 milliwatts of additional power, or 1 watt per cm^3 of reactive volume.

The electrolyte in his cell flowed continuously in a loop. It passed over the cluster of balls that made up the cathode, past a platinum anode, around a plumbing loop including a filter, a reservoir, a pump, and back to the cathode. The anode and cathode were electrically driven by a power supply. Substantial generation of anomalous power was reported in technical papers and demonstrated to the satisfaction of a number of observers.

George H. Miley, Professor of Nuclear and Electrical Engineering at the University of Illinois, Champaign/Urbana, edited three well-known professional journals (one for the Nuclear Society of America and two for Cambridge University Press) in addition to his teaching and research activities. Winner of the Edward Teller Medal, he is well known for his innovative contributions to hot fusion research.

The concept of fusion at room temperature caught his attention early, and he was one of the scientists who testified at the congressional hearing on the subject. The possibilities for transmutation interested him from the beginning as a means of understanding the physics of the nuclear reaction.

Miley experimented with thin films (0.1–200 nanometers thick) of nickel, palladium, and titanium. Thin films offer an experimenter the advantage of fast take-up of deuterium into the metal, minimum cracking, and the potential for a more complete analysis of the results. All the metal in the form of a thin film can readily be made available for post-experimental analysis, unlike the case with a cathode rod, where the bulk material must be sectioned prior to the use of most analytical techniques.

In 1994, Miley began working with Patterson using his cell. Miley and colleagues extended this concept to a form that used thin sputtered films on the beads as opposed to the original, thicker electro-deposited type used by

Patterson. The thin film coating on the beads consisted of only one metal, nickel (or Pd or Ti), for a particular experiment. While single coatings are less reactive, they greatly simplify the analysis. Miley reported having obtained reasonable, consistent results with approximately 20 runs, some of which were duplicate checks but many of which explored new metals or configurations.

The transmutation yield of an isotope was defined as the ratio of the fraction of an isotope in the thin film after the run to the initial amount present in the film plus electrolyte and other cell components.

Miley reported that during electrolysis a significant degree of atomic element transmutation occurred. The trends (e.g., yields versus mass numbers) he found were shown to be generally consistent with the results of Mizuno. Miley was able to provide absolute reaction rates.[11] Some elements exhibited large "yields," (e.g., 500 and 10 for copper and silver respectively). This result, plus measurements of the isotopic composition showing non-natural ratios, were cited among the evidence that the "new" elements were not due to impurities.

The results of the Mizuno and Miley investigations are compared on the chart, Figure 19.4.* The horizontal axis plots the atomic element number. The vertical axis is the rate at which the elements were produced per second during the experimental run as a fraction of the total amount of material. Mizuno's numbers are shown with triangles (▲), and Miley's numbers are shown with squares (■). Newly produced atomic elements appear bearing element numbers between three and 85.

Miley finds that in one five-week "nickel" run (with nickel coated beads for the cathode), 40% of the total amount of nickel in the cathode was changed during electrolysis into other elements. In Figure 19.4, (after rotating the page ninety degrees) each kind of atomic element lies along its own vertical line. Its production rate is shown on the left axis where each vertical increment represents an increase of ten times in the rate of element production. Notice that between atomic numbers of 24 and 34, chromium (Cr, atomic number 24), iron (Fe, 26), copper (Cu, 29), and selenium (Se, 34) appeared. Silver (Ag, 47), cadmium (Cd, 48), antimony (Sb, 51), are near atomic number 50 and the highest group includes lead (Pb, 82). Miley claims that the amounts of these elements observed was well above the amount contained in the electrolyte and cell components before the run began. The juxtaposition of Miley's and Mizuno's data in this figure indicates that there is a degree of corroboration of results between their experiments.

Miley also found in some instances significant isotope ratio changes from those of naturally occurring elements. He showed related results for other metals in later papers. He has proposed that such experiments result in charac-

* In this plot Mizuno's rates are arbitrarily normalized to Miley's using the value for copper.

teristic "signatures" (such as the high-low yield regions in Figure 19.4) which must be explained by any theory. He points out also that these new elements can be used to develop an understanding of the energy balance that leads to the production of anomalous power. By multiplying the grams of each new product observed by its respective bonding energy and subtracting the same quantity of reactants (protons and metal), he predicts, a posteriori, power outputs of the same order of magnitude as was measured.

What is to be made of these reported nuclear transmutations? The best that can be said is that they give the appearance of being created and caused by the operation of the cell.

The low energy nuclear reactions (LENR) hypothesis is, of course, heresy in orthodox physics. These data, if borne out by further work, would prove revolutionary. But the experiment is complicated, time consuming, and expensive. One cannot repeat it twenty times during a long weekend to ensure uniformity of result. Also, it is not clear that this work ought to be considered a continuation of the Fleischmann and Pons phenomenon. Miley takes pains at times to disassociate his work from that of the two chemists. It may well prove to be something quite different, or it may be merely a mirage and in due course disappear in the fashion described by Fleck in Chapter 11, p. 153. We will have to wait for some time to see if what is happening here becomes the genesis and development of a scientific fact.

Radioactivity Remediation

The nuclear elements that result from transmutation in the experiments described above display little or no radioactivity according to published reports. The LENR viewpoint holds that this is due to the long time interval during which the nucleus settles into its final state. No further settling takes place in the nucleus after the new elements emerge from the reaction. It has been suggested that radioactive elements now in the national inventory (nuclear waste) might be subjected to this transmutation process thereby preempting their projected radioactive lifetime. This highly speculative concept is called radioactivity remediation (RR).

To exploit this possibility, the entrepreneur has two approaches to RR. Either develop a commercial product to process radioactive materials or offer customers a consulting service in RR technology. Both have advantages and disadvantages as commercial undertakings. The first plan requires development of a device that treats radioactive materials by some sort of transmutation processing. At least two organizations have offered a RR device for experimental use. They have experienced widely varying degrees of acceptance from the initial users.

The other means for commercial entry into RR work is to offer technical consulting services to those who were grappling with the problem of America's backlog of radioactive waste. This course ought to be much less risky financially, and is a well proven way to develop a new technology without depending upon large new capital infusions. The customer participates fully and is in a position to expand or contract the level of participation as laboratory results warrant. It is a highly public arena in which to bring forth something new, so it is not clear whether the outcome would be dependent upon technology or politics.

Clean Energy Technology Incorporated (CETI), Sarasota, Florida, offered its anomalous power cell design as an experimenter's kit in 1995 and sold a number of them to sophisticated experimenters. It was one of their kits, or a homemade version of it, that Miley used for his transmutation experiments. CETI had to withdraw the kit offer when buyers found it necessary to burden the company with endless consultations. Their plan is to develop commercial products from that cell technology after further development efforts.

The Cincinnati Group, as they call themselves, are evangelical Christians who do not hold separate their lives as Christians from their work. None of them have degrees in science or engineering, but one has had training as a chemical laboratory technician.

They appeared in 1996 with a small device that they claimed performed the RR function. It was a two-inch cylinder three inches long and made of stainless steel and zirconium metal. It could be opened up to receive the radioactive material in the form of a conductive liquid electrolyte. The device was then sealed tight. Insulation along certain edges of the container caused an applied electric current to flow through the electrolyte. A sufficient voltage was applied to drive current through its contents and heat up the cell. They claimed that the solution's radioactivity was greatly reduced after an hour or so of this processing because the radioactive elements were transmuted into stable elements. They have continued in the process of demonstrating it and asking knowledgeable scientists to evaluate it. A couple of respectable laboratories have endorsed their device after working with it for a while.

The reader is warned that the material in this chapter is highly speculative. The research that produced the reported results is difficult to carry out, this is especially so if an attempt is made to thoroughly analyze the materials involved before and after in the experiment, and much of it is done with marginal budgets. A reasonable attitude for the observer to take towards it is simply to watch how it works out. Miley and Mizuno are serious and established experimenters and, as such, one hopes to see their work continue to be funded and their results published.

My presentation of technical data that resulted from evaluation of the

Fleischmann and Pons announcement and claims now comes to an end. It would constitute an oversight, however, if I did not offer some inkling to the reader of how theorists grapple, or fail to grapple, with the "problem" excess heat, helium generation, tritium generation and, especially, transmutation present to nuclear science. Some of the theorists, of course, assume that all the data were in error. But as we have seen and will see, a few address themselves vociferously to resolve the matter.

CHAPTER TWENTY

Theoretical Musing

This chapter about musing touches lightly upon the possibilities of what might be happening in the nuclear realm during generation of anomalous power. Dozens of models of various nuclear reaction schemes have been explored with elaborate prose and, in some cases, detailed calculations. None has yet rendered an answer to identify the source of anomalous power.

Fleischmann and Pons hypothesized an unknown, low energy, nuclear process to provide the anomalous energy for their experiment. Fleischmann augmented this hypothesis one year later by bringing to the attention of science something it had forgotten. Philip I. Dee reported in the Proceedings of the Royal Society, 1934, evidence for a nuclear reaction initiated at a low energy level. Using a cloud chamber, many of the observed tracks emerged after the reaction at 180 (geometric) degrees, a result that implies the colliding deuterons had little energy. To quote Dee, "this no doubt being the result of transmutations effected by slower [deuterons] which have lost energy by collisions in the target."[1]

The theoretician who elucidates the low energy nuclear reaction process, if indeed there is one, will qualify for considerable scientific recognition. Dr. Scaramuzzi estimates that, ". . . it is not possible to explain [cold fusion] on the basis of two-body interactions. It is necessary to demand the existence of a collective and coherent mechanism governing the phenomena."[2]

The theoretical search for an unknown or unrecognized nuclear process began immediately after the Utah announcement, as one might expect. A few scientists were willing to take the risk that Fleischmann and Pons were right about their measurement of significant amounts of anomalous power, even if they were not necessarily right about their nuclear measurements. These theoreticians assumed that the power measurements were true and the hypotheses of a nuclear process to power it followed rationally from the data. Moreover, we have seen from Beveridge that there was a good chance that the elucidation would be accomplished by someone from outside the field of nuclear physics. Beveridge says,

> The scepticism with which the experts nearly always greet these revolutionary ideas confirms that the available knowledge has been a handicap.[3]

The musing process is one of mental speculation. Ideas about possible low energy nuclear processes that at first glance seem plausible are vigorously evaluated. The critical question is whether such an imagined process can actually take place in accordance with physical law and produce the effects seen in the laboratory. That question requires calculation of the reaction rate of the process, the number of nuclear events per second, and the energy release of each. Typically, the theoretician is looking for something like a billion or more events per second for the imagined process to be consistent with the anomalous power observed, and thereby warrant further study.

At the time of the Utah announcement, Peter L. Hagelstein was engaged in research at the Lawrence Livermore National Laboratory (LLNL), Livermore, California, where cold fusion immediately became a favorite topic. It is surprising that he is not a physicist, at least not officially. His permanent appointment is that of professor in the Department of Electrical Engineering and Computer Science at MIT, and his occupation, in this case, is that of *applied* physics. Hagelstein soon announced that he was filing four patent applications, each involving ideas of what might be the nuclear source of the excess heat. He gave a seminar at MIT to explain these ideas to his colleagues. His ideas were outside the boundaries of conventional theory as might be expected and warranted if one considers the circumstances. Taubes reports that the provost, John Deutch, who sat in on the seminar, referred to those ideas as "malarkey."

Hagelstein is a disarming fellow. After he had discussed his first thoughts with Koonin, who was critical of them, the Caltech professor of theoretical physics reached the following conclusion.

> Well, I had an interesting two days last weekend [May 6–7, 1989] when I talked to Peter Hagelstein, the guy at MIT who came up with this much trumpeted theory. . . I was talking with him for a few hours (other people were there as well). Basically, he conceded that

> it was all nonsense. He's a nice guy; I'm sorry he got caught the way he did.[4]

Hagelstein, of course, did not "get caught" at all, but he did give that impression to Koonin, as he has done also with other physicists. Some might say that he gives the impression of being a misplaced hayseed from the corn belt. He can do that, at least until he has something to say. At this writing, he is co-author of a mathematics textbook on Applied Quantum and Statistical Mechanics that includes a derivation of the equations of quantum electrodynamics, a possibly important subject for cold fusion research.*

From the beginning, Hagelstein accepted the existence of anomalous power in the Fleischmann and Pons experiment as a new scientific observation. He made the assessment that the two chemists from Utah were competent scientists in the measurement of heat (flow). He announced in his seminar that four articles had been submitted to the Physical Review Letters journal for publication (they were never published). Hagelstein continued to develop his theoretical positions during the ensuing decade. In the give and take of scientific contention, many of his ideas died a brave death, but others, braver yet, arose in their places.

Early in this work, Hagelstein dismissed the possibility that electrically charged bodies, protons or nuclei, could be directly involved by means of a two-body fusion reaction to cause the observed low level of energetic neutrons. Conventional fusion occurs when two nuclei collide at such high energies that their velocity overcomes the repulsion of the mutual positive electrical charges of the nuclei. The cold fusion phenomenon, however, seemed to be functioning at a low energy level.

It is necessary to insert into a nucleus a sufficient amount of energy to cause a nuclear reaction to occur. The nucleus recognizes when it has too much energy and reacts in the ways available to it: emit a gamma ray, emit an electron particle (beta emission), emit an alpha particle† (alpha emission), or fission into two separate nuclei.

It was two years before Hagelstein hit upon a theory whereby energy might be gradually transferred between the crystalline structure (the lattice) of atoms and the nucleus. Most conventional physics and chemistry have always assumed that such energy transfers were insignificant and that the behavior of the nucleus and the lattice were independent of one another.‡ However, there

* The other authors are S. D. Senturia and T. P. Orlando. It is to be published by John Wiley & Sons (New York).

† The alpha particle is the nucleus of the common type of helium, helium four: it has two protons and two neutrons, giving it an electrical charge of +2.

‡ The Mössbauer effect is one of a few exceptions to this rule, along with hyperfine structure in the optical spectra, isotope effects on x-ray spectra, and so forth.

is a class of nuclei called quadrupole because of their asymmetrical structure. If atoms with quadrupole nuclei make up a crystal, and if that crystal has many vacancies (missing atoms that leave behind vacant places), then phonons* might be able to couple some amount of energy into or out of those nuclei.

Hagelstein investigated this possibility assiduously. He visualized his way through the physics of the atomic structures and worked out the mathematics. He computed the possible reaction rates. He was looking for effects large enough to cause the heat release that was observed in the experiments. What he found were conditions where a small change in the frequency of the phonons represented a large change in their energy content. He went looking, in the physical structures of the atoms and lattice and in the physical laws, for a way to force a phonon frequency change.

If a nuclear event were to cause their frequency to increase, this would mean the phonons had, in this instance, absorbed energy. There might be conditions under which significant amounts of energy could be added to the lattice with small frequency changes of large numbers of phonons.

Hagelstein concluded that,

> the major outstanding theoretical problem is *not* how to elude the Coulomb barrier; and it is *not* how to account for anomalously low neutron emission rates. The big issue is, and always has been, whether a large energy quantum can be transferred efficiently from a nuclear reaction to new degrees of freedom associated with the surrounding environment. This is true not just for heat production, but also for tritium production, gamma emission, charged particle emission, and induced radioactivity. Consequently, our interest must be focused on the problem of anomalous energy transfer.[5]

This then, was the area of his interest and research.

In this way, a theoretical physicist tries to bring understanding to experimental results. Hagelstein has offered predictions of something that the experimental scientists should look for. To a limited extent, he was successful in adjusting his theory to explain the experimental data. He advises the experimenter what to watch for when his theories predict some aspect of performance. But his predictions have not yet borne fruit. He has yet to understand the essence of low-energy cold fusion reactions.

* Phonons are mechanical vibration waves that are always present in solid materials. These vibrations are interpreted by scientists as discrete particles called phonons that abide by the rules of quantum mechanics.

A Musing Process

In the LENR literature, speculation about possible mechanisms is voluminous. An exposition of the many different nuclear processes hypothesized as cold fusion energy sources is beyond the purposes of this book. Nevertheless, an example of what has been tried is worth examination so as to see how such theoretical contemplation helps to advance the art by a process of trial and error.

There is an interesting duality at work in this activity. The musing process gets forced along by the intellectual challenge to the theorist, and the experimenter's craving for guidance: what best to try next? The theoretician is looking for a nuclear mechanism whose effect matches the experimental data. The mechanism can then be studied for properties that could be exploited in the laboratory. In this way, there is a mutual give and take between the theoretician and the experimentalist.

The theoretician's first and most difficult step requires selecting those experimenters whose data were to be accepted as valid, a somewhat subjective task. Who are the most careful experimenters? Who best understands how his experiment worked? Who is of sufficient intelligence and character that their data from limited experiments ought to be accepted as valid? In this fashion a list is made up of the data that a new theory must explain. The data deals with the lack of, or the generation of, heat, helium, tritium, neutrons, radioactivity, radiation, and possibly element transmutation. The theorist also notices whether these items came alone or in combinations. This sorting process gives the theoretician the requirements for a new theory to meet if it is to explain the data. Such a theory, if one is found, may then predict other effects. In that way, there is a test for the theory: do those other effects exist?

The atoms in a metal, or other material, are commonly arranged in the orderly rows and columns of a lattice. Lattice structure is studied in the specialty of condensed matter (solid state) physics. The electrolytic cell uses a palladium metal cathode that is composed of tiny domains each of which is a crystal with a lattice structure. It is in this structure of crystal domains, with their interfaces between domains, pressed together into a solid piece of metal that one looks for an understanding of the power source. One of the theorist's conclusions is that in a perfect lattice, with the atoms in their places, *there would be no low energy nuclear behavior.*

Palladium atoms are relatively big and heavy. The particular way that atoms arrange themselves in the palladium crystal allows a small atom like that of deuterium to be added to the crystal's interstices. The deuterium atoms fit nicely among the larger atoms without disturbing them too much. Deuterium could be added, if one knew how, until there was almost one deuterium atom for each palladium atom (the loading ratio).

One type of lattice imperfection is the presence of vacancies, places where the large atoms are missing from the ordered array of atoms. Ordinarily in metals there are only a few vacancies, a small fraction of one percent. The possibility for a new reaction might be greatly increased if the number of vacancies could be increased to three or four percent. The dual atomic mix of two sizes of atoms, along with the vacancies, might allow the nucleus of a large quadrupole atom to be exposed to the influence of events in the lattice.

Phonons are always present in solid materials. Their movements contain and can convey energy from one place to another. Phonon lasers, for example, resonate at a single frequency and with great amplitude because of reflections from opposite crystal boundaries. Some theorists conjecture that phonon activity may cause small packets of energy to be transferred into the nucleus of some of the large atoms. The nucleus could be excited in this way to an energy level that would result in a nuclear reaction.

The energy transfer from phonons to the nucleus may happen in tiny quantities (a quantum), but at a frequency of possibly 12,000,000,000,000 (or 12×10^{12}) times a second. The nucleus might become sufficiently excited by transfers that continued for five or ten minutes for this process to result in a nuclear event. The nucleus would become increasingly agitated by the steady increase of energy until it reached an energy level where it reacted. A reaction might result in an alpha particle (helium), or a beta particle (electron) emission, or, with sufficient excitation, the nucleus might fission. This process is called low energy excitation, and the resultant reactions are a category of low energy nuclear reactions.

The heavy nucleus (Pd) remains in an under-excited state for an extended period that lasts for minutes before producing a reaction.*

There was no theoretical structure early in 1996 that could explain the detailed observations of many cold fusion experiments: helium-four in amounts that produce the reported excess heat, low energy tritium, only marginal levels of radioactivity, and so forth. The theorists suggested that the prolonged period when the nucleus is in an under-excited state makes the formation of stable reaction products possible. This is in direct contrast with high-energy nuclear reactions that result in an array of highly radioactive products.

The theory postulates nuclear excitation by means of coherently excited phonons. This is an exothermic process for certain nuclei and would be a source for the excess heat. The resulting nuclear products would be helium-four along with a new element having two fewer protons and two fewer neutrons than the heavy atoms of the lattice. It would have an atomic number two lower than that of the heavy element of the lattice.

* This theory is said to comply with all the conservation laws of physics including energy, spin, momentum, mass, and parity.

Those nuclei that do not achieve sufficient excitation to emit alpha particles might be involved in beta emission or in an electron capture kind of reaction.

When the fission condition is approached slowly (in minutes rather than in nanoseconds), the initial nuclear state "sees" the final nuclear states of products, and a minimum energy principle can be applied. Minimum energy in the nuclear products means they emerge in non-excited states—i.e., without transitions that produce gamma-ray radiation.*

The precise reaction, and the energy needed to cause a reaction, depend upon the particular nuclei in question. There are a number of atomic elements that have the necessary characteristics for anomalous energy production. One of these is the expensive palladium, but researchers expect that there are other, more suitable elements that are less expensive.

In the world of theoretical physics, some scientist have offered strong objections to theories such as this one, objections that set forth possible flaws in the work. This example of theoretical musing may well have perished by the time this book reaches the reader. The problem it attempted to solve will remain with us, however, waiting for other theories to be tried and found more sturdy.

Each theoretician, as we have seen, must develop a target data set that his theory will explain. The items in the sets might settle about two groups of data, namely, those suggesting a fusion sort of reaction and those suggesting a fission type of reaction. Both groups might presage a collective and coherent source of nuclear reaction in or between flawed lattice domains of a required size.

The fusion collective reaction must explain the presence of heat, tritium, and an amount of helium-four that corresponds with the amount of heat. Exceedingly low levels of neutrons and radiation might be included in the theory.

The fission collective reaction must explain the presence of alpha particles and the transmutation of heavy elements.

Other Theoretical Challenges

New experiments in nuclear physics were inspired by the events of 1989. In two cases these produced anomalous nuclear behavior. Although no connection has been made between these results and those of the Fleischmann and Pons cell, it seems appropriate to mention them here.

* Gamma-ray radiation, a much more powerful radiation than x-rays, is particularly dangerous because of its capacity to penetrate barriers.

One way to transfer energy from a nucleus to the surrounding lattice is by the expulsion of a charged particle. Because such particles carry an electrical charge, they interact strongly with their surroundings, which quickly brings their travel to a halt and transfers their kinetic energy to those surroundings in the form of heat. Transfers of this type are not candidates for the energy source in the eletrolytic cell, because the numbers of such particles are many factors of ten less than required to explain the heat. But they are of passing interest because they are not explained by current nuclear theory.

G. P. Chambers, a physicist at the Naval Research Laboratory, irradiated a thin film of titanium with ionized deuterium atoms at the low energy of 350 eV. These ions penetrated the titanium crystal domains and their interfaces.[6]

He made thirteen experimental runs from December 1989 to March 1990. Of these, four produced charged particle emission, five did not, and in four cases the thin film of titanium delaminated from its base, voiding the experiment. Those runs that emitted particles evidenced a characteristic of the Fleischmann and Pons experiment by only working once. He was never able to revive the performance of a particular film for a second run.

Figure 20.1 shows the results of a run in which 1,171 particles were counted by the detector placed immediately behind the thin titanium foil. The particles passed through 4 mm of partial vacuum from the thin film to the detector. (For ease in interpreting the diagram, I have added the vertical line at 5.00 MeV.)

The target was irradiated for forty minutes before counts were detected. These were detected in multiple bursts. Even though bombardment was continued for two additional hours, the bursts never repeated. Considerable efforts were undertaken to make certain that the results were not an artifact of the experimental setup. Chambers interpreted the particles to be tritons (ionized tritium atoms) emitted with the energy of 4.99 MeV.

From the four successful experiments, Chambers calculated that the rate of particle production was 10^{-16} events for each deuteron pair each second, a rate 26 orders of magnitude greater than the prediction of nuclear fusion occurring under these conditions.* To quote from the report, "If the particles are originating from small active volumes within the film, then the reaction cross-section could be higher by many orders of magnitude." It is remarkable that Chambers produced 5 MeV tritons from irradiation by deuterons of only 350 eV energy.

F. E. Cecil, Department of Physics, Colorado School of Mines, Golden, Colorado, measured charged particles in an experiment that corresponded to fusion research techniques.[7] After a sample of palladium was loaded with deu-

* This result was seven orders of magnitude greater than that postulated by S. E. Jones for geophysical fusion reactions.

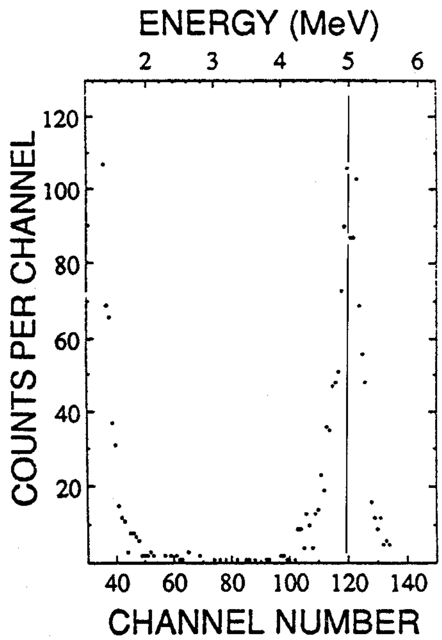


FIGURE 20.1 Chambers, Naval Research Laboratory, reported that 4.99 MeV tritons produced this peak during multiple bursts lasting over three minutes after deuterium irradiation had ceased.

terium by irradiation, a strong direct current was passed through the sample. The experiment was done in a vacuum chamber because charged particles travel only a short distance in air or water. First, Cecil mounted a thin film of palladium and irradiated it with a 95 keV deuteron beam up to a loading ratio of about 0.60. During irradiation, about 800 tritons and an equal number of protons were emitted by the palladium foil. The corresponding energy spectrum showed only two peaks: the tritons at 1 MeV, and protons at 3 MeV. A deuterium—deuterium nuclear fusion reaction produces, for charged particles, a triton at 1 MeV and a proton at 3 MeV.

Figure 20.2 displays the results from running a strong current through this Pd film after the deuterium beam was turned off. During this time, particles emitted by the foil were collected and placed into count "bins" according to their energy level. There is a small peak, perhaps ten counts, in channel 26 (immediately to the right of the 26 mark). This channel corresponds to a 3 MeV energy level. Cecil speaks of this data thus, "there is a suggestion of a peak at about 3 MeV which could be identified as the protons from [this same] reaction." He entertained this fact as though it was not a surprise to find a fusion reaction indicated, even when there was no bombardment un-

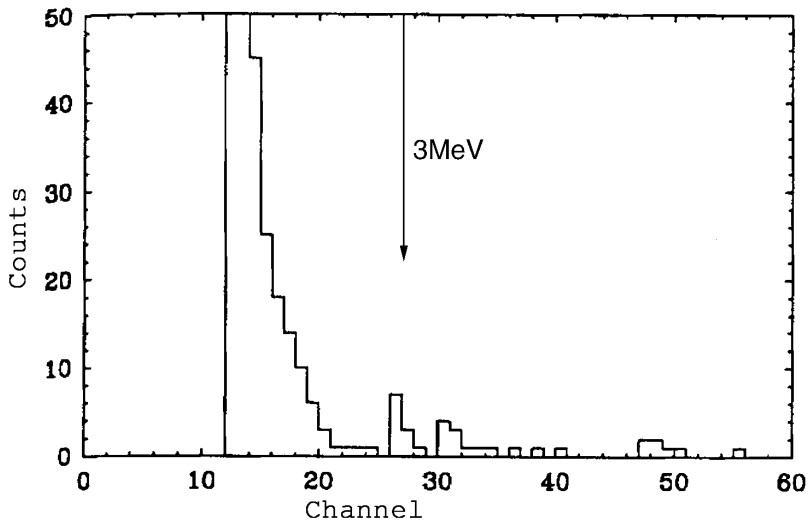


FIGURE 20.2 Cecil, Colorado School of Mines, reported that the peak at Channel 26 and 27 indicates a 3 MeV proton emission, presumably the product of a fusion reaction.

derway. The production of particles in this reaction is at the low rate of about 50 cm^{-3}-sec^{-1} (per cubic cm during each second).

In another run of the experiment, when the current was applied, a peak appeared at 5 MeV, one that did not appear in the control runs. Cecil offers no explanation for this peak.

The Nuclear Sum

Some skeptics complain not that Fleischmann and Pons are wrong, but that, if they are right, then the physics textbooks will have to be re-written. Will any of the conservation laws of physics be challenged? So far, the theorists say that their theories conform to the conservation laws of physics. Will any of the nuclear reaction formulae need changing?

There is no indication so far that any of them need modification except to note that they apply only to two-body interactions. There is some indication that nuclear cross section data may need modification, possibly, at low energies.* There may well be future theories derived from cold fusion research that call for, or that imply, some modification of the reaction rates.[8] The most

* Kasagi reported at Vancouver (ICCF-7) that proton yields were surprisingly larger than predicted at energies of 2.5 to 10 keV in Pd and PdO thick targets. An extrapolation of this result to lower energies might allow us to observe a nuclear reaction at room temperature.

likely requirement for nuclear text books would be additional material on solid-state energy transfers. This knowledge may take years to attain, and so it might be incorporated a little at a time.

The reader's best response to the claim of new nuclear processes is the usual one: stay calm and enjoy watching it unfold. Such change, simply enough, makes another fine spectator sport.

For the present, a valuable assessment is offered by Scaramuzzi.

> The study of collective and coherent phenomena promises very high intellectual rewards, and the hope of contributing to the solution to the problem of the energy supply for mankind is highly inspiring. Last but not least, the challenge of studying a field that is not well understood is definitely fascinating.[9]

A more important aspect of our subject for long term consideration is the evidence for a nuclear source of energy that is environmentally benign. Opposition to fission nuclear power was to some considerable extent based upon a desire to restrict the total availability of energy for individual use.

The anomalous power cell may be capable of technological development into a desirable source of energy for society. Little can be said of the time or funding that this development will require. Even when the scientific basis becomes well established, as in the case of hot fusion, the required engineering will be difficult to estimate.

Part Five

RESOLUTION

CHAPTER TWENTY-ONE

Outlook

Scientists who accepted the reality of anomalous power entered a career that dwelled in a no man's land between waiting for recognition by the scientific community and waiting for the realization of a commercial product. Either would validate the new field and the new career. Both would prove slow in coming.

Mistakes made by the scientific community aborted most of the public stage of development in cold fusion science. A discovery made in the academic environment ordinarily involved exposure of the science through publication. This public period could be short, after which that discovery disappeared into the industrial laboratory where results were held as proprietary. The national laboratories, however, were in a position to do continuing development and to publish it. It is during the academic, or national laboratory, phase that the scientific details become public knowledge. For cold fusion research, the academic science period in the U.S. was quickly ended by Baltimore. The research was continued mostly in private laboratories in California, and in government laboratories or university laboratories in Japan, Italy, France, and Russia. Both Japan and Russia now hold annual cold fusion conferences (open to all) to expedite correspondence among the scientists so engaged.

The University of Utah conducted research in cold fusion science (by Fleischmann and Pons) from late 1984 into the early months of 1991. This research was available for publication, and through the grace of IMRA Europe, much of it saw the light of day. Also, to my knowledge, there has been no restriction on publishing the research done at the National Cold Fusion

Institute (NCFI), Salt Lake City, Utah, from 1989 until its demise in June of 1991.

During 1989, academic research in other universities was published. Stanford University, Texas A&M University, Minnesota University, Case Western University, Caltech, and several others published their work in this field. The national laboratories were active in 1989, but that trailed off to a few isolated researchers who carried on for a decade at LANL. Their work also was published.

Scientific personnel and projects began to migrate as early as June 1989 to the proprietary laboratory in order to obtain protection for any inventions that might result. Fleischmann and Pons accepted industrial positions from Technova in 1992 that they might continue their work. But this meant that the work was to be done in a corporation rather than in their natural milieu, the university. After that move, they published information only about their calorimetry results. No publications by them have appeared in the crucial area of improved cathode design to achieve better reproducibility. In the case of cold fusion research, the window that allows public disclosure was prematurely shut.

The experiment proved difficult. It cannot be too greatly stressed how development of the field was thwarted by scientists grossly underestimating its difficulty. Even Fleischmann and Pons thought it would be easy for them to replicate their experiment since they had no trouble doing so up to the time of the announcement. They also had no idea how problematic it would be to bring others along to do the experiment successfully. These difficulties were compounded when the two of them were made unwelcome in American academia and moved into the private environment of corporate research and development abroad.

Even so, the original cell design is seen to have evolved in numerous ways. The variety of technology that has emerged was briefly described in Chapters 14–19. No one can say what ultimately will become of it, but a few comments are in order.

Three purposes direct this new field of science: (1) to develop a more readily replicable experiment in order to reduce the cost and improve the impact of research, (2) to learn the breadth of the Fleischmann and Pons effect over the many disciplines of science, and (3) to find the source of the anomalous power. Will one generation of scientists be sufficient to fulfill these purposes?

The scientists practicing the attainment of reproducibility in cold fusion research had to acquire ever increasing control of their experiment.[1] After validation was reached at the end of 1994, the next goal naturally enough would be to increase the reliability in obtaining anomalous heat in a single attempt.

The version developed by Preparata is a large volume flask with a long wire Pd cathode that is threaded up and down as it progresses around the inside periphery of the flask. A platinum anode is at the center. This unique experiment uses two power supplies to operate it. One is connected to the anode and cathode in the usual manner. The other power supply is connected to each end of the cathode wire. It maintains a current along the length of the palladium cathode wire during the time of the experiment. Preparata uses this second current to obtain high loading. By avoiding the sorting and selection process for palladium, he claimed to have obtained 100% reproducibility in building the electrolytic cells.[2]

The Scaramuzzi group came close to total reproducibility in 1996 with a system it developed to evaluate palladium samples for suitability, "Reproducibility is still an important issue, but much progress has been made, and I think that it is at hand."[3]

We have seen how Melvin Miles took several months to learn how to build cells that would generate excess heat, and how he later visited the NHE laboratories in Japan and was successful there in doing so. He found it necessary to sort the palladium samples that were available to him to find those that were suitable for generating excess heat. He has been quite successful in this endeavor. He now reports that more than half of the cells he constructs obtain anomalous power generation, which is about on a par with mammal cloning experiments.

After many years of experimentation by Mengoli, the reproducibility appears to be good (Figure 15.2). His experiment meets the one success out of two attempts requirement that was discussed in Chapter 10, p. 141. Only the considerable investment of starting a large number of trials will demonstrate that an acceptable level of reproducibility has been achieved. His experiment appears to be transportable and communicable. It is the experiment I would recommend to a laboratory that was planning to start work in this field. From all appearances, it is the experiment that should bring the highest likelihood of success.

Other developments in the field promise higher quality heat, and heat without a source of excitation. The Fleischmann and Pons cell itself resists commercialization. The greatest obstacle is the tendency of the electrolytic cell to cover its cathode with gunk. This build up takes a week to a month depending on the laboratory skill levels employed to prevent it. Continuous cell operation depends on constant technical nursing that might be possible only with a large-scale industrial application.

One of the most promising experiments emphasizes the heat after death phenomenon. Mengoli operated his Fleischmann and Pons cells at 95C to obtain fast startup of heat generation, and observed astonishing performance after the cell current ended. The cell demonstrated heat generation without the confusing factor of excitation input power. The demonstration seemed to last indefinitely, stopping only when the high temperature bath was allowed to return to room temperature. Such a demonstration is especially persuasive of the reality of anomalous power.

Mizuno investigated the use of ceramic proton conductors. Oriani corroborated his experimental results by generating anomalous power at 400C, and in one case without excitation. An operating temperature of 400C offers high quality heat for running turbines and generators. It is a long development pathway to first perfect the ceramic, then the heat generating system, then to scale the whole thing up to a high power level. This caliber of investment probably can not be undertaken in the U.S. because the Patent Office will not issue patents to protect cold fusion investments. But a technological promise of sorts is there, nevertheless, for whomever may exploit it.

Governments

In these paragraphs, I tell the disquieting story of attempts to develop commercial product applications. Product development had to build almost solely upon the observation of anomalous power in the electrolytic cell. The odds were heavily against success in such a venture because the scientific basis for the heat was not known. Also, product development under these conditions tends to be done by scientists instead of by engineers.

Stanley Pons occasionally indicated that commercial products could be fashioned quite promptly from his laboratory device. This unwarranted optimism is a common weakness of research scientists who know little of the strict demands of the marketplace, not to mention the prior demands of manufacturing. Such statements of ready commercialization ought to be dismissed out of hand.

Historical examples exist of useful products embodying high technology design that were developed without the prior establishment of a scientific base for the technology. The flash strobe lamp of the kind seen in cameras and on high towers, for example, became an article of commerce in the 1930s before its scientific basis was complete.

During the first twelve years, several attempts were made by agencies of various governments to develop the anomolous heat phenomenon as a power source. The organizations that engaged in such research are worth looking at briefly. These government-sponsored programs were in the news frequently and the demise of each was widely reported.

No one or two persons were to blame for the failure of National Cold Fusion Institute (NCFI). The Utah legislature allocated $5,000,000 for its founding.* From the beginning, it was impossibly chartered to achieve an article of commerce, a product design. Fleischmann and Pons were given consideration in terms of what they might contribute to it only after the institute was established. They were perfectly straightforward about their interests and pointed out that they were not product development engineers. They were at heart academic researchers and intellectual gadflies. Their relationship to the new organization was that they were available but their commitment was always oblique. They also harbored a suspicion that the organization's associates would leak information on the research to their commercial friends. In fact, the Institute never had their undivided support. Their principle activity while they were there was to set up a factorial† cell experiment, part of which was in the chemistry department with the rest located at NCFI.

The NCFI staff undertook experiments independently of Fleischmann and Pons. While they reported the generation of tritium, they were never able to make a cell produce anomalous power. The fact that the NCFI was unable to achieve that first milestone marked the defeat of their purpose. It closed its doors in June of 1991.

A Japanese student of Martin Fleischmann's approached the two chemists at the 1991 Electrochemical Society meeting in Kyoto to broach the topic of alternative employment should they leave their positions at the University of Utah. This opportunity blossomed into a friendship between the two and Minoru Toyoda, doyen of the Toyoda clan, founder of the Toyota automobile company and sponsor of Technova. As employees of Technova, Fleischmann and Pons picked up and continued their Utah research in a laboratory designed for precision calorimetry. The laboratory, IMRA Europe, was located in a technology park environment in Provence, France, not far from Monaco.

The research they started in Utah was completed there to a considerable extent. The materials problem with cathodes was brought under a great deal more control and the calorimetry was established at a high level of accuracy. Their basic experiment was advanced to a higher power level by operating at 100C, a temperature at which the electrolyte boils continuously. The calorimetric part of this work was extensively published.

After the death of Minoru Toyoda, Technova management wanted to see progress towards a power-generating device, an article of commerce. While Fleischmann and Pons were essential to the research function, it is unlikely that they were the right people to head a commercial development effort.

* Fleischmann says that he and Pons spent only $350,000 of it.

† This efficient method for doing a large number of experiments when each individual experiment was long running is described in a Chapter 5 footnote (see page 76). When the experiment is finished, the results of the many cells were then analyzed mathematically to sort out the results for each of the several variables, each measured against its control value. The advantage of this approach is that each of the five variables and its control got many experimental runs, not just one.

Fleischmann retired from the operation in August 1995. Pons did the same in early 1998, and the facility was closed. An estimated $40 million was spent, and no product of any sort was in sight. Technova had provided a setting where Fleischmann and Pons could advance and complete their anomalous power experiments begun in Utah.

The Japanese culture does not tolerate the kind of personal assault that America witnessed at Baltimore and Los Angeles in May 1989. There was a refreshingly wide range of opinion about cold fusion studies in Japanese scientific circles. Opinions were less extreme and more open to the possibilities for at least two or three years rather than for two or three months. The Japanese Ministry of International Trade and Industry (MITI) decided to try its hand at research in the field in 1993.

MITI did not have a national laboratory facility where a proposed program could be shaped into a realistic plan by an established research staff. Rather, space was rented, technical people with the appropriate skills were hired, and the facilities were filled with the best equipment. The form was perfect. The operating plan called for a series of milestone achievements leading to a prototype commercial power device in five years.

In retrospect, the entire time and effort of the five-year program were devoted to their first objective, obtaining anomalous power generation. Their cells were of their own design, because the cells made by Fleischmann and Pons could not physically fit into their calorimeters. They used a double calorimeter around their cells and it eventually produced conflicting data. The inside calorimeter showed the presence of anomalous power, but the outside calorimeter did not. With that kind of difficulty to be resolved, the laboratory suffered from its lack of an extended laboratory facility, staff, and management, the reserve of skills that are found at a national laboratory. The laboratory had been established without including a single electrochemist of Ph.D. rank. Staff was shuffled in and out in six months stays from sponsoring companies. No one of sufficient intellectual training that had the confidence of program management was available to resolve the conflict in the calorimeters, and to direct the research to completion.

The laboratory spokesman stated that, in their own cells, they never experienced anomalous power episodes. Fleischmann has made a presentation of MITI data showing that they did indeed get excess heat. Storms, who had done extensive investigations of the palladium metallurgy, said, by way of critique, that they did not take advantage of much that was known about selecting suitable palladium. MITI had little choice but to bring its efforts to an end. MITI did not conclude that there was no such thing as anomalous power; it had no grounds for doing so. It was simply that this research was too sophisticated for the scope of its program. Political considerations naturally required that it also terminate much of the other cold fusion research it was supporting in various academic laboratories throughout Japan.

Thus ended three attempts to move towards a commercial product without an available scientific base of knowledge. The money spent in those three cases was not wasted. Knowledge about the anomalous power phenomenon accumulated from the work at NCFI, Technova, and MITI and most of it was published.

Scaramuzzi spoke to an interpretation of these closures.

> One could be tempted to interpret the closure of these three important projects as a demonstration that CF research is failing in its objective to become a well-defined discipline in science. I am personally convinced that this interpretation is definitely wrong. Let me explain why . . . it was to be expected that enterprises that were born with the aim of having a practical fall-out in a short time had to give up. I am still convinced that a lot of basic research is needed, in order to better understand the science underlying CF, before practical objectives can be seriously addressed: this [scientific objective] can be better pursued by small groups that proceed with this clear idea in mind.
>
> And this approach in my opinion is what is beginning to happen. Let me mention a few events which show this tendency . . . : At Grenoble, in France, there is a laboratory funded by the French CEA (Commisariat a l'Energie Atomique) and by the Grenoble Institut Polytechnique which has started a research project in CF. The SRI group . . . is presently active in CF research with funds from the US agency DARPA (Defense Advanced Research Projects Agency). In Italy a new initiative is starting now: a cooperation between ENEA, INFN, LEDA that will allow the creation of a new laboratory at ENEA Frascati with a research program on CF, funded by the Italian Government for the next three or more years.[4]

This is an example of the sort of support that is needed and appropriate.

During the 1990s the U.S. Government supported cold fusion research through the Navy and the Defense Advanced Research Projects Agency (DARPA) of the Department of Defense. This was always done in a tiptoeing and whispering sort of manner, always fearful that they would be discovered and hounded to abandon their projects. By 2000, this quiet approach had spent about all the dollars for research on the new science that they felt was possible without attracting more attention than they could handle.

To everyone's surprise, the DOE then got back into the act. The reader will recall it was this same department that wrote cold fusion research's obituary in 1989. One of their offices initiated a project to find and to fund "Breakthrough Energy Physics Research" possibilities. "The ultimate objective is to develop and validate new scientific principles . . . that will enable revolutionary advances in energy generation. . ." "The implementation approach . . . involves partnering . . . to identify . . . anomalous effects and to extend theory development . . . to the point of supporting validation experiments."[5]

In an earlier attempt at this kind of indirect approach to our topic, the DOE had awarded Professor Miley a $100,000 contract to do further work on his thin films to generate excess heat and produce transmutations. The contract was specifically to conduct a "Scientific Feasibility Study of Low Energy Nuclear Reactions for Nuclear Waste Amelioration." The cold fusion skeptics went on the attack. According to *Science* journal, an organization called the Nuclear Control Institute wrote to the Secretary, "The credibility of DOE will be irreparably damaged unless funding for this cold fusion proposal is immediately withdrawn." The DOE's Office of Science put together a six person, secret panel of peer-reviewers that specialize in the fundamental sciences. This review unanimously recommended that the contract not be funded, and Miley's funding was withdrawn.

The new program, BEPR, accepted pre-proposals and also searched the scientific literature for possible breakthrough topics. It searched separately and in a similar manner for theories and for anomalous effects. The criteria for the latter are listed below along with the points for scoring the different ideas.[6]

1. Anomalous effects that are published in peer-reviewed scientific or technical journals +10.
2. Anomalous effects that are published in non-peer-reviewed journals or are in non-published pre-prints +7.
3. Anomalous effects that are privately published +4.
4. Anomalous effects that are unpublished manuscripts +1.
5. When two or more anomalous effects are published by independent researchers which correlate add points.
6. For each published challenge that is unanswered by a rebuttal −5.
7. For each published challenge that is answered by a rebuttal −3.
8. When one theory and one anomalous effect are published which correlate add points.

In this fashion, the DOE selected those (1) theories, (2) effects, and (3) theories combined with effects which deserve development support. The office says that an ongoing database of the reports offered for this survey will be maintained and augmented so as to be continuously available in the years ahead.

At the most preliminary level, the BEPR program in October 2000 rated Miley at their "acceptable level" to investigate LENR.[7] The program describes their measure of Miley's work to date thus.

> Another emerging area in the peer-reviewed scientific literature that describes a potential method for novel energy conversion, generation, storage, and use involves a phenomenon known as Low-Energy Nuclear Reactions (LENR's). Various researchers have published papers in the scientific literature on the phenomena. This effect is different from the so called "Cold Fusion" effect which described anomalous heating effects in deuterated metals. LENR's have identified nuclear reaction products that are attributed to hydrogen or deuterium interactions with the metal electrode (rather than D-D type fusion). A growing body of experimental evidence is cited showing that LENR's can occur under select conditions in solid lattice loaded with hydrogenous atoms. For example, the experiments of T. N. Claytor, and his colleagues at LANL.[8] There appear to be various reaction regimes leading to different nuclear products. To illustrate the LENR effect, recent experiments at the U. of Ill. were performed, where a large number of new elements were observed in thin films of various metals such as Ni undergoing electrolysis.[9]
>
> Mounting data supports the reality of LENR in solid state lattices loaded with hydrogenous gases under a variety of conditions.
>
> The situation is complicated, however, by the possibility that several different reactions regimes exist . . . Should the existence of LENR's be verified, the implications could be significant. The power densities reported in present cells are quite high, such that a simple volumetric scaling could be used to quickly develop 10–100 kW power units. There is no obviously fundamental block to going to yet much higher power levels, but new designs would be required to handle the extreme heat loads involved. It will take more independent experimentation, the refinement of a workable theoretical basis, and consistent replication to confirm and fully understand this anomalous phenomenon.

A study of the scientific literature by the DOE thus reveals the presence of credibly scientific activity in LENR.

It is evident here that no matter what reaction it is that provides the power for excess heat in a Fleischmann and Pons cell, no one is to associate it with the term Low Energy Nuclear Reactions. That phrase is now reserved, apparently for anomalous behavior separate from excess heat. It may be D + D fusion; it may be something else, but the term LENR must not be sullied by reference to excess heat. Which leads to the question, How high did the BEPR score on the evidence for anomalous power in the form of excess heat energy? Or, was that item disqualified at an early stage to avoid the issue of cold fusion research?

In any event, the BEPR program is a modest, courageous and worthwhile start-up endeavor.

Corporations

Why do commercial entrepreneurs try to develop market applications in the face of these failures? One answer is that the money being committed was quite modest. The products were relatively simple in their construction, so they could be tried at each step of development to see if they functioned properly. A company may well find success if it could continue to make its laboratory device generate excess heat while it was engineered into a useful product. Also, the funding could be arranged to match the development effort step by step. That course of planning resulted in a reasonable level of risk at each stage as long as the device under development was relatively small and simple.

The other side of that risk is that the payoff can be particularly great: a starting place in a newly developing sector of the energy industry. The rewards may be handsome for those who invest at an early time.

The founder of Clean Energy Technologies, LLC, (CETI), Sarasota, Florida, Dr. Patterson, received a patent quickly because he was over 70 years of age, and there are fast response rules in the Patent Office for people over a certain age. Patterson was cleverly able to avoid the absolute ban on cold fusion patents by channeling his application to a specific department in the U.S. Patent Office. He obtained his first patent in 1993 and a dozen or more have followed. CETI raised outside money in the low million dollar range, and expanded its technical and management depth accordingly. But these strategies did not work out. The technical base was simply too thin to sustain them. At this writing, the company has retrenched greatly.

ENECO, Salt Lake City, Utah, was formed to hold and exploit the original cold fusion patents under agreements with Fleischmann and Pons, and the University of Utah. The continuing refusal of the U.S. Patent Office to consider any submittal and a much later challenge to their European applications by CETI, combined to make that course of business development financially untenable. The company turned to product development in the field of cold fusion research in 1996 along with an accompanying new technology for solid state conversion of heat to electricity, under the direction of Fred Jaeger, its president. After several years of research at a cost of $2–3,000,000, the phenomena of excess heat could not be generated in a repeatable manner by its product device. Further activity in the field cold fusion research was terminated in the fall of 1998.

Blacklight Power Corporation, Cranbury, New Jersey, was founded by Randell L. Mills who has an M.D. degree from Harvard and an engineering degree from MIT. After the cold fusion episode got underway, he made his own attempt to understand theoretically what might be going on in the cold fusion cell. He soon announced an entirely new theory of quantum mechanics at odds with the textbooks. One result of his calculations was that he predicted the existence of a new form of hydrogen. This form placed the single electron that orbits the hydrogen nucleus at a considerably smaller radius to the nucleus. An electron moves from its usual position to the newly claimed position by giving up a significant amount of energy. Mills claimed that it was this change of the hydrogen electron orbit that was the source of the excess energy he was seeing in experiments with a nickel cathode in light water. His theory involves no reactions in the nucleus itself.

He established Blacklight Power to exploit his understanding of this new form of hydrogen, referred to as a hydrino. Previously, Mills had done four years of experiments with cells to generate anomalous power. The company considered these to be successful enough so that it started commercial development under alliances with one or more electric utility companies. Their plan was to offer electric water heaters that obtain a large fraction of their power from the new process and hydrino hydride chemical compounds. Blacklight has raised capital funds in the low tens of millions of dollars. Blacklight has been trying to position itself to issue an initial public stock offering, but that market continues to be so volatile as to make such an offering unpredictable.

One of those strange events that are not uncommon to cold fusion research occurred to Blacklight in the spring of 2000. A patent they had been issued was suddenly retracted by the Patent Office (PTO) without the PTO's official investigator being informed of the action. Blacklight took the matter into federal court where the Patent Office was upheld in its retraction of the patent.[10]

Fusion Power, Inc., was founded by Dr. Les C. Case, a chemical engineer with several degrees from MIT. He has generated excess heat with the concurrent creation of helium by means of commercial palladium catalysts in a deuterium atmosphere. This achievement, that constitutes a major contribution to the field, is presented in Chapter 16. Fusion Power, Inc., remains a solo establishment with limited priorities and direction. Case's purposes are to develop a self-sustaining reaction, to find ever more suitable catalysts, and to scale up his generator to that of a commercial prototype.[11]

Several established companies have had contact with the field of cold fusion research. General Electric negotiated an early participation with the University of Utah. The most substantial critique of the Fleischmann and Pons paper on their calorimetry was written by Wilson from its Schenectady, New York, laboratories. The Wilson paper seems to have marked the end of their interest.

Motorola took a more activist stand by making token investments in several small groups to get a look at what was going on.

Italy has been diligent in its cold fusion research from the time when Scaramuzzi entered the field in 1989. There were three groups planning to commercially exploit the field. Each had its nuclear theory and its laboratory demonstrations to support its commitment.

Just two years after validation, by the end of 1996, the Fleischmann and Pons experiment with its phenomenon of anomalous power had attained the reproducibility level of 50% or higher for the experienced electrochemist. The "serum touch," or what in this case might be called a palladium touch, certainly was involved to some extent in this achievement. That is, scientists such as Miles had a way of evaluating a palladium sample that allowed them to expect a 50% success level for each newly built cell.

Validation and nuclear products have each been given their due consideration in our story so far. Anomalous power necessarily leaves behind it a nuclear ash of presently unproved composition, but with evidence accumulating for the element helium. Society, however, has been given a new source of energy. Little can be known at this early time of how it will eventually be used. If one wants to muse in that direction, the best that can be done is to look carefully at the experimental work underway. From that, the long-term characteristics of this energy source might be estimated.

The useful application of a heat source depends to some extent upon the temperature at which it is generated. In use, heat is passed through a converter and out into the ambient surroundings such as the local air or water. The energy that can be extracted from that heat depends upon the temperature difference between the source and the ambient. The Fleischmann and Pons phenomena was produced most frequently in experiments well below the boiling point of water. At such low temperatures, this difference is small and the thermodynamic energy conversion efficiencies are relatively low. Heat produced at these temperatures is referred to as low quality heat.

Low quality heat has its uses. An important application would be in space and water heating for personal use. Much energy is consumed (on a per capita basis) to supply a warm room and warm water both in the work place and in living quarters. In economically developed countries, there is a sizeable investment in central heating systems to accomplish this, but the cost of such systems appears as a staggering obstacle for less developed countries. A large fraction of the population resides in the village level at the turn of the twenty-first century.

Low temperature operation of experimental cells might be embodied in a product of a size and cost designed to provide for the warming of a room or of a water supply at the family level of social organization. It is not certain that a source of electricity will be needed to get such a device started or to sustain its operation. A source of heat such as a small fire might do as well to start it operating. At the village level such a device would supplant the gathering of firewood and brush, and the use of kerosene.

At the twelve-year anniversary, it is not yet clear whether the heat generating formulae consumes the palladium or not and, if it does, at what rate. Assuming for the moment that it does, we can consider the sufficiency of palladium supply* in the words of Fleischmann (October 1999).

> [The] experiments with electro diffusion [G. Preparata] pretty regularly get heat [density] releases of 10 kW/cm^3 and occasionally 100 kW/cm^3. If this can be done reliably, this would allow one to construct systems that would satisfy a large portion of the world's energy needs with existing palladium production. This is all low-grade heat —a lot of the world's energy is used as low-grade heat.
>
> Increasing the quality of the heat will not be a particularly difficult problem. You can compress the system [and run it under pressure].[12]

There is also evidence that the metal niobium can serve the Fleischmann and Pons phenomena as a substitute for palladium.[13] V. A. Filimonov at the Institute for Physical Chemical Problems, Belarus State University, Minsk, obtained excess heat using niobium cathodes in heavy water electrolysis.[14] If his results are corroborated, niobium can supplement the supplies of palladium. World reserves of niobium are estimated at 14 million tons.

Mizuno and Oriani obtained heat from ceramic proton conductors at 400C, and Case obtained excess heat at 200C. Those temperatures give high quality heat of the sort that can, in large quantities, drive turbines to produce electricity. But the path from these experiments to something of economic value is a rough and serpentine one. All that can be said with certainty is that it is a promising area for research investment.

The status in 1997 for the generation of anomalous power is illustrated in Figure 21.1 by David J. Nagel. The figure of merit used was the power density in the palladium cathode because in certain fundamental ways power density is a measure of usefulness. However, the reader should keep in mind that the power densities to be described were obtained from small cathodes (i.e., small volumes). The historical progression from wood to coal to nuclear follows that paradigm of increasing power density. (Oil was prized for its usefulness as a liquid.) Nuclear power is shown with its power density of 500 to 1,000 watts per cm^3 in a fuel rod.

The range of power density values covered by point A was reported by

* The world's palladium supply is estimated at 100 tons per year.

SUMMATION

The Conflict Between Data and Theory

Resolution:

Fleischmann and Pons resolved the contention between their claim of anomalous power that was essentially without concurrent neutron radiation and their recognition of contemporary nuclear physics by proposing the hypothesis that a presently unknown or unrecognized nuclear process provided the heat power they had measured.

The skeptics resolved the contention by spurning the claims of excess heat measurements: "anomalous heat was claimed, but no nuclear products were reported." In this oblique way, they would deny the validity of the laboratory results by fiat. That denial contrasted unfavorably with the action of Irving Langmuir and Robert W. Wood, who went bravely into those laboratories that generated data conflicting with theory and participated in the experimental work. There, in those laboratories, they defined the term pathological science by demonstrating what was wrong with the experimental claims. The skeptics protocol can be seen to be wrong.

Conclusions:

1. It was wrong of the skeptics to offer guidance to the community about the nature of the cold fusion controversy while they summarily refused to recognize the existence of the experimental procedures that initiated and sustained the research.
2. Their demand for nuclear experimental data as a substitute for the heat data they spurned was a violation of procedure. The available data has been held hostage for over twelve years by their demand for the provision of data that was not available.
3. Because of those demands, the continuing contention between nuclear theory and anomalous heat data was mistakenly omitted from admission into mainstream science, there to be resolved.
4. That omission provided contemporary nuclear theory with illegitimate protection from contention. Theorems must be exposed to contention if they are to be considered falsifiable.
5. That failure to maintain the theorems of nuclear science falsifiable was a violation of scientific protocol by the skeptics.

Fleischmann and Pons from their research at the University of Utah from 1984 to March 1989. Their continuing research there, after the announcement, resulted in density levels of 100 to 1,000 watts per cm^3 and is shown in point B. Their work at IMRA Europe, 1992 to 1995, resulted in the generation of anomalous power at the boiling point of water with the additional phenomenon of "heat after death." They let the cell boil dry and then observed an enormous release of heat lasting for many hours. The density achieved, point C, was 4,000 watts per cm^3.[15]

The last point was achieved by Preparata. His electrolytic cell used a long, fine palladium wire for its cathode. A separate current flow is maintained from end to end through the cathode wire. The experiment generated power at a level of several hundred watts over a period of tens of hours. "The same [result] was observed in the about fifty similar experiments that we have conducted." That result produced a power density of 50 to 100,000 watts per cm^3, shown in Figure 21.1 as point D.[16]

The small size of the experimental devices used to gather excess heat and helium data implies that articles of commerce derived from the resulting technology might also be of small size. This special characteristic is displayed in Figure 21.2 by Nagel, where it can be seen that a cold fusion derived device would be uniquely small and portable compared with other nuclear sources of power. Applications that come immediately to mind are those for space, as well as water heating in the home and office that were already mentioned. Transportation applications might also prove practical.

It is noted that the United States may prove an inhospitable country for such devices because of the enmity towards availability of energy for personal use that has been emphasized by the leaders of environmental movements in this country.

The existence of anomalous power as a newly discovered natural phenomenon became sufficiently well documented by the end of 1994 to establish a continuing field of scientific study, a new science if you will. The signature of that science is the appearance of anomalous power in the Fleischmann and Pons electrolytic cell. This phenomenon ought to retain the attention of science because, in Marie Curie's words, "it defied all contemporary scientific experience." Science cannot properly ignore such an observation.

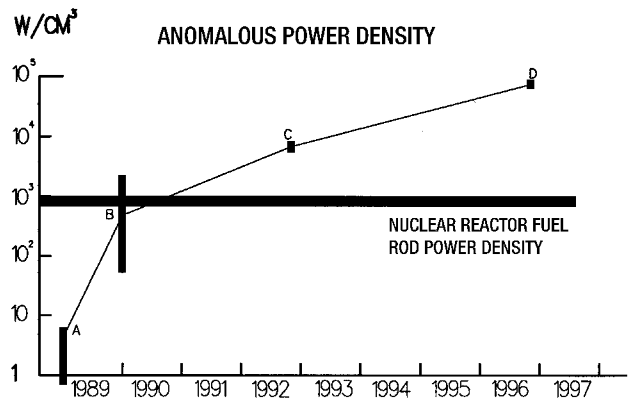


FIGURE 21.1 Nagel, Naval Research Laboratory, charted the increase of anomalous power levels since 1989, including points A, B, C by Fleischmann and Pons, and point D by Preparata.

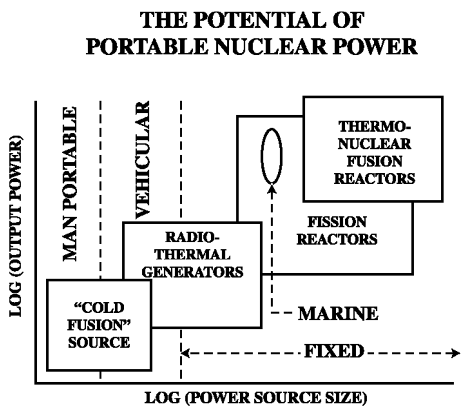


FIGURE 21.2 Nagel shows the relative sizes and power levels of several types of nuclear power generators.

The outlook for early development of a successful commercial product must be considered marginal at best considering that the past activity was carried out at penurious funding levels, without an understanding of the scientific base, without patents to protect an investment, and with the nominal hostility of the various American scientific establishments and little development activity remains.

What is more likely than the achievement of commercial success in cold fusion research is an hiatus in the development of the field of about one generation. After the antagonists have left the scene, the large body of technical literature would be re-discovered, found valuable, and diligently pursued in the appropriate academic departments. This would be done with the concurrence of the scientific establishment.

CHAPTER TWENTY-TWO

The Skeptics

It was a surprise for me to realize that the skeptic, as herein defined, does not often contribute to the advancement of science. In Chapter 10, p. 133, I quoted Beveridge in support of this view. The skeptic is one who will not accept an assumption that is fundamental to a field of study thereby leaving him blind to the research. Consequently his "criticisms" are not useful to those to whom they are directed.

The world accepts the implication of Magellan's ship's journey circumnavigating the globe, but Beveridge's flat-earther does not. A cartographer puzzles over the paradox of how to design a flat map to depict the spherical Earth. The distortions implicit in his finished map are ridiculed by the skeptic as error. The skeptic thus imagines himself a critic. The skeptic, believing the world to be flat, does not recognize that the cartographer's mapping problem exists. His "criticisms" do not help the cartographer.

The data of interest in cold fusion studies imply the existence of anomalous power in the Fleischmann and Pons experiment. Those committed to the field strive to assess that data with increasing rigor while the skeptic ignores that same data. The skeptic can criticize them; he can not help them. His "criticism" is sterile. Nevertheless, skeptics have played a significant, possibly formative, part in the cold fusion saga. Their part in it must be thoroughly considered.

The field of cold fusion research suffered from the armchair skeptic. He was supremely confident of his nuclear theory and so did not venture into the chemistry laboratory. He somehow knew, *a priori,* that anomalous power did not exist.

The source of motivation for the cold fusion researcher as seen by the skeptic is dramatized in the following quotation of May 1991. Park, spokesman for the APS, refers to the continuing cold fusion research activity,

> If everyone knows it is wrong, why are they doing it? Inept scientists whose reputations would be tarnished, greedy administrators, . . gullible politicians who had squandered the taxpayers' dollars, lazy journalists . . .—all now had an interest in making it appear that the issue had not been settled.[1]

Learn Calorimetry?

Learn calorimetry? Surely you are joking. An MIT nuclear physicist who participated in the early cold fusion effort there said, "I think to do calorimetry is one of the hardest things I ever tried to do. I'd rather stick to plasma physics."[2] The subject had the humorous aspect that the most aggressive skeptics were nuclear physicists who emphatically said they do not like measuring heat or heat flow.

Early acceptance by all parties of the lack of a high level of neutrons in the Fleischmann and Pons experiment left unaccountable amounts of energy remaining as its signature. It followed that if one wanted to criticize the experimental work being done, one must develop some degree of expertise in calorimetry. Almost none of them did. Their pointed refusal to learn the relevant specialty over a period of twelve years further identifies them as skeptics.

Huizenga's "Fiasco"

For the reader to come to terms with this field of science—and that is one of my purposes—it is necessary to briefly revisit some of its most influential books. John R. Huizenga wrote two editions of his book entitled, *Cold Fusion: The Scientific Fiasco of the Century*[3].

John Huizenga was the only tragic figure in the cold fusion episode. His vulnerability was in place from the beginning, in the classical tradition, and that vulnerability developed into tragedy in an autonomous manner. He believed in the ascendancy of nuclear physics to an extent that valued other scientific disciplines as expendable. This unfortunate stance led him inevitably to indulge in exaggerated metaphor. By 1999, he would say to the *New York Times* that cold fusion was as dead as it ever was, meaning that it was dead. He would say that about a field employing over one hundred scientists for more than twelve years.

As co-chairman of the Panel on Cold Fusion, Huizenga also wrote much of its two reports. He spoke at the annual meeting of the American Association for the Advancement of Science in February 1992, on the subject, "Cold Fusion Exposed." To my knowledge, he has not published a single, peer-reviewed paper on any aspect of cold fusion studies.[4]

Fleischmann and Pons hypothesized a nuclear energy source that was largely without neutron radiation.* The Utah announcement went as follows (Fleischmann speaking) (This quote is repeated from Chapter 4, p. 53),

> . . . the interesting phenomenon about this [generation of heat power] is that the rate of generation of tritium and the rate of generation of helium-three is only one billionth of what you would expect if the fusion reaction were those experienced in high energy physics. So we have a relatively low rate of production of neutrons.[5]

From the very beginning, Professor Huizenga was caught in a bind. His emotional involvement in this claim was clear from expressions like ". . . nuclear science is a mature field . . .," and ". . . fifty years of study of nuclear reactions . . ." The idea of a new class of nuclear reactions, or an extended elucidation those known, was unimaginable to him. His reaction came, not after extended study of the Utah claims, but from the day of the announcement.

Both editions of his book were written for the professional scientist. His target reader was qualified to evaluate the data that provided the field's source of motivation. Much important anomalous power data was available at his manuscript closing in June 1991. Yet no figure depicting those results was included, and no bibliography of anomalous power was provided. Nor was a reason offered to the reader for those omissions. Thus the professional scientist was deliberately kept ignorant of the blossoming anomalous power corroborations produced by a variety of laboratories.

Huizenga's Credo

The outstanding characteristic of Huizenga's credo (see Summation, page 306) derives from the absence of any separate statement of his about anomalous power without simultaneous reference to nuclear products. Calorimetric technology appeared in this manner at least partially hidden behind a veil of nuclear comment. The importance of anomalous power got quickly shuffled offstage, so as to not retain the reader's attention. He found it necessary to

* The original announcement claimed 10^4 neutrons/second, and this was quickly revised downward. Even 10^4 would be a factor of a billion fewer neutrons than would be needed if conventional nuclear fusion processes were providing the measured heat power.

SUMMATION

Huizenga's Cold Fusion Credo

John Huizenga spells out his position in a credo at the very end of his published works. The one paragraph passage appears three pages from the end of the second edition of his book. That credo should be examined in order to accurately understand his position. It consists of a paragraph of six statements.

1. "The term 'cold fusion' as presently used encompasses a mélange of claims as discussed in previous sections of this chapter."
2. "The more avid proponents of cold fusion continue to argue that the excess heat in many experiments is so large that the source of energy must be nuclear fusion or some other unknown nuclear reaction (sic)."
3. "A fraction of these proponents takes the more conventional point of view and admits that if the process is truly nuclear, there should be a commensurate amount of nuclear ash."
4. "The task for these advocates is clear cut: find the nuclear products."
5. "If the reported intensity of nuclear products is orders of magnitude less than the claimed excess heat, then the excess heat is not due to a nuclear reaction process."
6. "Furthermore, if the claimed excess heat exceeds that possible by other conventional processes (chemical, mechanical, etc.), one must conclude that an error has been made in measuring the excess heat."*

*Huizenga, John R., *Cold Fusion: Scientific Fiasco of the Century,* 2nd edition, (New York: Oxford University Press, 1993), p. 285.

direct his own interest, without exception, to nuclear considerations. This approach to cold fusion studies was typical of Huizenga's influence in the field, and it was a principal source of the confusion that racked the field from its inception. I consider this demand for nuclear product data with the heat data, implicit or otherwise, to be a corruption of conventional protocol.

The principal motivation for those working in the field was the well replicated demonstration of anomalous power. Huizenga's leadership of the scientific community was exercised to lead it away from an evaluation of those claims. This misdirection was one reason that searching criticisms of the calorimetry were paltry during the first six years.

Looking at the credo as itemized in the Summation one sentence at a time, he confirms in sentence 1 that "cold fusion" is only a name and should not be taken literally.

Sentence 2 is at the core of Huizenga's emotional involvement. Note that when he wrote the words "unknown processes" he felt it necessary to follow them with the notation "(sic)". He finds it difficult to discuss that topic even as an hypothesis.

Sentence 2 is a diversion. The contention is not that some "continue to argue"; it is about scientifically measured data. The use of the verb "argue" takes the topic of claimed excess heat to a distance where the obligation to evaluate the claim will pass by unnoticed.

Sentence 3 is incorrect. *Proponents* of cold fusion research accept that nuclear ash must exist commensurate with the amount of heat generated. No one is looking for this ash as the product of conventional nuclear fusion. The challenge is that nuclear ash from presently unknown reactions can be devilishly hard to find because you do not know what you are looking for nor where to look for it.*

Which brings us to sentence 4, "The task for these advocates is clear cut: find the nuclear products." Here, the reason is revealed why conventional protocol requires evaluation of the observation. Those "advocates" who offer such claims are generally chemists with expertise in electrochemistry and calorimetry and occupy laboratories equipped to suit such pursuits. Proper protocol calls for their work to be reviewed and criticized by the scientific community to see if it was done sufficiently well to withstand the rigors of peer examination.

Huizenga has no interest in this process. He subverts it by demanding that these same scientists learn experimental nuclear physics and undertake the re-equipping of their chemistry laboratories with nuclear facilities. Meanwhile, they are to grow old in this alternative pursuit. Should they die without finding the source, then their discovery of excess heat is to be buried with them. Sentence 4 expresses this fatal distortion imposed upon the most ordinary methodology.

Sentence 5 is ambiguous. ". . . the reported intensity of nuclear products . . ." refers to nuclear products as though they were evident, thus carefully stepping over any reference to the products of unknown processes. Obviously, cold fusion scientists do not report that which is unknown to them. Huizenga deftly steps over this logical element in his public arguments.

In sentence 5, he implicitly denies the possible existence of unknown processes by allowing it no place in the logic of his presentation. For those readers who do not notice the missing element, his statements lead to a pervasive confusion about the field.

The final sentence 6 is coupled tightly to sentence 5. Five denies the availability of conventional fusion sources, and six denies the availability of non-nuclear energy sources. *Voilà,* there is no such thing as anomalous power.

What sentences 5 and 6 assert is that nuclear measurements are science, and calorimetric measurements are not science. Throw away their measurements and keep mine.† I wonder if there can be found in science a more nar-

* This expression includes the phenomenon claimed by Randell Mills, where the ash is postulated to be a reduced hydrogen atom.

row, a more provincial view of one's professional specialty than is held in these sentences.

It is generally accepted in science that claims of discovery must be formulated in language that permits the claim to be exposed to possible falsification by future experimental work. Similarly, it would enervate science to protect theory from critical exposure to further experimental results. All theorems and formulae secured by experiment are properly held falsifiable; they should always be positioned so that they must contend with new data.

Sentences 5 and 6 serve to protect current knowledge of nuclear science from contention with the new data that, in proclaiming the existence of anomalous power, appear to defy all contemporary scientific experience.

What we have seen is how the principal and in many ways most influential skeptic has carefully avoided the most relevant data. As far as Huizenga is concerned, calorimetric claims go straight to the dustbin. Oh, they can be left on the desk for a decent interval first, but let no one dare to draw conclusions from them. The full dismissal in principle of calorimetric data has been Huizenga's hallmark *from the beginning.* The following quote is worth repeating again:

> Although the McKubre experiment is considered by many advocates to be the premier evidence for excess heat, no nuclear reaction products were reported![6]

This denial of a scientific observation is based on attitude, not science. He shows no interest in learning what McKubre has done in his laboratory, nor does he show any interest in whether McKubre did it well or poorly. In either case, it is to be ignored.

I visited a bookshop on the edge of Stanford University after a visit to McKubre's laboratory in 1995. There I found the second edition of Huizenga's book. It was on my flight east that I read it and came to this credo at the end. After reading it, I felt affronted. During the first six years of the cold fusion controversy, he had simply *assumed* that all of the calorimetry was faulty, without exception. At that moment, I realized he had no grounds for this assumption. He offered no expertise in calorimetry on which to base his judgement. It was merely a professional habit, and hazard.

Huizenga maintained this assumption, out of view, from 1989 through his manuscript closing in 1993. Even then, his credo was buried in the appearance of an ordinary paragraph of commentary. After my realization of his

† Huizenga from time to time has supported this position by talking about the difficulties of measuring heat flow. Such talk of imponderables comes from a person with no claimed expertise in the subject and with no record of time spent in a calorimetry laboratory familiarizing himself with how it is done.

weak, unsupported assumption, I was sure that my nascent manuscript was moving along the right track.

Huizenga also spoke to the matter of data interpretation in the following categorical language.

> In the cases where the fusion products were reported to be many orders of magnitude less than the excess heat, nearly all the excess heat was assumed to be due to an unknown nuclear process. This point of view was first stated by Fleischmann and Pons.[7] Their assumption that the reported excess heat was due to some unknown nuclear process puts the responsibility on them to delineate the characteristics of such process.[8]

Again, Fleischmann and Pons did not make an *assumption* about unknown processes. They formed an *hypothesis,* one based on scientifically acquired data pertaining to anomalous heat *sans* radiation. The hypothesis followed from the data. *There was no other reasonable explanation for their data.* Certainly Huizenga can suggest no better explanation, nor does he volunteer that the law of conservation of energy be set aside, Karl Popper fashion.

Huizenga was also wrong about the assignment of responsibility. Other scientists with the appropriate skills and laboratory facilities would be more than willing to take up the nuclear delineation research once Fleischmann and Pons's anomalous power claims have been recognized and evaluated by mainstream science. Contrary to what Huizenga asserts, Fleischmann and Pons bear no responsibility to undertake learning experimental nuclear physics, nor need they go to their grave without claiming credit for the anomalous power that they spent ten years measuring. What was needed was the communal effort to review their calorimetric work, and Huizenga's loud skepticism has stood athwart that most natural, historical, conventional, and appropriate direction of scientific method.

Huizenga occasionally asserted that evidence of excess heat power was due to systematic error in all heat experiments. He may be right; after all, people who merely guess are sometimes right. But to support his point, he should quote citations from the extensive literature on calorimetry if only because it is outside the fields of expertise he claims as his own. Exactly what might those systematic errors be? Put them down in a paper and submit it for editorial review, and for peer review, and if the quality of the analysis is sufficient, his claim of systematic error will be published for evaluation by the cold fusion community. This is the scientific method. Science does not give the skeptic or the critic a free ride.

It is curious how Huizenga got his science backwards. He was indignant that two chemists would dare to advance the hypothesis that an unrecognized mode of nuclear process might exist, and demanded the nuclear products as proof of their heat claim. On the other hand, if that proof were forthcoming, he would then be willing to entertain the hypothesis that a Fleischmann and Pons electrolytic cell might generate excess heat power. He has placed the cart before the horse.

I am afraid that his approach to this field followed most like that of a pedant. As the Ichabod Crane of cold fusion studies, Huizenga stood before his class of unruly "believers" brandishing a fresh birch switch, demanding that Masters Fleischmann and Pons stand and recite their nuclear products.

Publications

Jerry E. Bishop, an intrepid science reporter for the *Wall Street Journal,* reported frequently on developments in the field of cold fusion research starting one day prior to its announcement in Utah. His reports came more often and were generally longer than those of most major newspapers, and he was not always writing as a skeptic as were other science reporters.

He was selected in March 1990 by the American Institute of Physics (AIP) as the winner of their annual science writing award for the year 1989. The announcement by the AIP, a professional umbrella group that includes the APS, that their annual award for excellence in science writing would be given to Bishop greatly annoyed the skeptics. I quote from Huizenga's book to sample the atmosphere of this little tempest.

> . . . I immediately contacted Professor Peter W. Trower, a physicist and one of the judges for the AIP award. In addition, I contacted the Office of Public Affairs of the APS [Park]. I was upset and particularly interested in learning about the criteria that were applied in making the Award . . . On March 20, 1990, Dr. Robert L. Park, Executive Director of the Office of Public Affairs of the APS wrote a letter to the Manager of AIP declining his invitation to attend the luncheon honoring Bishop for the AIP Science Writing Award. In addition, several other officers of the APS boycotted the ceremony (I declined my invitation to attend.)[9]

Kenneth W. Ford, the executive director of the AIP, had to plan his award remarks with special care. ". . . because the award was controversial, I wrote out my remarks with care and followed the text."[10]

I present the entire award text here so the unhurried reader may savor its ambiguities.

It is my pleasure to present to Jerry Bishop of the *Wall Street Journal* the AIP Science Writing Award for the best writing on Physics for the general reader by a journalist in 1989.

Jerry Bishop is a transplanted Texan who has been a distinguished science writer and reporter for several decades. His work has been recognized by numerous prizes in the past—by the American Heart Association, the American Medical Association, the National Association of Science Writers, and the AAAS. This year he won the American Chemical Society's Grady-Stack Lifetime Achievement Award. And this is his second AIP award. He won our science writing prize also in 1972.

His award this time is presented for his 1989 series on cold fusion in the *Wall Street Journal.* Jerry Bishop broke that story, with a one-day jump ahead of the now famous press conference held by Pons and Fleischmann at the University of Utah last March. During the weeks immediately following that announcement, he reported frequently on the claims emanating from Utah and from various other laboratories. The articles were well written, often under deadline pressures, and they conveyed to the general reader what nuclear fusion is about and what some of the cold fusion advocates were claiming. Beginning in May, it became increasingly clear that the Utah claims were without substance. Jerry continued to write on the subject and covered both sides of the controversy, although in the opinion of many (including, I must admit, my own opinion), he did not give as balanced a coverage as we would have liked, nor draw attention to the fact that the Utah researchers were violating accepted codes of scientific conduct.

I mention this concern because, as Jerry himself knows, and as many people in this room know, this particular award has become controversial. There are some, including the excellent panel of judges, who think that the clarity of his writing, and his consistent attention to an important and newsworthy topic, justify the award. There are others who greatly regret that he did not use his reportorial and writing skills to make clear to his readers—at least after May 1989—that there is no credible evidence for the claims of cold fusion. So I have to express the hope, Jerry, that you will not abandon the subject yet. It needs to be brought to closure, so that general readers understand what nearly every scientists now understands—that cold fusion as a practical power source is an illusion.

I am sorry for the roundabout route to this award presentation, but I could not let the disagreements that it has generated pass without notice. Whatever one thinks of this particular series, one must

> recognize Jerry Bishop as one of the finest science writers in America, with a long list of accomplishments. It is therefore my pleasure, Jerry, to present you with this award—consisting of a check for $3,000, a certificate, and a handsome chair guaranteed to encourage creative effort.[11]

This "hole" in the AIP's procedure was quickly mended after the presentation. As Huizenga explains it, "In response to the concerns of many of us, the AIP has changed its rules on the journalism award. In the future the awardee selected by the AIP judges will have to be approved by the AIP Board of Governors."[12] However, there is a footnote to his remark in the second edition of his book as follows "The AIP later reversed its decision on approval." The zigzag was indicative of the anxiety generated within the physics community by a distinctly threatening experiment created by scientists working in a different discipline. It also indicated that the outspoken skeptics were not necessarily representative of opinion within the APS and the AIP.

Bishop continued to report comprehensively on the field called cold fusion until the end of 1991. Reports on the emerging evidence for helium-four as the long sought for nuclear product were reported from the summer of 1991. The results of B. Bush and M. Miles experiments in series one were published. After that, WSJ reports became sporadic and then petered out within a year.

There are four scientific journals that are read by a broad audience. Scientists follow significant events in fields other than their own through these publications. Developments in cold fusion research were not known by most scientists because these four publications had maintained a silence on cold fusion developments for many years. I found in my travels and interviews that members of university physics departments were unaware of developments after 1989. It is appropriate, therefore, to illustrate some of the positions taken by these publications during the first ten years.

The critique of the Utah claims in *Nature* was aggressive, the journal becoming an immediate player in determining the destiny of the field. It was more patient than some of its audience in that it permitted itself twelve months before committing itself to an outlook dominated by the nuclear physics point of view. The failures were counted as well as the successes obtained by various experimenters, and thereby the journal obtained a useful measure of the experiment's reproduciblilty, which was poor, but not of its validity, which was good.

The journal gave preemptory consideration to particle and gamma ray detection, thus directing attention away from excess heat as the experiment's signature. It gave full credit to Fleischmann and Pons for their initial recognition of the very low level of neutron (and other) emissions compared with the excess heat. In the meantime, though, it had to protect the world of science from them.

Professor Oriani's experiment (Chapter 14) that was eventually published in *Fusion Technology* was first submitted to *Nature* magazine in September of 1989.[13] It was the first full-length report corroborating Fleischmann and Pons excess heat phenomenon. The journal submitted his manuscript to two referees who responded with apparently reasonable comments and questions. They could see no errors in the material submitted although they decried the fact that only 15% excess power was claimed. They were unhappy with the paper's statement, "although we do not understand the origin of the excess heat, we do not claim that some form of nuclear fusion is involved." They recommended the paper be published provided the raw cell data were included, more details on the calorimetry were presented, and more nuclear measurements were made. Oriani resubmitted his paper after amending it accordingly. In January he received a rejection letter signed by the U.S. editor. The reasons were that the sporadicity of positive results made the whole field suspect and there was an absence of theoretical understanding of the alleged phenomenon.

Oriani's paper ought to have been published in *Nature* to corroborate the Fleischmann and Pons claim for anomalous power. The scientific community then might have taken a more balanced view of the whole episode. *Nature* rejected it based on two wrong reasons: lack of theoretical underpinning and difficult replication. The first reason indicates failure to abide by conventional protocol. The second reason brought to mind the fact that within the decade *Nature* reported the several "Dolly" episodes as mainstream science, even though reproducibility in that kind of experiment ranged from one success in 50 tries to one in 227 tries.

After one year had passed, *Nature,* March 29, 1990, published an editorial, "Farewell (Not Fond) to Cold Fusion,"[14] the result of a poor sense of protocol and the many null reports of neutron particle emission. It is amusing to note how incredulous the editor was about the first Annual Cold Fusion Conference planned for that same week in Salt Lake City. "But there is a limit to people's patience, which has probably been reached with the organization of the first 'annual' cold fusion conference." *Nature*'s principal conclusion was that, "What has irretrievably foundered is the notion that cold fusion has great economic potential."

The Washington office of the journal played a strong role in the cold fusion saga, as might be expected. David Lindley, their Washington correspondent and associate editor, had a commentary of two-page length in the same issue. It was titled "The Embarrassment of Cold Fusion." He reviewed the current peccadilloes of the field, afterwards attempting to define a cleavage.

> On one side, non-believers see the negative results of Salamon, et al.,* as incontrovertible proof that nothing unusual is happening [in the cells]; on the other, believers take the same results as affirmation that cold fusion indeed demands new physics, which they knew already.
>
> The two sides are separated by a matter of faith, not one of science.[15]

This simplistic analysis overlooks the anomalous power data, and libels those who do not overlook that data as belonging to a new cult of "believers." Lindley cannot incorporate the actual presence of anomalous power into the claims of the previous twelve months. He apparently required an explanation for the source of the measured power. His "believers" tentatively accept the anomalous power data as an empirically valid measurement. The two sides, then, were separated not by differences of faith but by differences of science—the recognition or non-recognition of a well-measured observation based upon scientifically acquired data.

He brought his extended commentary to an end compounding his reasonable error of understanding by assertion of the ethical error of ridicule, "Would a measure of unrestrained mockery, even a little unqualified vituperation have speeded cold fusion's demise?" This is hubris.

In 1994, the retired editor of *Nature,* John Maddox, physicist, was interviewed.

> I think it's inevitable that it too has a persuasive, articulate, charismatic man who believes that he's neared a very important discovery that challenges scientific orthodoxy. It's inevitable that his students, his colleagues will have to take a view, are we for him or against him? And in a way it becomes quite quickly like the funding of the literature. You have a charismatic leader who persuades some people who become his followers and that they have seen the true light. And there's a large element of that, I fear, in what's happened to cold fusion.
>
> I think it will turn out that this is just . . . unconnected with anything to be called nuclear fusion. And I think it's come, broadly speaking, it's dead. And it will remain dead for a long, long time.[16]

He evidenced a complete lack of interest in the successful demonstrations and replications of anomalous power. All attention was directed towards evidence for the fusion reactions of nuclear physics.

* A null result from particle and gamma-ray measurements of some of Fleischmann and Pons's operating cells.

Scientific American shares its articles with sister publications in several other countries. The Nikkei Publishing company produces the Japanese version under the name *Saiensu* (Science). *Saiensu* devoted a two-page news article in its March 1992 edition to the research of Kyoto University scientist Akito Takahashi, a hot fusion nuclear physicist. He had recently announced success with a cold fusion cell that generated substantial heat along with continuous neutron emissions at a low level.

A cold fusion advocate in America garnered the signatures from several scientists to add to his letter to Jonathan Piel, editor of *Scientific American,* asking why it did not report similarly on the Takahashi experiment. The magazine's published commentary and Piel's letter response illuminate the isolation that was imposed upon cold fusion research by this reputable journal. The editorial, "Japan, Cold Fusion, and Lyndon LaRouche," is worth reading in its entirety, but I was not given permission to reproduce it.[17] It appeared in the May 1992, issue as a half-page box. The final paragraph of the editorial below gives the tone of their commentary.

> Fleischmann, when pressed, reluctantly confirms that he and Pons are indeed supported by Technova. Although he declines to reveal details about his work, he does note that "good information" on cold fusion can be found in *21st Century Science & Technology,* a journal published by followers of Lyndon H. LaRouche. LaRouche, who is now simultaneously running for president and serving a fifteen-year sentence for fraud, has previously claimed the existence of an international drug cartel run by the Queen of England.

21st Century Science & Technology is in fact superior to *Scientific American* if one wished to follow developments in cold fusion research, not withstanding arguments usually referred to as guilt-by-association. This editorial commentary leaves the magazine's subscribers ignorant of the provenance of the editorial—what caused it to be published. Nor are its readers informed of the availability in peer-reviewed journals of Takahashi's work and the work of Fleischmann and Pons. It employs the words secret and mysterious for matters that were not secret or mysterious.

The letter petition to the editor included as its second paragraph a statement of the current status of cold fusion research. Piel's letter reply (December 3, 1991) was brief.

> I am struck by the final clause of the first sentence in the second paragraph of your petition: ". . . even though its precise physical mechanism is not fully understood at present." Such a characteristic is typical of another kind of event in science, one that Irving

> Langmuir accurately described in a classic paper in the 1950s. You should look up the reference.[18]

The petitioner promptly replied listing the six Langmuir criteria, pointing out that "the mechanism is not fully understood" was not among them.[19] (His letter kindly overlooked the fact that there exists no such classic paper in the 1950s by Langmuir.)* Those familiar with the field of cold fusion studies will recognize well what Piel did then in response to the reply. Without notice, he abruptly abandoned the correspondence.

Piel's letter, its flippancy, its disdain, its intellectual indolence, and its rude discontinuance shows well why the American scientific community does not know about progress in cold fusion research since 1989.

Science, published by the American Association for the Advancement of Science, has been the most straightforward in its sparse reporting of the cold fusion news. An early editorial by its then editor, Daniel E. Koshland, Jr., was one of the best.[20] It discussed the factors and considerations important in evaluating the Utah claims. It was the first to recognize the importance of ". . . the relentless detail of electrochemical experiments and heat balances." The editorial gave appropriate allowance to the impossibility of the two chemists hiding the experiment from publicity until the answers were in. It did assume, however, that the experiment could be tested promptly. "The bigger the result the more quickly it is going to be checked." And it, ". . . will be checked in a very short period of time." The editorial was one of the most knowledgeable of the period.

Science has also provided coverage of developments better than the other broad-audience scientific journals. Its report of the *Physics Letters A* (May 1993) article describing Fleischmann and Pons operation of a cell at the boiling point for a period of time at an excess power of 144 watts, or greater than 1,000 watts per cm^3 of the palladium cathode, a level commensurate with commercial electric power generators.[21] The report said the power was four times the excitation, but did not mention the high level of the claimed power, or the power density, both important matters.

Unfortunately, the report was framed in such a way as to continue the sterile debate—does cold fusion exist?—instead of allowing itself to move beyond that to the question of the existence of anomalous power. The article points out that the editor of the referenced article, J. P. Vigier, would not allow any mention of cold fusion in the *Physics Letters A* article—fine. Why not have *Science* do the same in its report? Treat the *Physics Letters A* report as an elec-

* Langmuir gave a symposium that was audio recorded. The symposium record has been reconstructed and published in recent years, especially with the advent of cold fusion claims. It contains much that can instruct the skeptic.

trochemical experiment. Was it well done? Ask those qualified to answer in that discipline and in the discipline of calorimetry. Thus, the journal might report on the science in cold fusion research instead of helping to maintain a well-balanced antagonism.

A new editor was announced for *Science* about the time of publication of the first edition of this book. A copy was sent to him by a Harvard classmate of his who, like the new editor, also had lived through the vicissitudes often experienced by university presidents. Enclosed with the book were reprints from Goodstein, whom the editor knew, and quotes from Scaramuzzi, whom Goodstein introduces. A thoughtful reply was received in due course indicating the topic would be looked into. From time to time thereafter, one of the more interesting technical reports from the field would be sent along. Nothing was published on our topic in *Science* during the subsequent year after the initial contact. Never has this journal informed the professional scientists of widely corroborated empirical data as in the prolific corroboration of the anomalous power event.

The magazine *Chemical & Engineering News* (C&EN), is published by the American Chemical Society. It is a principal source of information about what was going on in other specialties. It expressed bewilderment as the events of 1989 unfolded, generally following the orthodox line of argument. It has differentiated itself during subsequent years, however, by giving its readers at least a little information on progress in the cold fusion ghetto.

That information was regularly smothered under what can only be described as a poverty of expression. It suffered from two stylistic postures that dominate its pieces on cold fusion research. The writers who were assigned to this difficult field used sarcasm freely, which did not promote understanding. And for information conveyance, they used a style disparagingly referred to as, ". . . the he-said/she-said form" of presentation.[22] I have not noticed any instance where the technology was presented. The casual reader would get the impression that those working in the field have obtained no scientifically interesting results.[23]

The four publications stuck closely to the establishment position. Evidently, the editors wanted to remain a part of the scientific establishment. If they accurately reported on cold fusion science from year to year, they might face derision from the establishment and the other journals. This reaction might have effected their advertising prowess. And there was always that desire to be comfortable, to be a part of the orthodoxy.

The best summary of Frank Close's book, *Too Hot to Handle: The Race for Cold Fusion* (1991) (Princeton University Press), was given by him at a press conference broadcast over BBC as a "Science Now" program for August 28, 1992. The occasion was the appearance of Martin Fleischmann as an invited and honored guest at the annual meeting of the British Association for the

Advancement of Science, which took place at Southampton that year. He appeared with Fleischmann to be questioned by the press. The press actually questioned Fleischmann, with Close having the occasional remark to add. Close made the following summary comment as the meeting neared its end. It seemed to express his philosophy concerning cold fusion more succinctly than any summary he offered in his book.

> What I heard this afternoon, by and large, was very much the same as I could have heard two years ago. We were told that many people are finding heat; we are told that people are finding neutrons, tritium, and helium. But what was not said is that individual experiments do not see heat and neutrons, tritium, or helium in amounts that would be required if a nuclear process is going on. When questions were asked to specify precisely who was doing this and who was doing that, no clear answer that I was aware of was really given to that. And, my summary still is the same that I concluded when I researched the first eighteen months of this episode, that there are no experiments in test tubes at room temperature producing watts of power from a nuclear process.[24]

The complaint parallels that of Huizenga. What Close wants to see in the Fleischmann and Pons electrolytic jar is nothing less than a nuclear power plant of reduced size, but otherwise fully functional, as might appear in a snow-shaker paper weight.

Beveridge describes a characteristic of the skeptic that seems to fit Close and those others who were watching the first Annual Cold Fusion Conference. As he puts it,

> It is not uncommon for opponents of an innovation to base their judgement on an "all or nothing" attitude, i.e., since it does not provide a complete solution to the practical problem, it is no use.[25]

This characteristic was illustrated in the account Close gives in the case of a nuclear physics paper from 1934.[26] The various fusion reactions were then being sorted out from one another. We have previously noted that one of Rutherford's assistant's, Philip L. Dee, prepared a paper that described a fusion reaction that seemed to happen at exceptionally low excitation energies.*

Fleischmann introduced that paper as evidence at the ACCF-1, March

* Using a cloud chamber Dee had noticed that occasionally d-d fusion gave tritium and a proton that left trails at 180 degrees from each other. This implied that the incident deuteron carried little energy, and the resulting fusion event was caused by a low energy source.

1990, as evidence that nuclear reactions can be excited at low energy levels. The reaction by Close is illuminating, although he was not there and Dr. Petrasso took his place. I quote below Close's explanation of the significance of it followed by Petrasso's actual comments at the meeting.

> . . . there is [a] much sharper reason why these pictures from the past offer no succor to aficionados of test-tube fusion. The fact that the images show clear proton and triton tracks emerging from the [solid] with their full energy shows that the *usual* fusion has occurred—the products have not been hidden from view.[27]

At ACCF-1 Petrasso explained it thus.

> Dee's work has nothing to do with test-tube fusion for the reason that (in Dee's photographs) the triton and proton [fusion] products come out and escape *with their full energy.*[28]

(In both cases the emphasis is in the original.) The point being that known nuclear processes cause high velocity particles to fly away from the reaction that caused them, but the Fleischmann and Pons cell has no corresponding particles flying out of the cell. Therefore Dee's account is of no help. This is the all or nothing attitude referred to by Beveridge.

It was Fleischmann's purpose in offering the paper to the conference to help solve merely a small piece of the source question. He is quoting Dee's paper only to illustrate that there exists an example of a nuclear reaction being instigated by a low energy source in the early nuclear physics literature. This is a thoroughly interesting and worthwhile contribution. Did it offer succor by illustrating a case of low energy nuclear excitation? Yes. Was it meant by Fleischmann to appear as the whole solution? Certainly not.

Taubes's Book

During late 1991 and early 1992, the ebb tide of cold fusion studies ran fast: no further public or professional support could be expected in the United States, the Japanese moves were not yet known, and developments in Italy, Russia, and India were not yet well reported. In late 1992, Gary Taubes announced the end of the cold fusion saga when he closed his manuscript and gave it the title, *Bad Science: The Short Life and Weird Times of Cold Fusion* (1993).* But cold fusion did not have a short life, nor were its times weird, nor was its science bad.

* Random House. N.Y., 1993.

Taubes's contribution was considerable. He did the heavy legwork needed to write the who-struck-John part of the story. Without his book, the history of the saga would have lost much.

Taubes assumed the establishment attitude that there was no such thing as excess heat. This assumption leads to the conclusion that each report of a successful experiment must be the result of either incompetence or fraud. His writing necessarily produced a classic muckraking investigation. Taubes displays this attitude most clearly by referring to the lack of neutrons commensurate with the heat as an *admission* on the part of Fleischmann and Pons.[29]

Taubes draws some odd conclusions after reviewing the first newspaper reports of the Utah announcement. He asserts that "A belief in cold fusion required an act of faith from the start."[30] There seems to be no awareness that Fleischmann and Pons were faced for four years with two elementary facts: they measured gross amounts of unexplainable energy, and they measured precious few neutrons. They were absolutely confident of both sets of measurements. Their data showed heat without neutrons. There was no act of faith there. I have to conclude that Taubes's book is unresponsive to the sheer intellectual force of experimental science.

Taubes did not seem to understand why Fleischmann and Pons advanced the hypothesis of unknown nuclear processes. He says, "From this evolved the working hypothesis for CF that something almost magical happened to the fusion process within the cold molecular lattice of the Pd."[31] He then waxes eloquently about the invariability of the natural laws. What would Taubes point to as a more likely source of the measured energy, or would he suggest that the new energy is appearing from out of nowhere? He did not see the challenge.

Taubes had published an article prior to the appearance of his book asserting that scientific fraud had been committed in a cold fusion research laboratory at Texas A&M University at College Station, Texas. It was the laboratory of John O'M. Bockris who was conducting numerous experiments on the Fleischmann and Pons phenomenon. These experiments reported the generation of excess heat as well as tritium.

Bockris's career had been distinguished indeed. Much of the current generation of electrochemists had been raised on his twelve textbooks on the subject. (They are written in a style unusually clear and straightforward in literary structure for a scientific textbook.) Bockris reported generating tritium, an experiment of considerable significance as tritium can only be made in a nuclear reaction.

Taubes wrote an article about this for *Science.* In it, the suggestion was advanced that the tritium in Bockris's laboratory was coming not from incidental contamination or electrolytic enrichment from the heavy water, as his critics claimed, but from the deliberate addition of tritium from a laboratory supply of tritiated water, in other words, fraud.[32]

The story is developed further in his book. A finger is pointed at Nigel Packham, a graduate student who was responsible for supervising the cells in question. The only evidence offered was that the tritium generation events seemed to be timed coincident with contract funding renewal intervals. The school conducted an inquiry and exonerated everyone involved. Certainly, the school's inquiry was superficial, but then Taubes offers us no substantial evidence either. So the matter was a tempest in a teapot but with much collateral destruction.

Taubes was incredulous at the lack of control cells in the Fleischmann and Pons experiment, as well as in other experiments. There was no hint in his text that the presence of an unrecognized nuclear process made the matter of finding a suitable control problematical. The alternative control method of balancing the energy in and out was not mentioned, nor that of using calibration pulses in an active cell.

Fleischmann demonstrated with a drop of red dye how well the contents of his flask was mixed by the bubbling action. Taubes comments on that demonstration as follows. "The demonstration was impressive; however, it was bogus . . . The temperature gradient in the flasks simply had nothing to do with what could be called the red dye gradient."[33] The video demonstration of how the dye quickly mixed was much more persuasive for the critics. One of them realized that the outside of the liquid was well insulated by the vacuum of the Dewar flask to allow the rapid mixing to eliminate temperature gradients. That demonstration was the best that could be done in one week's time with the available resources. Fleischmann and Pons published a paper two years before the close of Taubes's manuscript whose purpose was to answer such questions.[34] It states, ". . . (using ensembles of 5 thermocouples which could be displaced radially and axially) showed that the temperature was uniform to within ± 0.01 degrees throughout the bulk of the cells."

Taubes's argument on this matter led to another question: the responsibilities of the experimentalist and the skeptic. He said at one point, "It was no longer a scientist's responsibility to defend his research but the scientific community's task to defend its criticism. Cold fusion existed until proven otherwise."[35] Not at all. Defense of the research would come from replication of its results in at least one independent laboratory within the first six months. In the meantime, the standard for presenting one's arguments in the literature holds the same for both parties. Just any incidental chatter, as occurs constantly on the e-mail circuits, does not constitute scientific criticism. Standards as to what constitutes scientific discourse apply to all parties.

The double standard of limited and formal laboratory procedure,

SUMMATION

Errors Made in Response to the Utah Claims

The errors made in the evaluation of the Fleischmann and Pons claim of anomalous heat are as follows.

Errors of a technical nature.

1. The Department of Energy's failure to evaluate the anomalous heat experiments.
2. The careless assumption of calorimetry error in all the successful heat experiments.
3. The adoption of Irving Langmuir's name and terminology "pathological science" without invoking his technical criteria.
4. The criticism of cold fusion without commitment to a laboratory based, hands-on evaluation of the claim in question.
5. The inability of the scientific community to understand the proper place of failed experiments.
6. The scientific significance of difficult replication was woefully exaggerated.

Entirely surprising to those involved in the original announcement was error in the matter of scientific protocol.

7. The trumpeting of *absolute* appraisal exclusively to the press that were based upon uncertain experiments and analysis.
8. The early polarization of the evaluation process by means of ad hominem commentary.
9. Inordinate haste.
10. The animated attention given the claim of nuclear fusion to the virtual exclusion of the separate claim of significant power generation.
11. The scientific community beset those exploring this field with the slanderous accusation of "believer."
12. Finally, and most corrupting of all, was the precipitous institutionalization of opinion that there was no new science in the field:

a. During the first six years *Nature,* the *Wall Street Journal,* and *Scientific American* have adopted a rigid stance against reporting advances in cold fusion research.
b. The APS publicly and officially ridiculed the field for the first six years.
c. The Patent Office was absolutely rigid in refusing to consider patent applications on their merits.
d. The Department of Energy was utterly deaf to accomplishments after 1989.
e. In their 1995 publication "On Being a Scientist" the NAS makes several references to cold fusion research as an example of how science should not be done, but offers no comment about the proper conduct of scientific evaluations.

13. While the scientist is responsible for the correctness of what he has published, no member of the DOE Panel has argued that a further review was needed in light of the work done after 1989.

by which many scientists survive, could be misinterpreted by the science reporter. I believe that was what happened in the following quote from Taubes's book.

> The assumption seems to have been that there are two levels of scientific data; one that can be defended against a roomful of reporters and one that can be defended in a scientific meeting.[36]

There were indeed two levels, but they applied to the self and to peers as the respective audiences.

Taubes seemed to believe that science is some sort of a game, although I confess I cannot figure out exactly what kind of a game he had in mind. The straightforward idea somehow escaped him that two accomplished scientists, who reasonably enjoyed great confidence in their own laboratory techniques, had created an experiment whose data revealed the observation of a new world of scientific interest.

Considering its timing, the book provided a large audience with enjoyable reading, but otherwise had little tangible effect on the field.

This review of Taubes's book completes our presentation of what the skeptics had to say during the first six years. In all, they were successful. Their outspoken and oblique comments maintained the field of cold fusion studies in an intellectual ghetto.

After Twelve Years

As the tenth anniversary of the Utah announcement rolled around, Dr. Park, spokesman for the APS, reviewed a new book on cold fusion practice. Lessons from the preceding chapters are well illustrated by the form of the review. The book, *Nuclear Transmutation: The Reality of Cold Fusion* by Tadahiko Mizuno, tells of Mizuno's personal journey into the successful generation of huge amounts of anomalous power, and the subsequent search for the source of it. The latter led him into a claim of discovery of the transmutation of elements that was presented in Chapter 19 from his and other's technical reports.

The review in full is quoted below from Park's column, "What's New," as published in the Bulletin of the APS.

> THE UNDEAD: A REVIEW OF "NUCLEAR TRANSMUTATION." The subtitle of this thin volume . . . is "The Reality of Cold Fusion." The publisher is Infinite Energy Press, which probably tells you everything you need to know. This year marks the tenth anni-

> versary of the announcement by the University of Utah that Stanley Pons and Martin Fleischmann had achieved dueterium fusion in a simple electrolytic cell . . . Within a matter of weeks, a DOE panel officially pronounced cold fusion dead, amidst revelations of altered data and suppression of evidence. But the corpse does not rest peacefully. This personal account by one of a small corps who have not given up on cold fusion is wonderfully revealing—but not for what it tells us about science. "If you limit your goal to finding fusion products," Mizuno snorts, "anyone can see you will not learn much. This is why the focus is now on transmutation." He says of his fellow believers, "They have been treated like heretics by the rest of the scientific community. This has formed a bond of solidarity between them. Working with practically no funding against a tide of opposition . . . they have slowly but surely brought about a new discovery." It is an eloquent statement of how pathological science survives. In the final chapter Mizuno asks rhetorically, "What sort of reaction is cold fusion? As you have seen in this account, we still have no clear idea." After ten years, nothing has changed.[37]

If the nuclear equation has not changed, then nothing has changed. Is this a provincial attitude that only physics is science? What about Mizuno's results in chemistry and calorimetry? Pierre Curie also could have said, "What sort of reaction caused radium's warmth, I have no clear idea." Curie was awarded the Nobel Prize.

Park makes no reference to the excess heat that Mizuno achieved and described. He suffered, as did Piel, from a misunderstanding of both cold fusion research and of pathological science. The criteria of the field called cold fusion was characterized by the well-measured phenomenon of anomalous power followed by the search for its source. The substance of pathological science did not incorporate a lack of knowledge of the source of the questioned observation. In the review, Mizuno's accomplishments are not even alluded to. The review pretends that Mizuno accomplished nothing. It is in this way that the scientific community has been kept ignorant of progress in cold fusion research.

In his recent book *Voodoo Science, The Road from Foolishness to Fraud,*[38] Park announced its purpose: "This book is meant to help the reader to recognize voodoo science" (page 10). He includes Pathological Science under his umbrella title of Voodoo Science, and under Pathological Science he includes what he calls "cold fusion." Unfortunately, he never does realize his promise to help the reader recognize voodoo science except, of course, by following his markings. Park simply answers the question, Can it be explained by science? If it cannot be so explained and yet claims to be of science, then it is labeled voodoo science. He does not explicitly tell the reader that his definition of voodoo science is about any claim to scientific certainty that is not explainable by science.

Presumably, he does this to avoid bringing attention to an important weakness in his theme. In the special, but important, case of the advent of new science, how does Park separate real science from his voodoo science? The answer is that his method cannot help him. His method would be helpless evaluating Curie's 1903 claim of radium running warmer than its environment, a claim that defied all contemporary scientific experience. The 1989 "cold fusion" announcement at Utah claimed new science: a radiation-less source of nuclear energy that produced anomalous heat in the Fleischmann and Pons experiment. Park's method is helpless in the face of such a claim.

CHAPTER TWENTY-THREE

Un Cri du Coeur

By the end of 1994 it was clear to the few attentive observers that a new area of research had emerged, even if it remained ensconced in a scientific ghetto.

David Goodstein, vice-provost at Caltech, was one of those attentive observers. His institute played a determining role at the outset of the new field, although he had no part in that activity. As a provost, he was in a position to appreciate the potential for his institution to suffer disrepute in the current circumstances. I mentioned previously an article by him referring to Karl Popper's philosophy of science that appeared in *The American Scholar* in the Autumn 1994 issue. Goodstein chose to write at a time almost precisely after the anomalous power phenomenon had become well validated. No other article about cold fusion research was written at that critical point by an orthodox scientist of his academic rank and intellectual capacity. If we are to understand the orthodox position, his views are worth considering at some length.

Caltech's contribution to the cold fusion saga was extensive. It provided the chairman for the DOE's Energy Research Advisory Board, one member of its Panel on Cold Fusion, an historian of science who wrote a damning article for the *New Yorker* magazine, two professors who led the charge at Baltimore, tacit authorization for their ad hominem assault, and an extraordinary profusion of institute resource in support of their experimental program.

Goodstein describes one instance of Caltech's role, which was quoted earlier in part. His full statement gives an accurate assessment of the Baltimore event.

> For all practical purposes, [cold fusion] ended a mere five weeks after it began, on May 1, 1989, at a dramatic session of The American Physical Society, in Baltimore. Although there were numerous presentations at this session, only two truly counted. Steve Koonin, Nathan Lewis (speaking for himself), and Charles Barnes, all three from Caltech, executed between them a perfect blocked shot that cast Cold Fusion right out of the arena of mainstream science.[1]

Goodstein explained what nuclear fusion was and the various reasons why it was difficult to achieve. He then introduced Dr. Francesco Scaramuzzi, a highly esteemed nuclear physicist. Scaramuzzi worked as a senior scientist in the government laboratory at Frascati (Rome), Italy. Goodstein tells us of the significant contributions he made in the field of hot fusion. Goodstein knew him personally and vouched for him. Why did a highly esteemed scientist need vouching for by Goodstein? Scaramuzzi needed it because he had known sin: he practiced cold fusion research.

Goodstein was in something of a quandary about this circumstance. He told of a trip to Rome where he visited Scaramuzzi who had just returned from the fourth international conference of cold fusion scientists (ICCF-4, December 1993) at Maui, Hawaii. He sat in on Scaramuzzi's report of that meeting to fellow physicists. Goodstein was fascinated that scientists at ICCF-4 had found that a D/Pd ratio of 0.85 was necessary for the Fleischmann and Pons cell to work.* He expressed astonishment at hearing clear and precise science conveyed from a cold fusion conference. His article continued wonderingly, "I have looked at [Scaramuzzi's] cells, and looked at [his] data, and it's all pretty impressive." This was his quandary.†

There would be no quandary if cold fusion activity had promptly died away as expected in 1989: the University of Utah would have suffered a public failure, Fleischmann's and Pons's reputations would have been destroyed, and Caltech would have become a home for heros. The episode did not play out in that way.

Personal Exposure

A serene observer might have expected to see Goodstein take a continuing academic interest in what he had discovered through his Italian friend. If there

* The ratio is considered to be slightly higher now, nearer to 0.90 D/Pd ratio although there is some evidence it is lower at elevated temperatures (250C) and in certain palladium alloys, e.g., boron.

† Goodstein was acting as a critic. The skeptics of cold fusion research do not allow themselves to get in a position where they witness the data gathering. By this means, they avoid the quandary.

was science enough in the field for Scaramuzzi, Caltech's chemistry or physics departments might beneficially find space for a graduate student or two in these studies. A step of this kind, however, would seriously undermine the historic position of Caltech in the cold fusion annals. Goodstein explains that Koonin, and Lewis ". . . are my faculty colleagues, and I count them all among my personal friends of many years." The stakes were high for him. If he were to write that his colleagues were wrong at Baltimore, Goodstein would jeopardize his social, professional, and economic position, both at his university and in his field. He, his family, and those who chose to remain his friends, would suffer the effects of a sort of personal combat. He wisely decided to turn aside from his nascent interest in cold fusion studies.

When describing the field, Goodstein fell into the same elephant trap as have other physicists: he lacked appropriate expertise for the subject matter. He reiterated the difficulties governing the fusion of two deuterium nuclei in an extended argument of thirteen paragraphs. There was not even one sentence about calorimetry, the measuring technique employed by Scaramuzzi to collect his "pretty impressive" cell data.

Goodstein states, "All parties agree that . . . the primary event would have to have been the fusion of the two deuterium nuclei . . ." Does this statement reveal a lack of scholarship? There are four sources for knowledge about Fleischmann and Pons's claims: the Utah press release, videos of the event, patent applications, and the Preliminary Note. These four sources make it clear that the claims find conventional D+D fusion a small part of the processes generating the anomalous power, roughly one part in a billion. This scholarly aberration is best understood if Goodstein is viewed as writing only to the orthodox scientific community, where it would be correct to say that all parties were so agreed. It seems that he did not consider cold fusion researchers to be a part of his audience.

Goodstein's article might have caused a turning point: a reconsideration of the field by establishment scientists. The cold fusion research community is eager to show its results, but events at Baltimore served to prevent that possibility. The orthodox scientist might tout its achievements, but he will simply get bounced over the fence and into the ghetto: poor David, he finally fell for that cold fusion story. To open discussion within the orthodox community, he will have to speak to the assertions of Drs. Park and Huizenga *directly*: Is cold fusion dead, as they assert? Goodstein is properly fearsome of the stiff price such a challenge will exact.

Goodstein did not study-up on his interest in cold fusion before visiting Scaramuzzi and before writing his article. He would have known then about the D/Pd ratio's minimum value. It had been in the literature since June 1991. It is strange that he felt comfortable writing in the Phi Beta Kappa journal an article about the cold fusion episode without first reading up on the subject.

To understand him in this matter, we have to consider what might happen if he were to display a knowledge of that field, of its citations, its accomplishments, and its scientists? Would Goodstein not appear to be a turncoat? It is not surprising that these circumstances have in them the element of fear.

What did Goodstein learn from his visit to Scaramuzzi? A great deal, even though he was burdened with a misunderstanding of the role of ready replication and experimental recipes. He learned that the deuterium loaded titanium that was temperature cycled had produced evidence of neutron emission, that the Fleischmann and Pons type of electrolytic cells really do generate anomalous power, that Scaramuzzi considers cold fusion studies to be a valuable field in which to do research, that Lewis's criticisms of Fleischmann and Pons's excess heat measurements were repudiated, that light water as a control does not allow the generation of excess heat (using a palladium cathode rod), and that there is a threshold of 0.85 D/Pd ratio below which no anomalous power is generated.

The Silence

What did Goodstein do with this newfound knowledge? Did he, for example, write to one of the four broad-audience scientific journals advising them that their avoidance of cold fusion achievements was inappropriate? Did he, as vice-provost, inform the dean of science that research in cold fusion study areas might be undertaken as circumstances permitted? Did he inform the heads of the departments of physics and chemistry they might consider introducing graduate level thesis research in this area of study? It is not likely that he did any of these things. Instead, he wrote an ambivalent and maundering article.

When Goodstein learned, inadvertently, about the solid scientific work going on in cold fusion research his response was not unique. Earlier, I mentioned the three experienced electrochemists who visited the McKubre laboratory at SRI, Menlo Park, California, during the years 1990 through 1994. They were A. Bard, (University of Texas, Austin, Texas), H. Birnbaum, (University of Illinois, Urbana, Illinois), Richard Garwin, (IBM, White Plains, New York), and N. S. Lewis, (Caltech). Each spent several days examining McKubre's laboratory practice in detail.[2] They found no procedural error with the measuring technique or data reduction techniques used to evaluate the operating performance of the cold fusion type cells. They had no contractual obligations either to reveal or to keep the things they learned confidential. Nevertheless, they chose to say nothing to the scientific community.

Dr. John O'M. Bockris, distinguished professor of chemistry at Texas A&M University, College Station, Texas, ran cold fusion cells during the summer and fall of 1989. He reported excess heat and tritium, but the results were sporadic. At last, he came to a point where he had a cell that ran continuously for three weeks. It was time to call in some of his critical colleagues in the department who knew what he was attempting to do, so they could witness his results. The first one to be invited explained that he was busy moving from one house to another and could not spare the time. The second explained that he was simply too immersed in an examination schedule to break away, and the third just happened to be leaving on a trip shortly, so sorry. This inference of fear was a continuing pattern.

Dr. Huizenga visited the cold fusion laboratory at California State Polytechnic University, Pomona, California, on February 28, 1997. At this time, he was retired. He was visiting at the invitation of the physics department to speak against the cold fusion heresy that was alive in their department.

Drs. Robert Bush and Robert Eagleton, full professors in the department of physics, were running light water cells. Bush was Huizenga's host in the laboratory. In Bush's words, one cell was, ". . . evidencing excess power. And, while the gain (P_{out}/P_{in}) was rather modest at that time (about 1.12), the excess power was well outside the possible error bars . . ."[3] Huizenga was invited to spend time taking data. Huizenga demurred. Bush invited him to return on another date and do so. Huizenga demurred. *Bush then offered him a fellowship to cover the expense of a return visit.* Huizenga demurred. He refused all offers to participate in the experimental work in accordance with the manner of Drs. R. W. Wood and Irving Langmuir in the cases of Blondlot and Barnes respectively.

These illustrations of avoidance of the laboratory are representative of the intellectual climate ten years after the Utah announcement. If the reader feels that I have belabored my theme too long, let me say that, prior to his Italian visit, Goodstein represented the intelligent, knowledgeable, and cosmopolitan American physicist in his ignorance of cold fusion research after 1989, and in the audacity with which he has written and spoken about it without troubling to read up on the subject beforehand. What Goodstein learned was that, except for Petrasso's well founded criticism of Fleischmann and Pons's nuclear measurements, *Baltimore was bogus.* Cold fusion research was not a pathological science. The assault of Koonin and Lewis was mistaken: Fleischmann and Pons were not incompetent and delusional. Indeed, evidence of a new means of generating energy had been found in the flow of anomalous heat power that defied contemporary science.

Did Goodstein continue his learning about cold fusion science to see what more the field had to offer? Did he recommend that other scientists look at the field? He seems to have denied his scholarly faculties the leeway to resolve his quandary. For an accomplished scholar, his article is without intellectual resolution. I conclude that the article in its essence asks, How can I

(Goodstein) look further into cold fusion research without being consumed by the consequences of doing so; if I announce that Koonin and Lewis were wrong at Baltimore, and that in their hubris they did lasting damage, not only to Fleischmann and Pons, but to Caltech and to an incipient science, how do I survive in one piece?

The article is *un cri du coeur* from a member of the scientific establishment: what have we wrought with our antics, and what are we to do about repair? So far, the answer to his article has been a comprehensive silence from the scientific community in the United States.

I have concluded that fear is an important force in preventing scientists from looking into cold fusion studies even as an academic exercise. Progress will have to be made in the ghetto without help from orthodox scientists, even those with open minds. There is one source of help, though, from outside the ghetto. The silent partner in this controversy made provision that anomalous power would continue to register on the calorimeters even as their accuracy was much improved. Presumably that help will continue.

It is my thesis that this climate of fear was put into place at Baltimore by the mistaken attack on Fleischmann and Pons's mental acuity and by the savage and ignorant criticism of their calorimetry. After those eminently successful attacks, who else dared to risk suffering from such public ruthlessness?

The scientists who constitute the cold fusion field of study have been introduced earlier to the reader. The scientist's employer is also of interest to this account, be it corporation, university, government agency, or institution. None of these institutions came forth to endorse cold fusion studies in spite of the presence of 100 or so technicians devoted to the field.

This difference between the individual and the institution derives from the ability of the individual to behave in a coherent manner, even about complex affairs. Institutions generally lack this ability; they suffer from institutional incoherence. The individual can more or less simultaneously take into account his career, his relationship with his peers, his family, and altogether arrive at a commitment to devote some years to cold fusion research while accepting the associated contumely. For an institution to make a similar commitment would require the alignment of the multitudinous interests of dozens, perhaps even hundreds, of officers, ranking staff, the board of trustees, and possibly a board of regents. This is an impossible task with such an heretical topic.

Imagine the doctoral degree candidate with a dissertation that had been fulfilled under a professor who was active and successful in cold fusion research at Grand University.* The candidate accepts the reality of anomalous power, as he had measured it in his own experiment. He thus produced inter-

* This illustration is derived from an actual instance.

esting and publishable results in the course of his studies. These were incorporated into his thesis and dissertation. In due course, he successfully defended his dissertation, the last step towards his degree. While you might then expect the candidate would be awarded the degree, institutional incoherence stood athwart that outcome despite the quality of the candidate's work.

Consider the plight of the department head who planned to approve the degree award. He would be questioned by the department's members: does our *department* really believe that cold fusion is true; does the department assert that nuclear fusion can actually be sustained in a jar sitting on a bench top; is it really so much smarter than Harwell, MIT, Yale, and Caltech who demonstrated that it does not work; is it possibly suffering from pathological science; does the department head understand that the vast majority of scientists in America know that cold fusion studies are a farce; and, always, should the department risk its hard earned reputation?

Imagine that this department head is an unusually stalwart fellow who was willing to accept that departmental burden, and then proceeded to affirm the Ph.D. degree award. Each of these same questions then devolve upon the dean of science from an ever-widening audience. If the dean bravely affirmed the award, then the same burden fell on the president. If the president decides to support the degree award, then the trustees would soon be asking, why was Grand University standing alone in seeing the "truth" of cold fusion studies; something must have gone wrong. What about the reputation of our university? The regents, philanthropists, and alumni would inevitably be forced to reconsider their support of the university. The recognition process would progress in this manner with any institution of established rank and reputation, be it a government agency or a private corporation. Several hundred scientists could each be dedicated to the field of study, but institutional incoherence will work to insure that not a single institution would acknowledge the legitimacy of the field.

Institutions develop coherence as a reflection of opinion expressed by the scientific establishment and in the mass media. This is why the public assault at Baltimore and in the *New York Times Sunday Magazine* did such lasting damage to the evaluation of the Utah claims. After those assaults, who would be so reckless as to engage their institution, as an institution, in the study and evaluation of the Utah claims?

Science Reporting

Much of my reading in the preparation for this book included stories written by science reporters who covered the cold fusion episode. The Baltimore meeting was a supreme science story with 100 of America's most distinguished science reporters present. I expected to see a kind of reporting that was different from what I usually read in newspapers and magazines. I was disappointed.

The elements that appear in a scientific article ought to include those things that make science unique. There are the claims, the procedural protocols, and there are the experimental outcomes. A science reporter can be expected to have a well developed sense about protocols, and to allow for their presence in his reporting.

Reporters Deborah Blum and Mary Knudson edited the book, *A Field Guide for Science Writers.* Could they offer reasons why reporters were not more effective in explaining the episode of cold fusion as it was played out? One of the protocols involved the review of a scientist's work by his peers. Here was what one of the book's science reporters had to say about the peer-review process.

> Is it good science? Here the single most useful guideline is science's own peer review system. The system is not perfect, but it's the best that's available . . . Why it is critically important to find out whether a scientific claim or advance has survived the peer review process. It is incredibly easy for scientists . . . to come to believe passionately and honestly that something is true when, in fact, it is false. The peer review system is a time tested way . . . Science writers who ignore the system risk misleading their readers and embarrassing themselves.[4]

All in all, a fine statement. It continues a few sentences later.

> One of the most famous instances of non-peer reviewed science getting into the mainstream press was "cold fusion." Two scientists claimed to have invented a tabletop device that caused nuclear fusion at ordinary temperatures, yielding more energy than was consumed in the process. Ordinarily a scientist would repeat the experiment and, . . . submit a paper to a peer reviewed journal. Only after it was accepted and published would the press hear about it.[5]

My readers know that the two scientists had been working on the experiment for five years and that they had no trouble repeating it. Their Preliminary Note did pass peer-review, and so forth. The writer of these quoted paragraphs, while he recognized that, "the [peer-review] system is not perfect," did not warn of those situations where it actually breaks down. In this case, a new sub-specialty of science was being created by their claims. Where do you find someone qualified to review the claims of anomalous power?

Continuing the above quote, "But not these two scientists. Amazingly, they called a press conference before they had even figured out how to reproduce their own results."[6] The reporter was incautious here. He did not realize that some experiments were difficult to reproduce, but those experiments were still science and possibly important science. As noted earlier, the first cloning of the sheep Dolly involved 227 failed attempts and only one success.

Continuing. ". . . most good science writers conducted their own peer-review process, asking physicists to comment, . ." I infer from this that *chemists* need not be consulted. Here the reporter had uncritically embraced the nuclear physicist. He should have positioned himself better to see if those physicists were responding to the real claims, or whether were they putting up a straw man, perhaps in order to protect their hot fusion funding. A reporter can best maintain his self-orientation by developing a *direct knowledge of the original claims,* which were available to him. From that stance, he could report insightfully about the evaluation of those claims.

To continue his description of the reporting, ". . . notes of skepticism attended most of the cold fusion coverage, except for that in the *Wall Street Journal,* which to the astonishment of many science writers, climbed aboard the bandwagon." The reporter misunderstood what the WSJ was doing. It did not climb aboard the bandwagon. It consulted not only with physicists but with chemists as well. That was the essential difference.

In my own reading of the science reporting, I saw mostly political reporting about a scientific subject. It involved a substantial explanation of the science that was followed by an in-depth report on how the sides were divided, on whose side had the big guns, and the preponderance of votes. The *political* reality was that, ". . . the vast majority of experts long ago dismissed it . . ."

Getting It Right

There are a few cautionary rules for reporters once the chase is underway. Do not assume that the scientific establishment is as pure as the driven snow, that it has no other purposes than to help sort out the science. I have run into themes of punishment for slights, noble motives of protecting the public, protection of funding, and myriad other reasons for putting down the claimant, all of them quite separate from the question of the validity of the claims.

Scientists are people subject to human failings. The institution of science is as likely to fail as any other social institution. We take it for granted that catastrophes occur even in our most important and carefully run institutions: major banks occasionally collapse in bankruptcy; schools fail to educate children; a surgeon cuts off the wrong leg, a house is built on the wrong lot. Most of the time our institutions function well, but no one thinks they are perfect. For some strange reason, reporters have convinced themselves that the profession of science alone has somehow achieved perfection.

There is wide variety in science. It stretches from the physicist, to the geologist, to the biologist, and beyond. The nuclear physicist may demand a recipe for replication of an experiment, but that demand may be peculiar to nuclear physics. To a considerable extent, the nuclear physicist does not look upon the "Dolly" event as following the protocols required for science. The reporter who must jump from field to field in his reporting should be keenly aware of this variety of protocols. He may find a number of the references in this book helpful in that regard.

According to another science reporter, "[Science reporting] often requires the skills of a good police reporter".[7] I would emphasize that comparison. The police are often constrained in their actions because they cannot get away from the fact that a crime has been committed, and something has to be done about it. A comparison, if it is not driven too hard, will help to elucidate the rules. There was an old tradition attached to the "police blotter," a genre sometimes called crime reporting. The reporter's job was to do much more than print the official statements passed out by the police department or the mayor's office. The reporter had to learn the circumstances of a crime, and watch the police behavior with a critical eye. There was no suggestion in this that the "crime" reporter knew better than the police, only that he was independent. He had to keep the whole picture in mind, and think about how things fit together. This kind of analysis of the content and sequence of events was not evident in the large volume of reporting on the cold fusion saga during 1989.

The best way for a reporter to learn exactly what the claims were was to read what the proponents said. These claims could then be followed to see if the scientific community was seriously evaluating them, or merely grandstanding for its own diverse purposes.

Another step in the right direction comes with identification of the science sub-specialty of interest. This should be done with care if the reporter wants valuable consultations. The global warming debate has been marked by such choices made by the different sides when planning interviews. The choices in the matter of cold fusion were largely between physics and chemistry, but the choices between nuclear physics and solid state physics, calorimetry and electrochemistry were also important.

It is particularly worthwhile to report about critical experiments published in serious journals, especially when the matter is controversial. No one field of science has a direct channel to interpret natural phenomena and can know for certain who or what is right. The sophisticated reporter is always aware of nature as a hidden partner in science; she can trump any argument mankind might devise.

The influence of the science reporter was a strong one as the cold fusion episode entered its twelfth year. The American scientific community remained ignorant of the experimental developments in the field. The reporter would recognize the fact that professors of chemistry and physics in the most prestigious research institutions are quite unaware of what has been achieved in the years since 1989. Reporters making inquiry can be careful in selecting the expertise they need to consult. They can, for example, ask the expert interviewee the extent of his familiarity with cold fusion literature and research, and pass that on to the reader. The best source will be a scientist who is up to date on the literature.

This is the place to turn, finally, to a resolution and conclusion regarding the many contentions in the field of study called cold fusion.

CHAPTER TWENTY-FOUR

Resolution

The Utah announcement in March 1989 defied all contemporary scientific experience. The claim for discovery of anomalous power elicited responses that were immature in their haste, ignorant in their assumptions, wrong in their conclusions, and, ultimately, corrupt in their protocol. The claim to have measured neutrons, conversely, attracted criticism of an entirely proper form—an error of procedure in measurement enabled the skeptics to correctly declare that the claim, as presented, was in error. For the principle surviving claim, that of anomalous power, the skeptics assumed that a flaw would be found in the calorimetry measurements. When that expectation failed them, they dissembled. They pretended for years and years afterwards that mankind did not know how to measure heat flow. A hoary and notorious protocol of science sustained their pretense: the empirical evidence of the chemistry laboratory would be spurned, as earlier skeptics spurned Galileo's telescope. The skeptics refused the laboratory and its literature of excess heat. How did skeptics go about their dissembling ways where data sets that demonstrated anomalous power were available during the 1990s in some abundance? How did they refuse empirical evidence while keeping their respectability intact within the halls of orthodox science? Entreaties from the curious about anomalous power measurements were answered by demands for concomitant nuclear data. The skeptics held the proffered heat measurements hostage to unavailable nuclear data. This pattern of behavior hid from the professional scientist the aggregating empirical evidence for anomalous power.

Much in the manner described by Ludwik Fleck, the *genesis and development of a scientific fact* unfolded during the decade of 1984–1994: discovery of the anomalous power phenomenon—excess heat. The Fleischmann and Pons experiment further required years of development to attain reproducibility, much as have the recent cloning experiments in biology. Physicists particularly have refused an allowance of time for the attainment of reproducibility. Their strict protocol produced a false negative evaluation, and doomed the field to a decade and more of confusion and obfuscation. The wrong paradigm of scientific methodology led astray the evaluation process.

Aristotelian argument rejected things it did not understand so as to limit its discourse to topics it believed it did understand. The extensive corroboration of the phenomenon of anomalous power was rejected by critics on the argument that it was not understood: Where is the reaction equation, or its products? This dismissal vitiated the protocols of post-Aristotelian science.

Galileo announced that he had seen mountains on the moon in the knowledge that he did not know how they came to be there. In this announcement, he let out the first bawl of modern science. From that moment, not only would science admit into its sanctum things it did not understand, it would make those things be objects of its most devoted attention.

It required expertise in calorimetry and data reduction to critique the many excellent excess heat reports that emerged in 1990–1994. We have the Fleischmann and Pons's seminal paper of July 1990, the Wilson, et al., critique published simultaneously with Fleischmann and Pons's defense in July 1992. These three papers are a quintessential expression of the proper methodology of science, and I commend them to the professional scientist.

That mode was rejected by the skeptics. Their names as authors appear no where on peer-reviewed papers of that period (with one exception)[1]. When there emerged extensive sets of measurements that purported to demonstrate a new source of energy, the American scientific establishment failed to evaluate them. The APS and the ACS, for example, have not provided technical review committees of experts.

Individual physicists, acting as skeptics, undertook to represent the scientific community. They set aside the measurements of anomalous power without argument. *Nature* journal led the pack when it wrote, "Both sides in the debate seem to accept that the heat measurements will probably not prove convincing one way or the other, and that the presence or absence of nuclear products is the crucial evidence."[2] Huizenga referred to a list of experiments reporting excess heat by insisting, "This list, however, doesn't contain a single entry where the claimed heat is accompanied by commensurate numbers of fusion products."[3] With that, the representatives of the scientific community would ignore excess heat measurements regardless of their quality.

A review of the skeptical writings over the past twelve years shows a lack of comment on the topic of anomalous power. Books by the skeptics include no bibliographic reference to empirical data after September 1989. One skeptic, who purchased his copy of this book in May 2000, reported to the fusion physics community that its title was "Why Cold Fusion Prevailed,"[4] and thereby revealed to all a lapse of intellectual integrity. We have mentioned previously how a tenth anniversary newspaper article reported that McKubre proclaimed the abundance of anomalous power reports that are "virtually without challenge," in the peer-reviewed literature, while Huizenga declared that the field of cold fusion is dead.[5] In the manner of these four examples, various dissembling mechanisms successfully locked out empirical evidence from evaluation by professional scientists.

Pierre Curie announced his empirical evidence for anomalous power discharge from radium and was awarded a Nobel Prize. Similarly, the empirical evidence for an astonishingly rapid expansion of the universe is recognized as requiring explanation by the cosmologists.[6] There was no assertion from scientists that these two examples were pathological because of the lack of causal information. The empirical data in these two examples was not hidden from view pending some additional knowledge. Science requires only that there be no procedural error in the measurements.

A skeptic's wary thought process illustrates a concern with undetected error.

> I have often looked at experiments which gave results that appeared to violate the laws of Nature . . . the fact that I had not detected the flaw, did not mean that the experiment was correct and that the laws of Nature had been violated.[7]

But if it were necessary to violate the laws of nature to generate excess heat, then none would be generated. On the other hand, if the measurements are incorrect, then an avid pursuit of the "science" must in due course explicitly and particularly reveal that incorrectness. "Undetected error" may be a stopping point because of penury or fatigue, but it is not a scientific stopping point.

Cold fusion research is a respectable scientific activity founded upon the empirical phenomenon of anomalous power. This new field warrants the fulsome attention of science and the government because of the importance of a malleable type of low energy nuclear reaction to science, of a possibility of radioactivity remediation, and of a potential source of energy for society.

In America, to hide important information from the public is now referred to as a "cover-up." When the excess heat did not disappear with the introduction of precise calorimetry, skeptical physicists adopted a cover-up behavior towards it. Huizenga's book omitted references to the topic for occurrences after June 1989. F. Close did not include such references in his book. Nor did the DOE Panel report them after September 1989. Morrison omitted the words "excess heat" when reporting the title of this book to the fusion community. Even Goodstein refers to Scaramuzzi's "impressive data" without allowing one sentence to reveal that he too had obtained excess heat from the electrolytic cell. In this manner, the extended corroboration of the discovery of anomalous power has been kept hidden from the professional physicist and chemist, and from the public. No cover up like this has happened before. It is a profound scandal in American science.

There has been virtually no controversy in the science itself, while at the same time there has been great controversy in the public domain. A scientist performs the electrolytic cell experiment after the example of Fleischmann and Pons, and successfully demonstrates the generation of excess heat. He submits a report of his experiment to an established journal of science where it passes peer-review and is published. During subsequent years the publisher receives no assertion of procedural error to invalidate the published paper. The report is established as valid science. However, let that same scientist try to use a government auditorium to discuss his results with a peer group, or let him try to patent his innovative cell configuration, or let the government try to award a research grant to this scientist to extend his experimental work, and it is likely that the sky will fall down. The arousing cry from the skeptics will be "almost all scientists agree there is no such thing as cold fusion." With that demagoguery and follow up efforts, permission to use the auditorium will be revoked, the patent office will refuse the patent, a select panel will be appointed to revoke the grant award. In this strange fashion, the controversy has been played out for twelve years, violating the most ordinary, well-established scientific methodology. Thus peer-reviewed publication counts for nothing, and demagoguery counts for everything.

And the skeptics response to determined questions asserting the presence of published, peer-reviewed, yet unscathed, reports of excess heat, after dismissal of the "where are the nuclear products" gambit, generally turns to rumor-mongering those reports: the peer-review was incestuous, the reports are incomplete, the experiment is not reproducible. These are criticisms that the skeptics dare not address to the publishers for review and publication.

The indictment against the skeptics is perfectly straightforward. It is not that the skeptics ought to accept the calorimetric data as valid; it is perfectly proper for them to reject it if that is their considered conclusion. What is unconscionable is that they have hidden their decision from the scientific community. Their decision does not appear anywhere in the peer-reviewed literature of science where it can be properly acknowledged and critiqued by other professional scientists. This is another of the skeptics errors of scientific protocol. I have on occasion seen Park's book, *Voodoo Science,* used as a reference in such matters. But Park is only guessing. There is no methodology in his book wherein he is enabled to separate pseudo-science from new science.

SUMMATION

Skeptic's Errors of Protocol

1. Avoidance of Peer Review

Publication of empirical data in peer-reviewed journals is left un-responded to by the skeptics. That data stands virtually unchallenged in the scientific literature. The skeptics have nothing to say about cold fusion research in those venues that require editorial-review and peer-review of a critque. They carefully avoid exposing their opinions to the most accepted of scientific procedure.

2. Expression of Contempt

The skeptics, in their arguments with cold fusion practitioners, quickly resort to expressions of contempt. This behavior necessarily closes the doors to dialog. Personal attack, along with the use of slander and ridicule, are serious violations of scientific ethics and are harmful to science.

3. Lack of Evaluation

With the proper experimental conditions well-met, and with an exceptional experimental result, the scientific community has failed to evaluate the published work. Instead, it has allowed a demand for nuclear products, better replication, theoretical underpinning, and intellectual understanding override normal protocol.

A Retrospective View

This book was written by one who loves science. I wrote it in the hope that this new field of scientific research, misnamed cold fusion, might become known to mainstream science rather than being known only to a small cadre of scientists working in an intellectual ghetto.

Throughout history, a lack of available energy to substitute for human power has been the source of much human misery. A technology that made available a local source of heat energy for family use would bring about a revolution in human well being. It would solve one of the world's worst environmental hazards: the scraping bare of the wooded growth of the landscape for fuel. So far, the study of excess heat offers a potential technology in low quality heat sufficient to substantially augment world energy resources.

Do I anticipate that those scientists who have followed my narrative this far will agree that a well-measured observation of anomalous power began in 1984–1989? No, perhaps not. The history of science is replete with many who have gone to their graves refusing the latest turn in the course of discovery. Some will find my extended concern with the methodology of science to be an unfortunate digression. Others will be dismayed at my recognition that the strict criterion does not bind all of science. As for the rest of us, What was learned?

To answer that question, it is best to look at what was announced, while investing that look with the power of retrospection. At the University of Utah in March 1989, there were in effect three announcements, each independent of the other two. First, Fleischmann and Pons claimed that a sustained deuterium-deuterium fusion was achieved. Their measurements were poorly done, their claim was reasonably dismissed, and the two chemists turned to a redesign of the experiment. They later published data showing neutron emission from their excess heat experiment, but that experiment has not been corroborated.

The second independent claim was that of the appearance of massive amounts of unexplained heat energy in their cell—megaJoules of energy. That claim of excess heat was well corroborated during subsequent years. They postulated that the source of that energy must be an as yet unrecognized or undiscovered nuclear process. Studies undertaken to find a nuclear source reaction for the heat production concluded with evidence for the generation of helium-four in amounts that corresponded to the excess heat, as though that reaction were the only branch of deuterium-deuterium fusion with its 23.8 MeV energy discharge and the discharge were absorbed by the lattice as heat.

The third claim was for recognition and hypothesis of a class of nuclear reactions that release useful amounts of energy while not producing the well-known lethal radiation usually associated with nuclear power. That lack of penetrating emissions continued to be a characteristic of the field during the ensuing decade.

Any one of these three claims standing by itself would justify a major public announcement and, if confirmed, be considered revolutionary by the scientific community. For one press conference to announce all three simultaneously was simply beyond belief. That the three claims were intertwined with one another assured confusion on the part of the most level headed scientist.

There were at least two additional sources of confusion. Fleischmann and Pons presented a classical chemistry experiment that now had the word fusion bonded to it. The strict criterion of the nuclear physicists would unfortunately be applied to a discipline foreign to them, namely chemistry, with disabling consequences: those who would speak for the orthodox position of the nuclear community failed to develop a functional acquaintance with the experiment. They knew, *a priori,* that there was no need to get their hands wet in the laboratory.

The second source of confusion resided within the Fleischmann and Pons cell. The cathode consisted of a piece of commercial palladium rod or sheet metal. As such, it could not be exactly reproduced, thereby limiting the reproducibility of the experiment. Without exact reproduction, the strict criterion produced a false negative response to the second and third claims. Once the skeptics were wedded to this conclusion, there was the natural tendency, as satisfactory reproducibility was attained during the ensuing years, for them to avoid public admission of an error in evaluation.

The outspoken nuclear scientists stood on their demand for only nuclear data; the cold fusion scientists were confined to their empirical results in the discipline of chemistry. The chemistry profession, as such, was nowhere to be found. The two schools of scientists, in the essence of each, did not contend with one another; they merely took different kinds of stands: "Cold fusion is as dead as it ever was," and "the existence of anomalous power is virtually without challenge."[8] The strict criterion placed anomalous power outside of science, namely, in chemistry, although none of the skeptics would say so for the public record. They merely repeated the statement that cold fusion was dead. The scientific community was thoroughly confused by this. The controversy was not really a *scientific* controversy when articulated in this way. It was one of attitudes and manners.

If the cold fusion contention were about the quality of the excess heat measurements—were they definitive, and if not, why not—about, for example, the need to subtract two large numbers to obtain the claimed excess, there would be no story to be told and no book to be written. The issue would have been about the proper interpretation of a succession of measurements—about science. Unfortunately, the contention was political, not scientific.

The skeptics did not say that the excess heat measurements were inadequate; they simply ignored them as though the laboratory work did not exist and the scientists doing that work were dead. The experiments were not reported in the books by Close, Huizenga, Taubes, in essays by Park, or in ICCF reports by Morrison. In this way, they created and maintained a circle of silence around the cold fusion ghetto, one recognized by all involved as a profound refusal of orthodox scientists to communicate in any reasonable way with the practitioners. They characterized the controversy not as one about the adequacy of the observations but as one carried on between science and foolishness.

A scientific controversy about cold fusion would have been a continuing debate about calorimetry, its capabilities and limitations: how to measure heat accurately. But the skeptics always expressed themselves in such a way that the heat claim was not directly referenced. They spoke in ambiguous language: one was never sure whether the term "cold fusion" was meant literally or as the name of the field. Other professionals and science reporters saw fit to not enter the fray with questions aimed to lay bare these meanings. The result was a political controversy involving clever maneuver, rather than a scientific controversy about the quality of the calorimetric measurements.

Nature, Science, Scientific American, and *Chemical & Engineering News* ought to have started reporting the many corroborations of heat after the aggregation of evidence became clear at the end of 1994. American science was harmed by the indefinite delay in publication that allowed the scientific community to remain unaware of that advancing state of the art in the generation of anomalous power.

Due Discipline

For the last time, we revisit the keenest expression of the confusion. The following quote comes from a book whose manuscript closed in July 1991.

> . . . it missed the heart of the controversy, where the reported magnitude of the fusion products was more than 8 orders of magnitude (10^8) less than the reported magnitude of the heat. All critical scientists, including both chemists and physicists, realized the importance of this very large and damning inconsistency.[9]

The power measurements, with no radiation, were evidence of a new kind of nuclear reaction whose products were unknown. The attention of the scientific community was turned away from its duty to evaluate the claim of well-measured anomalous power. That led, in turn, to a perpetuation of confusion about the legitimacy of the anomalous power claims.

My insistence that it was the most conventional duty of the scientific community to evaluate these claims does not constitute an idealistic and impractical attitude. Confusion reigned because of a failure to follow conventional protocol, thereby thwarting science. Enforcement fell on the professional societies, professional journals, and science reporters. They were supposed to adhere to what might be called *due discipline.*

Some of the most senior scientists in the United States jumped precipitously to the conclusion that the anomalous power claims were pathological. Why was that conclusion not held to a review of Langmuir's criteria? Six months into the fray only one prominent physicist had publicly rejected the Langmuir criteria as not appropriate to cold fusion studies. He dismissed it in an unspoken manner so that the rejection was not obvious though it was presented in a *New York Times Sunday Magazine* article. Orthodox science failed to be sufficiently self-critical of its own judgements.

The statistical underpinnings of Fleischmann and Pons's cells fooled some of the more ardent nay-sayers when many cells failed to display anomalous power. One outspoken skeptic assumed a smooth statistical distribution for his evaluation of the failures. He went on to "explain" the significance of the geographical distribution of experiments counted as failed and as successful experiments. The journal *Nature* compared the acceptance of excess heat to the acceptance of religious belief on the same grounds. The slander of labeling practitioners of cold fusion research as "believers" may have derived from that assumption. That folderol continued for many years without self-correction by the scientific community.

Science was belittled when it failed to evaluate a well-measured claim that was of *scientific* importance. The most damning behavior was the insistence that an unconventional protocol replace normal procedures. No instance came to my attention where orthodox scientists (or science reporters) questioned this egregious substitution. When superconductivity was discovered, when radium was found to be warmer than its ambient, these were accepted as interesting scientific observations subject only to the demand that the observations be well performed. This was routinely done because the experiments were readily reproducible. An assertion of thorough evaluation was set forth by the presentations at Baltimore, but that assertion did not hold up to its critique. In this matter, no scientist of orthodox persuasion stepped forward to demand that the anomalous power demonstrations be adequately evaluated. While the DOE Panel made a gesture in that direction, necessary funding to support it was specifically ruled out.

For eight years McKubre maintained a laboratory to demonstrate anomalous power. There were a total of three informal, but substantial, evaluations by recognized experts from outside the cold fusion community. They found no procedural error, but they remained silent.

For the first time, orthodox science decided that there be no communication with those working in cold fusion research. Professional journals were discouraged from publishing such research. Skeptics would not participate in laboratory experiments. Scientific journals would not accept paid advertisements for books supportive of the field. These were anti-intellectual positions. The broad-audience magazines of science have assiduously maintained a wall around the cold fusion research ghetto even unto 2002. This refusal to listen, look, and report should be treated with the contempt such attitudes ordinarily attract.

For the first twelve years, skeptics, upon inquiry, told the press that cold fusion research was as dead as it always was and that it would remain dead for a long, long time. We see that such talk was merely an expression of hubris: nuclear physics is science; conflicting data when obtained from other disciplines, like chemistry, are to be ignored. That arrogance harmed science by delaying the scientific community's evaluation of anomalous power evidence for a decade or more.

Hold Falsifiable

In March 1989, the community saw its duty clearly to evaluate what had been announced. Unfortunately, its time horizon, was too short. The two or three months allotted to the task proved inadequate. Twelve years later, science needed to return to its duty even when long term emotional commitments made that difficult. Its duty was to follow the evolution of a significant claim of discovery, one that intimated a new source of energy for society. The seminal paper of July 1990 by Martin Fleischmann, Stanley Pons, Mark R. Anderson, Lian Jun Li, and Marvin Hawkins, remained essentially unread by the scientific community eleven years after publication. Professional disciplines essential for undertaking that task include electrochemistry, calorimetry, and modern data reduction methodology.

Scaramuzzi articulated this failure. "How is it possible that after 10 years the extreme positions about CF have not softened, . . . I have just stated that reproducibility has been 'almost' reached. Why, then, has nothing changed? . . . One often has the feeling that any attempt to restart communications is doomed in advance."[10] One could say the same about the evidence for helium-four which now assumed the tentative role of nuclear ash that was once thought by the skeptics to be so terribly important.

The governments of France, China, the U.S., and Italy, in that order, have established what are meant to be scientific research programs. They represent a more mature realization of how a possible new science ought to be evaluated.

The outspoken nuclear physicists continued to insist that the nuclear products be identified before the field could be recognized as a science. But was that really their purpose? Bressani catalogued remarkable progress in identifying helium-four as the long demanded nuclear product. The outspoken physicists evidenced no interest in this, and the broad-audience scientific journals did not report on it. Were the demands for nuclear products disingenuous? If they had demanded it, why then did the skeptics not demand research funding to complete exploration of helium-four as the possible nuclear product associated with anomalous power? In fact, that line of research persisted and recognition of helium-four as the nuclear ash appears imminent. Would the scientific community at large now be informed of this achievement?

The skeptics were protecting their economic, professional, and scientific turf with insatiable demands. They would either have to be overridden, or science would have to wait for their passing before the data for anomalous power and its nuclear ash could be admitted into mainstream science. Only then can its proper contention with the formulae from past nuclear experience get underway. Until then, the skeptics maintain that the evidence for anomalous power resides outside of science because it conflicts with nuclear theory. *To protect a theory from possible experimental falsification by refusing new data does harm to science.*

What remains of the controversy can be watched with curiosity and pleasure. No one can know for certain what will be found for the energy-source reaction, nor when. It is not possible to know for certain what will be the early applications of an ensuing technology. Nor is it known which skeptics will be converted, and which will go to their graves unpersuaded. It will prove a fine spectator sport.

Who might qualify to serve as the continuing sports commentator? Most likely not a fusion physicist. His expertise resides in high-energy physics that seems to play no role in the anomalous power phenomenon, not to mention that a conflict of interest might exist in the competition for funding between hot fusion and cold fusion research.

From a strictly technical point of view, an electrochemist is the appropriate commentator for the sport. He should be intimately acquainted with the experimental work, of course. Another commentator might be a solid state physicist to follow the evolution of theory about where the energy comes from. He will need to be familiar with contemporary laboratory evidence of nuclear activity that a theory will have to satisfy as well as with established nuclear theories with which it may have to contend. How long will this commentary last? If the source reaction is a multi-body, coherent process and if only a meager resource is devoted to finding it, historical comparisons would imply that the elapsed time may be twenty or more years to completion.

Validation

Early on, Fleischmann and Pons reported output power 20 times the input power during a 48 hour burst. W. Hansen, after analyzing cell data produced by the Utah chemists, reported, ". . . a profound energy source." McKubre observed 0.9 watts generated over 7 days, amounting in all to a half megaJoule. Huggins, in a remarkable cell and calorimeter design, recorded a peak generated power of 5.6 watts. Arata reported the production 200 megaJoules over four months with a 125 watt power maximum. Fleischmann and Pons later produced 100 watts for 32 days, and during 158 days generated 300 megaJoules of energy, equivalent to 80 kilowatt-hours of energy.

A laboratory researcher only needs to make an observation to add an interesting new fact to the inventory of scientific data. Galileo implicitly established that premise when he reported mountains on the moon and moons orbiting Jupiter. He could not explain these things and he knew he was not obliged to do so. Science could spend a millennium, if need be, handling the perplexing data his observations brought into the realm of science. That dichotomy of protocol is a mantram of this narrative. I earlier referred to it as *the stand-alone quality of an observation.* The immediate demands for causal relationships are perfectly reasonable as *separate* demands because they will serve to spur research by the scientific community, but those demands must not be permitted to hold the observations hostage from validation.

The saga of anomalous power began with its first recognition in the winter of 1984–1985. That happened with the "meltdown" experiment in Room 1113 of the north Henry Eyring building at the University of Utah. By the summer of 1988, Fleischmann and Pons had a tightly specified experimental cell and instrument that was designed to demonstrate the effect in a well-measured manner. That type of cell demonstrated anomalous power in a considerable number of experiments without failure up to and including the day of the Utah announcement. By the end of 1994, some two dozen independent laboratories had reported the detection of anomalous power in full-length reports. A scientific fact was validated in this manner. Nearly 100 such demonstrations and reports were completed by 1999. It is not unlikely that by now excess heat has exhibited itself in a laboratory cell more than a thousand times.

As replication of anomalous power phenomenon turned the evidential tide against them, the skeptics found that they did not have to declare *mea culpa,* simply because no one was watching. They need not say anything. Cold fusion research had been so stigmatized during 1989 that nothing reported by its researchers was noticed. This lack of watching and noticing was particularly true of the broad-audience science journals and the professional societies. Even the *Wall Street Journal* gave up watching.

Two summary statements are appropriate at this point in our review. In October 1999, McKubre said, "The evidence in my view for the appearance of an anomalous unaccounted excess heat in the deuterium-palladium system is essentially overwhelming. There is something there. It is larger by more than one order of magnitude, in some cases by more than two orders of magnitude, than the sum total of all possible chemical reactions."[11] Previously, Arata had written in the *Proceedings of Japan Academy,* "[We] have duplicated many times the principle, structure and functions of [the] "DS-Cathode" [in the deuterium-palladium system] which generates helium and tremendous excess energy as reaction products with 100% reproducibility sustained over several thousand hours."[12]

Once the existence of the excess heat phenomenon is accepted then there is a new theoretical area to be explored. We have noted the statement by theoretician Hagelstein, when he took his place at the rostrum, registered the proper form: "I accept that excess heat energy exists." He would then proceed with his theoretical paper. Guiliano Preparata, Department of Physics, University of Milan, Italy, published a theoretical exposition in 1996 offering his analysis and construction of a theory to support the experimental outcomes.[13]

The preamble to the DOE Panel's report gave cold fusion studies its validation criterion. It said that even a single short but valid cold fusion event would be revolutionary. Huizenga comments that cold fusion consists of a "mélange of claims." I resolve these two positions with the assertion that even a single short but valid anomalous power event would be revolutionary. The validation steps have followed the most universal of protocols, replication in an independent laboratory.

Only one question was raised in the literature about the true independence of the laboratories that have reported anomalous power. It was that the inadvertent recombination of the oxygen and hydrogen gases in the cell might provide the power that was then misconstrued as anomalous power. A review of the reports, however, shows that for experiments in a variety of laboratories such recombination was not inadvertent at all. Some experiments have measured the recombination and allowed for it, while others have incorporated into the experimental design the deliberate recombination of all the gases. Finally, there are the control experiments that demonstrate no anomalous power even though they are subject to the recombination claim. Oriani, in his corroborating experiment that was submitted to *Nature* (September 1989), provided a cylindrical, perforated glass wall between the cathode and anode to prevent recombination.

After 1994, the remaining concern was the attainment of reproducibility with the Fleischmann and Pons cell. During these years, a high order of reproducibility was brought within reach. Several experimenters had achieved 50% reproducibility of excess heat generation. Some had developed particular variations of the cell design that claimed 100% reproducibility and awaited only the additional steps of funding and fulfillment in order to attain the imprimatur of an established institution. Arata's and Preparata's versions of the experiment may prove highly reproducible.[14]

There will be a long wait until there is available for anomalous power some avenue by which a proof may be acquired. It may come in the form of an experiment that is tolerant of the variety of palladium metallurgy. It may require that the palladium cathode be constructed one atom at a time in accordance with a tight specification.

The experiment moved quickly to much higher power levels. These operated at 50 to 200 watts output power, and achieved extraordinary power densities, although the highest of those densities were achieved in wires of exceedingly small diameter. Evidence of nuclear activity continued with many experiments reporting a number of different kinds of particle and radiation emission. Scaramuzzi declared that the bewildering variety meant that the source of the heat and emissions must be a coherent, multibody nuclear reaction.[15] Finally, the Mengoli run of seven days without cell excitation current gives virtual proof of the phenomenon of excess heat if mankind knows how to measure the presence of heat at all.

What about blame for the damage that was done, the damage to Fleischmann and Pons, to the American scientific community, to the development of science in this new area? I am sometimes asked, if I had 100 milliliters of blame to allocate, how would I apportion it.

Apportioning Blame

A first dollop of blame for what ensued, ten milliliters, goes to the University of Utah for a poorly executed press presentation. Peterson and Brophy spoke in generalities correctly leaving the heavy work for Fleischmann and Pons, neither of whom had their statements prepared beforehand. It was unconscionable to expect them to stand before a national audience and extemporaneously compose the principal announcement. The press release was cut back by attorneys to a skeleton of what was needed to inform the scientific community and the public. The administration needed to overrule the specialists, and it failed to do so.

The university's faculty agonized during the summer of 1989 quite certain that the developing contention would do permanent harm to the school. They were concerned that it would become difficult to attract the best faculty and graduate students. The statistics show that these concerns were misplaced. The graphs of these numbers continued their growth in a straight line showing no dip that could be attributed to the cold fusion episode. This was a lesson to be learned by the administrators of other research institutions.

I allocate another ten milliliters of blame to Fleischmann and Pons. I accept that they had a tiger by the tail and no way to let go. They were inexperienced in matters of public communication. A part of their science was in error. They ought to have renounced that data promptly, and it was wrong that they did not share cell data with their colleagues at the university.

The Baltimore APS presentations get thirty milliliters for the comprehensive errors in descriptions of Fleischmann and Pons's experiment and of the two chemists' persons. The claim that they were incompetent and mentally ailing was unethical as well as wrong. The significance of the number of failed experiments was grossly exaggerated. To ascribe Langmuir's pathology to the announced claims can be seen to be wrong.

The four press conferences also get thirty milliliters for their intense, aggressive purpose, and for their successful impact. The assertion of "absolute" knowledge that was offered to the cream of the nation's science reporters was a knowing attack. The further assertion that the Caltech experiments were more sophisticated than those done in Utah was unnecessary as well as wrong. The four press conferences turned the scientific community away from its duty to evaluate what was claimed and redirected its interests to the mean practice of politics.

Nature's rejection of Oriani's corroboration of excess heat in January 1990 gets an award of twenty milliliters of blame. His paper fit the proper protocol of a corroborating experiment. That rejection was explained as due to the difficulty of anomalous power replication in general and because there was no evidence of nuclear products. While each of these reasons was of great interest, an answer to them was not required according to the usual standards of scientific corroboration. The fact that the corroboration was published in another place does not subtract from this aliquot of error. The various publications are not of equal stature.

Mother nature proved adequately stubborn. Twelve years later, more than 100 technical people were still devoted to the field. While the skeptics successfully stopped some of the research, and treated the researchers as if they did not exist, those same skeptics, fortunately, did not take to burning the published papers. These are still available in libraries. Even if, at the twelve-year anniversary, all research monies were to disappear and all cold fusion research were to end, there resides in the published citations many experiments of sufficient stature for subsequent generations to pick up and pursue, to a completion of the scientific base.

An Ordeal of Passage

The ordeal I speak of does not involve the extended effort Fleischmann and Pons devoted to the defense of their scientific claims. That belongs in another category. Research that was done with their type of cell involved the most ordinary sort of scientific activity. The kind of electrolytic cell work that Fleischmann did in his cold fusion experiments, he had done frequently during his professional life. As he put it: "It's been said that we have gone off the wall with our ideas, but that's absurd. Actually, we are extremely conventional scientists. I always say we are so conventional, it is painful."[16]

The personal side of resolution sees the two principal protagonists as accomplished scientists continuing the work of a lifetime, but being forced in their mature years to endure an ordeal of passage, one which society traditionally reserves for its prophets. The best that can be done by way of offering some emotional resolution of this matter is for the reader to turn to the words of Koonin in his 1989 retrospective interview, and to the words of Fleischmann in a 1994 interview. In his retrospective of the Baltimore events, Koonin offered the consolation, "That we said these things just won't matter . . ."

With reference to his own ordeal during this period, Fleischmann offered the following thesis.[17] "If you have any integrity at all, you do what you have to do, and you take the consequences." The ordeal consisted of punishment, to state it bluntly. Koonin at Baltimore accused Fleischmann and Pons of being incompetent and possibly delusional. N. Lewis responded to a question asked by a reporter: Should reporters ignore claims of excess heat? Lewis answered "absolutely," while his own experiments lay percolating just outside the parameters of interest. The APS Bulletin reported about that meeting, "The Corpse of Cold Fusion will probably continue to twitch for a while even after two nights of assault."[18] In April 1991, the APS Bulletin announced in a reference to Pons, "CF Huckster Resigns Position at Univ. of Utah."[19] The slander of the label "true believers" was continued for more than six years. At an official meeting of the APS in 1995, in his official address Park said, "To be sure, there are even today true believers among the cold fusion acolytes, just as there are sincere scientists who believe in alien abductions, psychokinesis, creationism, and the Chicago Cubs." The sarcasm even went into the title of his talk, which was, "Pigs Don't Have Wings: When Scientists Fool Themselves."[20]

I visited with Park in the autumn of 1996. He spoke at some length and with passion of the University of Utah's refusal to release an assay report for helium in a palladium cathode, "Fleischmann and Pons had agreed the test was critical." I also inquired about his refusal to attend the AIP ceremonial lunch for Jerry Bishop, reporter for The *Wall Street Journal.* Was this not how a conformist society was built—with punishment by ostracism? His reply was to the point, "Many things are punished by ostracism. Especially, withholding information within the scientific community gets punished."[21]

I turn to Robert Frost, the New England poet, to consider the impact of the ordeal on Martin Fleischmann and Stanley Pons. Frost viewed the matter of a scientist's self-esteem with some penetration.

> "Have you ever thought about rewards," I was asked lately . . . I don't know what I was supposed to think unless it was that the greatest reward of all was self-esteem. Saints like John Bunyan are all right in jail if they are sure of their truth and sincerity. But so also are many criminals. The great trouble is to be sure. A stuffed shirt is the opposite of a criminal. He cares not what he thinks of himself so long as the world continues to think well of him. The sensible and healthy live somewhere between self-approval and the approval of society. They make their adjustment without too much talk of compromise.
>
> The scientist seems to have the advantage of [the artist] in a court of larger appeal. A planet is perturbed in its orbit. The scientist stakes his reputation on the perturber's being found at a certain point in the sky at a certain time of night. All telescopes are turned that way, and sure enough, there the perturber is as bright as a but-

> ton. The scientist knows he is good without being told. He has a mind and he has instruments, the extensions of mind that fit closely into the nature of the Universe.[22]

Indeed, Fleischmann and Pons had their minds and their instrument, their cell, which did fit closely into the nature of the universe. But so far the greatest reward of all had been denied them—esteem from the community of scientists.

Scientific discovery often follows from the development of advanced laboratory instruments, as was the case with Galileo's telescope. Fleischmann and Pons showed how they could calculate the power balance of their cells to within one tenth of one percent in their paper of July 1992. This indicated that they were masters of electrolytic cell design and its concomitant use as a calorimetry instrument. It was rightly so that they were the ones to discover the phenomenon of anomalous power.

What place will Martin Fleischmann and Stanley Pons hold in the halls of science? Fleischmann had received many awards in his specialty of electrochemistry, and he was elected to the Royal Society, England's most prestigious scientific honor. What additional awards they might be graced with or denied cannot be anticipated, but there is a way to give the matter some consideration.

Jeremy Bernstein, a physicist, had a word to say about comparisons that I found apt. He compared the poet Stephen Spender (who was very good) with renowned poet W. H. Auden, and compared the physicist Robert Oppenheimer (who also was very good) with Paul A. M. Dirac who founded the science of quantum mechanics. Bernstein concluded, "That is what great poetry and great physics have in common, both are swept along by the tide of unanticipated genius as it rushes past the merely very good."[23] Great chemistry, I am sure, gets swept along as well by the tide of unanticipated genius.

Expressing his summary views of the Baltimore event one week later, Koonin had two separate conclusions. The first was, "I feel that we did a good job," and the second I mentioned above, "That we said these things just won't matter."[24]

Fleischmann was fulsome in his summary view.[25]

> If it had been anything else, we would have said, Oh . . . People don't want us to do it; forget it; just leave it alone. But this is not in that category. This is interesting science. New science, with a hint of a possibility of a very useful technology. Therefore, if you've got any integrity, you don't give it up. You give it up if you find you are

> wrong. But as long as you believe that you are right, you have to continue with it. And you have to take the consequences.

Is this not similar to the response of the Swedish chemist Svente Arrhenius with his discovery of the mechanism of electrolytic conduction more than one hundred years ago? He believed he was right and he persevered for twenty years before receiving the recognition that was his due. One can only wonder why discovery seems to be so punished. Why, so often, must the next Columbus be brought home in chains?

Acknowledgments

I wish to thank the scientists involved in cold fusion research and those who attended the ICCF conferences for the attention they gave to my interrogations and interviews. In a number of instances, I have asked them to review those paragraphs in the manuscript that described their own work. Any mistakes or omissions are mine alone.

The manuscript was finished September 9, 1998, although much editing was done during the ensuing seventeen months before the printer got to it. Margaret S. Montague tackled the initial editing task of transforming my writing into a readable text. The manuscript for the Second Edition got wrapped up on September 28, 2001.

I first met David J. Nagel when he appeared by appointment for breakfast in the 7:00 A.M. darkness of a winter's morning in Washington, D.C. We rated the best window table for our breakfast in the otherwise empty dining room. David accomplished a knowledgeable, line by line review of the text. When I asked him later if he would write an introduction to follow Arthur Clarke's foreword, he accepted immediately. I am grateful for his support. Arthur Clarke's gracious willingness to add to his incredibly busy schedule to provide a Foreword for this manuscript will always be remembered.

Cornell University maintains a Cold Fusion Archive, a large and historically important collection (#4451) located in the Carl A. Kroch Library for Rare and Manuscript Collections (whose building is situated 80 feet below ground surface). Professor Bruce V. Lewenstein of the Departments of Communications and Science and Technology Studies is the technical coordinator for the archive. Mrs. Elaine Engst, University Archivist, manages the collection. Both gave freely of their time and interest to help me find the things I was looking for, not only during my visits but in response to requests at other times as well.

Scott and Talbot Chubb, Richard A. Oriani, and Mitchell Swartz were always ready to explain matters and answer naïve questions. Peter Hagelstein read my manuscript and made several important points of criticism. I am indebted to Edmund K. Storms for a careful review and comment on my work in progress. Thomas O. Passell was always helpful in finding elusive documents. I was pleased to find a place for the important work done by George Miley and T. Mizuno. B. F. Bush and M. H. Miles gave freely of their time to help sort out the early detection of helium.

Several Japanese scientists that have made significant contributions to the field were cordial in responding to my entreaties and agreeable to being interviewed. Dr. Xing–Zhong Li was always ready to inform me about activities in China. I was awed by the fine scientific work accomplished in Italy. At Frascati, F. A. Celani, Vittorio Violante, Antonella De Ninno were always cordial and helpful. Tulio Bressani always responded to my inquires promptly and fully. The excellent work of Guiliano Mengoli, Padua, entered this book with the second edition. I first met Franco Scaramuzzi at the Hokkaido meeting and I am pleased to see that his analyses and descriptions of the cold fusion saga find an important place in this edition.

Chase and Grethe Peterson were gracious hosts for my several visits to Salt Lake City. At the University of Utah, Theodore Eyring escorted me about so that I could see whatever laboratories or rooms were of interest. The senior faculty, staff of the university, officers and trustees were always cordial and cooperative. In a like manner, faculty members and former officers at Brigham Young University helped me to understand the events that took place twelve years ago.

To Martin Fleischmann I owe special thanks for being patient with me during two long and difficult interviews and for pleasantly fielding questions at other times. John O'M. Bockris overwhelmed me with information about his fine work and unfortunate controversies, only a small fraction of which found space in the text. Marvin Hawkins helped put together the early laboratory events of 1988–1989. Fred Jaeger's close association with Fleischmann and Pons provided much background color.

My friends at Cold Fusion Technology, Inc., saw the manuscript for the first time in March 1999. There, Jed Rothwell proved an excellent text editor as well as contributing up to date information on cold fusion research. Eugene F. Mallove reviewed it also and offered comments on a number of points for which I am thankful.

To those many others that gave assistance when asked, my thanks and appreciation for your comments and judgements.

I thank my wife, Kate, and my son, Carl, for suffering with my trials and tribulations during the writing and production of the book.

The large commercial publishing houses found the manuscript too technical and the subject matter too heretical for their interest. For this kind of book, the commercial and intellectual advantages resided with publishing the book through my own Oak Grove Press, LLC, and thereby retaining all copyright rights. I asked the Infinite Energy people if they would be interested in distributing the book. Fortunately, they were pleased to do so.

APPENDIX

The Wilson Critique

The following is a continuation of the Wilson, et al., critique from Chapter 9.

The considerable power levels that Fleischmann and Pons measured in their seminal article (July 1990) claiming anomalous power generation were reviewed in Chapter 4. During the following year R. H. Wilson, J. W. Bray, P. G. Kosky, and H. B. Vakil, of General Electric Co., Schenectady, New York, and F. G. Will, of the Department of Chemical Engineering, University of Utah, offered a substantial critique of that article.

The Wilson group was active in cold fusion experimental work from the beginning. Their paper was submitted for publication in June 1991 and accepted for publication that December. A copy of their manuscript would then have been sent to the original paper's authors for preparation of a response. Fleischmann and Pons responded, and the two papers were published together in July 1992, in the normal manner of professional publications.

Wilson summarized his critique as follows.

> We evaluate the data and methods of Pons, Fleischmann and coworkers and, where sufficient data are available, conclude that they overestimate significantly the excess heat. This is in part because in their calibration they did not include calculation of the change in input electrochemical power to the cell resulting from the calibration heater power. An additional significant overestimate of excess energy occurs when the calibration is made at cell temperatures above 60°C, owing to the increased evaporation of heavy water during the calibration.*

* Wilson, R. H., et al., "Analysis of Experiments on the Calorimetry of LiOD-D2O Electrochemical Cells," (*Journal of Electroanalytical Chemistry*, vol. 332, 1992).

The Wilson critique had two purposes. It discusses Fleischmann and Pons's seminal paper, and it reports on research with its own electrolytic cells, none of which produced excess heat. The concern here is with that part of the report that discusses the Fleischmann and Pons paper. (The question of the significance of failed experiments was evaluated in the Chapter 8, page 107.)

Wilson's criticism of Fleischmann and Pons's paper was limited to the following concern expressed in the paper's abstract ". . . in their calibration they did not include calculation of the change in input electrochemical power to the cell resulting from the calibration heater power." Their criticism of inadequate data treatment concerns the consequences of using the calibration pulse to determine cell performance. The impact is a reduction in the amount of calculated excess power reported in the original Fleischmann and Pons article. Wilson accordingly recalculates the generated power for the Fleischmann and Pons cells.

The recalculated excess power in one case amounts to *minus* 0.43 watts.* This result implies that there is a thermodynamic black hole in that cell which swallows 0.43 watts of power thus causing energy to disappear from existence. This unexplained *disappearance* is fully as remarkable as the original Fleischmann and Pons announcement where they claimed the unexplained *appearance* of power. The Wilson critique is incomplete on this point.

Fleischmann and Pons respond as follows to these criticisms of their calculations.

> The central assumption in the paper by Wilson is that one can assume the systems to be in a steady state at the point in time at which they are calibrated . . . and at which the values . . . are to be evaluated. In point of fact there is no such steady state . . . as can be seen from . . . the paper by Wilson, . . . The magnitudes of [those] terms . . . are in fact comparable to those of the corrections . . . introduced in deriving the heat transfer coefficients . . .†

In other words, a principal assumption of the Wilson paper introduces an error equal in size to other significant corrections being proposed.

Consequently, Fleischmann and Pons note that the integral form of the differential equation must be used for computation, as follows,

> It is well known in many fields of research that accurate values of the parameters of the differential equations which model the systems can

* Wilson, R. H., et al., "Analysis of Experiments on the Calorimetry of LiOD-D2O Electrochemical Cells," (*Journal of Electroanalytical Chemistry*, vol. 332, 1992), p. 10.

† Fleischmann, M., and S. Pons, "Some Comments on the Paper Analysis of Experiments on Calorimetry of LiOD/D2O Electrochemical Cells, R. H. Wilson, et al.," (*Journal of Electroanalytical Chemistry*, vol. 332, 1992), p. 38.

> only be obtained by comparing the *integrated* forms of the equations with the experimental data. (p. 38)

Fleischmann and Pons continue by cautioning on the limitations of this technique. The process of integration will smooth values over time thus reducing mathematical errors in the computed coefficients.

After identifying the various methods used to calculate results from the cell data as methods one through six, Fleischmann and Pons offer the following assessments of Wilson's work.

> Wilson does not deal with any of these evaluations: they regard Method 2, which was outlined . . . as "very complicated and very difficult to follow in detail." However, this method, together with low pass filtering, . . . is the standard method of modern data processing. (p. 40)
>
> The filters which have been used . . . take full account of the evaporation of the [electrolyte] . . . the assertion that we did not take this into account can be seen to be incorrect . . . We observe that the results of the independent investigation using filtering were presented to the group at GE during 1991; their omission of reference to this work shows that they also reject this method of data processing in addition to Method 2. (p. 40)

Clearly, the Wilson team had not achieved a review of calorimetric data reduction; they were not evaluating each of the possible ways to get the optimum information out of the cell data reading. It was up to Fleischmann and Pons to explain to the Wilson team what needed to be done.

Fleischmann and Pons provided a tour of the six computational methods and then got down to the nitty-gritty.

> We are therefore reduced to examining the claim that the method put forward by Wilson, Method 5, provides an accurate means of evaluating [excess power]. The authors imply that as the results obtained by their Method 5 differ from those obtained by our own approximate method, Method 1, it is our method which must be judged to be incorrect. (p. 40)

Fleischmann and Pons's conclusion is reached after several pages of equations and tables.

> We conclude that [the two methods] are comparable but that they give the [heat] balance at different [times of the cell's operating cycle]. (p. 44)

After rejecting the possibility of maintaining the electrolyte level perfectly constant, they promptly continue to a way of dealing with this intrinsic artifact.

> The answer lies in making the [equation's] term . . . part of the evaluation [calculation] and this in itself dictates the strategy that the whole [period of the variables] be fitted to the integrated forms of the . . . equations which model the calorimeters, i.e. it dictates the use of methods such as Methods 2 and 4. It is not surprising that such methods can give precise results as a matter of routine. (p. 47)

Fleischmann and Pons touch upon two summary items that are of interest to us. One concerns the general accuracy of calorimetry, and the other concerns the results of the Wilson heat generation effort.

> The information on this issue [of calorimetric accuracy] which was contained in our original paper and in the related papers has been ignored by Wilson. They have also ignored the fact that we showed that it is possible to achieve at least 99% heat accuracy by the methods we have used . . . ; we have never claimed an accuracy better than $\pm 1\%$ or ± 1 milliwatt whichever is the greater. (p. 47)

They gave the following assessment of Wilson's recalculation regarding their cells's performance.

> . . . They also do not discuss the fact that even on the basis of their own evaluations[,] the excess [power] for a 0.2 cm diameter x 10 cm length Pd cathode polarized at 128 milliamperes for each square centimeter of surface area has reached [approximately] 50% of the [power] input after 15 days of [operation] . . . Presumably they believe that the errors have now reached 50% to explain away these effects? It should be noted the these [power] outputs are of the order 4 watts/cubic cm . . . and that over the duration of the experiment shown[,] the total [energy] released is of the order of 4 MegaJoules per cubic cm . . . which hardly lies in the province of chemistry. (p. 47)

Wilson does not mention their own recognition from their own calculations of the existence of excess energy as presented in the Fleischmann and Pons original paper. They are also unwilling to face up to the implication that the amount greatly exceeds what can be explained by measurement inaccuracies.

Chronology

The following chronology is not meant to be a cold fusion history. It includes primarily those items referenced in the text.

1866	First investigation of hydrogen in metals by Thomas Graham.
1903	Pierre Curie discovers that radium is always warmer than its environment.
1926	Paneth and Peters report fusion of hydrogen to make helium. The following year they retract the report as mistaken.
1929	Cöhn demonstrates that hydrogen in metals migrates toward the negative terminal.
1935	Ludwik Fleck published his definitive account of the difficult-to-replicate scientific experiment: the Wassermann Test for syphilis. His account is titled, *Genesis and Development of a Scientific Fact.*
1950	Fleischmann awarded Doctorate in Chemistry by the Imperial Collage of the University of London.
1967	Fleischmann accepts Faraday Chair in electrochemistry at Southampton University.
1972	Fleischmann collects together materials to drive deuterium into palladium very hard. The effort is abandoned for lack of time.
1975	Pons admitted to postgraduate studies at Southampton University. Receives Doctorate two years later.
1983	Pons accepts position in the chemistry department at University of Utah. Fleischmann and Pons begin their collaboration

1984	Fleischmann and Pons plan to try an electrolytic cell experiment to fuse deuterium inside a palladium lattice. Electrolyte temperature rise is to be the parameter to watch.
1985	Fleischmann elected member of the Royal Society of London.
Winter	Large release of energy from early cold fusion experiment in laboratory in the north Henry Eyring Building at University of Utah.
1986	Pons appointed a full professor.
1987	
July	*Scientific American* magazine published "Cold Nuclear Fusion" by Jones and Rafelski.
1988	Pons becomes Chairman of the Chemistry Department.
August	Two Utah chemists submit proposal to fund their cold fusion studies to the DOE.
September	DOE selects S. Jones at Brigham Young University (BYU) to evaluate the proposal.
Fall	M. Hawkins, graduate student in the Department of Chemistry, begins experiments with the Fleischmann and Pons designed Dewar cell.
1989	
February	Fleischmann and Pons undertake nuclear measurements to back up excess heat results.
March 22	Preliminary Note manuscript received by the *Journal of Electroanalytical Chemistry.*
March 23	Press announcement of cold nuclear fusion at the University of Utah.
April 10	Publication of the Preliminary Note.
April 18	F. Scaramuzzi announces the detection of neutron emission from titanium infused with deuterium gas and then temperature cycled.
April 24	Secretary of Energy Admiral James Watkins forms a panel on cold fusion under joint chairmanship of J. Huizenga and N. Ramsey.
Baltimore May 1	The American Physical Society spring meeting at Baltimore, MD.

4:00 P.M.	First press conference. Nothing transpires. There have not yet been any presentations.
5:00 P.M.	Second press conference. N. S. Lewis asserts there is absolutely nothing to the claims of excess heat.
7:00 P.M.	First special session on cold fusion. - Koonin reports no possibility of d-d fusion producing the excess heat and comments on the mental and professional status of Fleischmann and Pons. - N. S. Lewis reports no evidence of heat or nuclear effects from his experiments at Caltech. He also reports that Fleischmann and Pons did not obtain excess heat in their experiments in Utah. Their faulty results are attributed to a lack of stirring in their cells.
May 2	
10:00 A.M.	Third press conference (conducted by S. Koonin).
7:00 P.M.	Second special session on cold fusion that ended late in the evening. (This was the end of the APS special program on cold fusion research.)
Los Angeles May 8	American Electrochemical Society meeting session on cold fusion research at Los Angeles, CA.
5:00 P.M.	Session on cold fusion research. N. S. Lewis and M. Fleischmann speak.
9:00 P.M.	Fourth press conference: Bockris, Appleby, Fleischmann, and Pons. (N. S. Lewis takes a microphone, stands on chair and asks "loaded" questions.)
May 18	Petrasso report in *Nature* shows fatal flaws in Fleischmann and Pons claim to have detected neutrons.
May 23 Santa Fe, N.M.	DOE sponsored meeting on cold fusion reports.
July 12	Interim report of the DOE Panel.
August 17	N. S. Lewis's report published in *Nature.*
September	Oriani submits anomalous power corroboration report to *Nature.*
September 15	Deadline set by the DOE Panel for receipt of reports about cold fusion research. Presumably its final report reflects nothing that developed after this date.
September 24	*New York Times Sunday Magazine* published the Crease and Samios lampoon about Fleischmann and Pons and their claims.

October 16	EPRI/NSF meeting in Washington. N. S. Lewis hears Fleischmann report on excess heat generation and raises no argument about the validity of the work.
November 11	Final DOE Panel report issued.
December 21	Fleischmann and Pons manuscript of anomalous power measurements received by its publisher, the *Journal of Electroanalytical Chemistry.*
1990 January	*Nature* decides to not publish Oriani's confirmation of the anomalous power phenomenon first reported by Fleischmann and Pons. The *Wall Street Journal*'s Jerry Bishop receives award from the AIP for his reporting of cold fusion events during 1989.
January 26	Oriani informed referees do pass his paper for publication, but Washington, DC, editor rejects it.
March	First Annual Conference on Cold Fusion. - Huggins reports 5.6 watts peak power generated. - McKubre reports a D/Pd threshold effect.
March	*Nature* says "farewell" to cold fusion.
July 25	Fleischmann and Pons's 56-page seminal paper is published. It describes anomalous power generation and its calorimetry.
1991 Spring	W. Hansen, professor of physics at Utah State University, Logan, prepares an important report for the Utah State Fusion/Energy Council supportive of excess heat.
June	Second ACCF held at Villa Olmo, Como, Italy.
1992 July	Wilson, et al., publish substantial critique of the Fleischmann and Pons paper of July 1990. Fleischmann and Pons's defense is published concurrently.
October 21	ICCF-3, Nagoya, Japan
November	Storms reviews the field to date in an extensive summary article in *Fusion Technology* (20, 1991, p. 433).
1993 December	Fourth ICCF at Maui, Hawaii. More definitive data on D/Pd loading threshold is presented.
1994 Autumn	The *American Scholar* publishes article by David Goodstein, "Pariah Science."

December	By this time, anomalous power (excess heat) was widely confirmed by replication in many independent laboratories using different types of cells and calorimeters.
1995 Spring	Dr. Edmund Storms reviews the field to date in the *Journal of Scientific Exploration* (10, no. 2, 1996,p. 185).
Spring	Dr. G. Preparata published *QED Coherence in Matter* (World Scientific Int. Publisher, May 1995).
Spring	Dr. N. Hoffman's book, *A Dialogue on Chemically Induced Nuclear Effects,* is published.
April	Fifth ICCF held in Monaco.

Glossary

Some of the abbreviations used in the chapters, references and appendix are as follows.

AAAS	American Association for the Advancement of Science
ACCF	Annual Conference on Cold Fusion (This form was used for the first two conferences
ACS	American Chemical Society
AIP	American Institute of Physics
ANS	American Nuclear Society
APS	American Physical Society
C&EN	*Chemical and Engineering News*
DOE	Department of Energy
DOI	Department of the Interior
EPRI	Electric Power Research Institute
ERAB	Energy Research Advisory Board (of the DOE)
ICCF	International Conference on Cold Fusion
NAA	nuclear activation analysis
NASW	National Association of Science Writers
NCFI	National Cold Fusion Institute, Salt Lake City, Utah
NSF	National Science Foundation
NYT	*New York Times*
Panel	The DOE/ERAB Panel on Cold Fusion
WSJ	*Wall Street Journal*

Anomalous Power Citations

As was mentioned earlier, several books written to the professional scientist on our subject conveniently omitted bibliographic references to anomalous power reports. This list has as its purpose, in addition to repairing the previous absence of reports, to provide the reader with a list of the more significant publications pertaining to the measurement of anomalous power. Some of the reports include the measurement of helium and neutrons.

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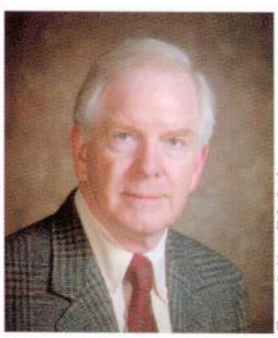
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Charles Beaudette was born in Boston in 1930. In 1952 he graduated from MIT with a bachelor of science in electrical engineering. After selling the instrument company which he founded (Dychro Corporation) in 1961, he worked for ten years with EG&G Corporation, where he specialized in image processing. During this time he participated in the technology development for what became the office facsimile and the PC modem. Now retired, Beaudette lives in Cumberland, Maine.

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