Demarcating Science:
The case of Cold Fusion

A study submitted in part fulfilment of the requirement for the award of
Master of Science in Science Communication

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I declare that this dissertation is entirely my own work and that all sources used have been cited.

Signed:

Grant Pownall

Abstract

In March 1989 two chemists, Martin Fleischmann and Stanley Pons, announced a breakthrough in nuclear fusion, the process that powers the sun and hydrogen bombs. “Cold fusion” promised cheap, clean, safe energy for everyone. Months later, their triumph turned to disgrace as prestigious laboratories around the world could not replicate their findings. Most thought that the duo had made mistakes in their experiments and some even accused them of incompetence and fraud. Cold fusion was declared dead.

Some researchers, however, refused to let cold fusion die and continued to investigate this controversial phenomenon. Over the past 20 years mounting evidence has proved cold fusion to be a real.

In this dissertation, I investigate why cold fusion was rejected and continues to be rejected by the mainstream scientific community. I show how the initial marginalisation of cold fusion was achieved by critics, how they maintain it and what motivates them.

Cold fusion’s rejection from mainstream science is revealed as a case study in how scientists actually demarcate canon science from pseudo-science. Traditionally, philosophers have proposed criteria for demarcating science only in theory. Only recently have sociologists analysed the actual social, political and economic techniques used by scientists for demarcating canon science. I argue that because technical criteria for proving or disproving the phenomenon were missing in 1989, social and political criteria were used to cast cold fusion out of mainstream science. Thus only a political and social effort can return cold fusion to the mainstream. If cold fusion is rehabilitated, it could fulfil its promise of providing limitless, cheap energy that is free from greenhouse gas emissions and dangerous waste.

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I'd also like to thank my father Mike Pownall for inspiring my interest in science. I'd like to dedicate this dissertation to my daughter Hannah Rose: may you inherit a cold fusion future.

Chapter 1 Introduction

If Martin Fleischmann was excited he didn't show it. His face was serene and smooth. In the next half hour, he would announce a major scientific discovery to the public. It had the potential to solve the world's energy problems - clean, safe, limitless energy for rich and poor alike. However, Fleischmann was also irritated. He was a scientist. He didn't like making scientific announcements by press conference.

He and his colleague, Stanley Pons, had been working on a secret scientific project in the basement of the chemistry building at the University of Utah for over five years. They'd achieved great results. Fleischmann felt that the duo had another year's work to do before they were ready to publish but their plans had been thwarted. Just two months earlier, they found out that a rival research programme was going on in a nearby university[1]. The University of Utah pressured him and Pons to announce their discovery to ensure the university could claim the lucrative patent rights. Fleischmann reluctantly agreed.

They were in a spacious conference room in the chemistry department of the University of Utah. It was coming up to 1pm on the 23rd of March 1989. Sitting to their left at the press conference table were the president of the University of Utah Chase N. Peterson, the vice president of research James Brophy, and University of Southampton dean of the faculty of science and professor of geology Robert Nesbit. The room was filling up with press who were chatting excitedly among themselves. Television cameras were focused on the podium.

There were more press than expected. Clive Cookson of the London *Financial Times* had broken a story overnight in US time. The newswires relayed the story to US news outlets and it was sensational. Two distinguished electro-chemists working in secret had created “a sustained nuclear reaction at room temperature.” (Cold Fusion Press Conference 1989) There was no dangerous radiation; no nuclear waste. The apparatus was small and simple. It created vast quantities of power but it ran on an extract from seawater.

The news was too good to be true. If it was not for the reputations of the two scientists, the discovery may well have been dismissed as another perpetual motion machine designed by cranks and garage inventors to create free energy. But Martin Fleischmann was research professor at the University of Southampton. He was a fellow of the Royal Society. He was one

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[1] The rival project was led by Steven E. Jones from Brigham Young University. See Chapter 3.

of the world's leading electro-chemists. B. Stanley Pons was Professor of Chemistry at the University of Utah and head of the chemistry department there. He had gained his PhD under Fleischmann at the University of Southampton. They both had reputations of being first rate researchers, coming up with creative and original ideas.

The conference opened with remarks by Chase Petterson and James Brophy. Then Pons rose to speak. He held up their apparatus to the television cameras.

Figure 1 Pons showing a Cold Fusion cell at the 1989 press conference (Cold Fusion Press Conference 1989 Source: posted by Steven Krivit on youtube)

> We've established a sustained nuclear fusion reaction by means that are considerably simpler than conventional techniques. Deuterium, which is a component of heavy water is driven into a metal rod, exactly like the one I have in my hand here, to such an extent that fusion between these … deuterons [nuclei or centres of deuterium atoms] … occurs to form a new atom. And with this process there is a considerable release of energy. (Cold Fusion Press Conference 1989)

Fleischmann then stood to answer questions. He was relaxed and joked with reporters. He claimed that the heat released was as a result of an "unknown nuclear reaction". The duo's working hypothesis was that this reaction was nuclear fusion; the same process that takes place in the sun or in hydrogen nuclear bombs. The researchers also claimed that they had detected neutrons, the signature sub-atomic particle of a nuclear reaction. Their discovery would soon to be dubbed “cold fusion” by the media to contrast it to multi-billion dollar hot fusion programme in which physicists had been trying to tame the power of the sun for over 50 years (see chapter 2).

The press were bewildered. This was sensational. It was the end of worries about global warming and the imminent exhaustion of the world’s stocks of oil. After some uninspiring questions from the more technical of their fellow reporters, they rushed off to write the science story of the decade – if not the century.

For the careful science reporter, however, things were not quite right at that press conference. It was a great story but what about the science. Had the Pons and Fleischmann work been peer-reviewed and accepted for publication? If so, why was there no pre-print of the scientific paper available at the press conference? This was just the beginning of the questions that would be asked in the next few days and weeks as other scientists rushed to replicate the experiment.

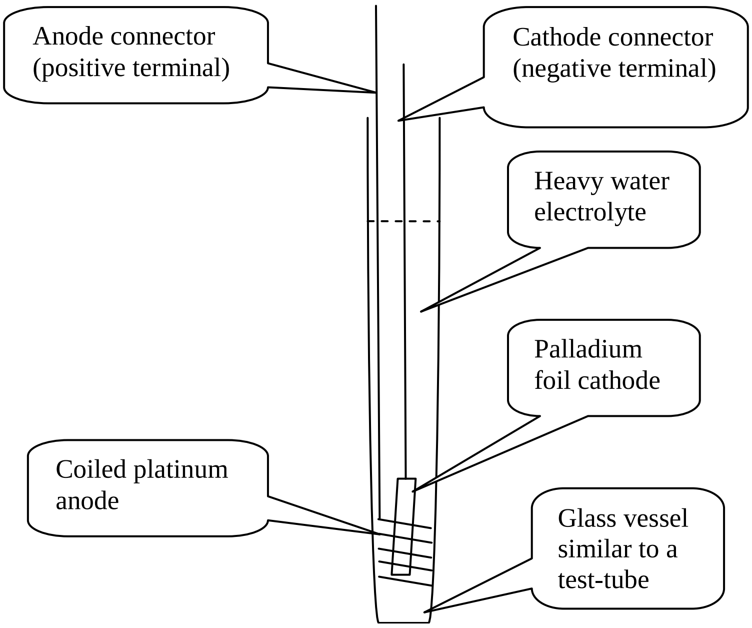

Figure 2 A simplified Pons-Fleischmann type cell (calorimeter details removed).

The Pons-Fleischmann experiment seemed simple enough. It consisted of an electrolytic cell, not unlike a car battery, but run in reverse using electricity to separate water into hydrogen and oxygen. Instead of using ordinary water in the electrolyte, Pons and Fleischmann used a special kind of water: heavy water[2]. In heavy water, ordinary hydrogen is replaced with a heavier type

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[2] “Heavy water” has special connotations for physicists. It is used in special “fast breeder” nuclear reactors that create plutonium, a component of some atomic bombs.

of hydrogen called deuterium (an isotope of hydrogen). Deuterium has a proton and a neutron in its nucleus (centre) compared with ordinary hydrogen, which just has a proton. These extra neutrons make heavy water about 10% heavier than ordinary water.

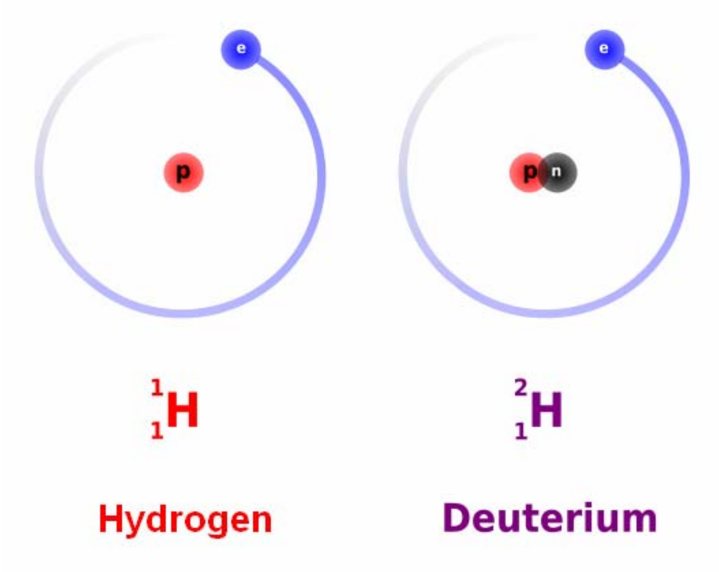

Figure 3 Hydrogen and its heavy water isotope deuterium (Hydrogen Deuterium Tritium 2009).

Next they used a special precious metal, palladium, for the cathode (negative electrode) and a platinum coil for the anode (positive electrode). Palladium is special because it absorbs astronomical volumes of deuterium. Palladium will hold as much as one atom of deuterium for every atom of palladium. The ratio of palladium atoms to deuterium atoms is important in cold fusion. It is called the loading ratio.

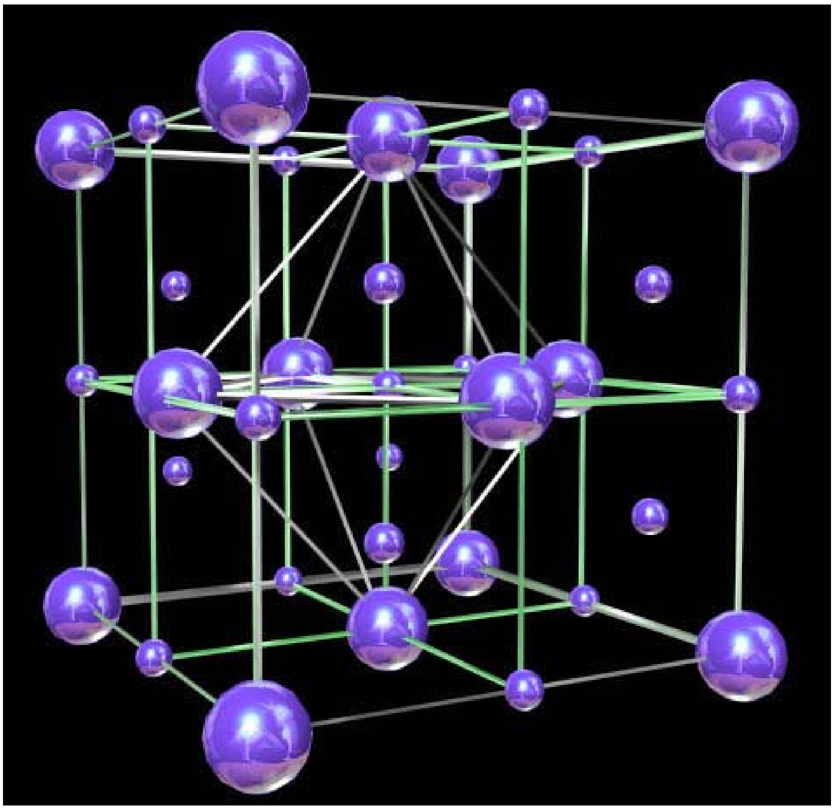

Figure 4 A model of deuterium in the palladium crystal lattice (the small spheres are deuterium) (Mosier-Boss 2009).

The next step was to place the cell in a special heat measuring apparatus called a calorimeter. The calorimeter measures the amount of heat energy leaving the cell as input energy is applied. Then Pons and Fleischmann applied an electric current to the cell for days, weeks or even months[3]. The applied current separated atomic deuterium from the heavy water and drove it into the palladium.

According to the principle of the conservation of energy, the amount of heat leaving the cell as measured by the calorimeter should equal the amount of energy applied to the cell by the electric current. This is exactly what Pons and Fleischmann found whilst the cell was “charging” for days or months. Then, suddenly, usually because something upset the equilibrium of the cell, the power measured by the calorimeter shot up. The power coming from the cell was 10, 20 up to 50% higher than the power applied by the electric current.

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[3] A long time was needed to load enough deuterium into the palladium. It is now known that there needs to be at least 85 deuterium atoms for every 100 palladium atoms for cold fusion to work (D/Pd loading ratio of 0.85) (Cravens and Letts 2008).

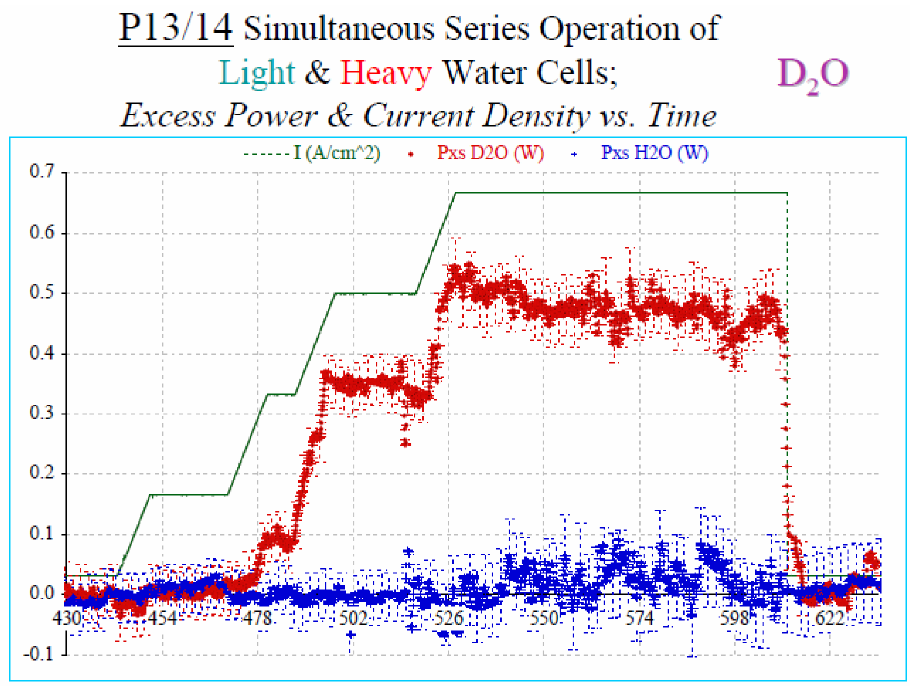

Figure 5 Excess Heat from a Pons-Fleischmann type cell (McKubre 2007)

Pons and Fleischmann were chemists. They could show that there was no known chemical process that released the amount of energy they measured. The only source that could produce such energy from such a small piece of palladium was a nuclear source. For most scientists however, Pons and Fleischmann's nuclear hypothesis was implausible and there were significant theoretical objections to it.

The most compelling favourable evidence was the sheer quantity of heat being released from the cells. When a chemical reaction takes place, for example the lighting of a match, each atom involved in the reaction releases around one electron volt of energy[4]. But in the Fleischmann-Pons apparatus each atom was releasing millions of electron volts of energy. There was as much energy per gram of palladium coming from the Pons-Fleischmann cell as coming from the core of a nuclear power station (Rothwell 2007 p23).

The only explanation was that the energy was coming from a nuclear reaction. Although the duo detected neutrons (subatomic particles that partly make up the nuclei of atoms), about a billion times fewer neutrons were detected than would be expected if the reaction were nuclear. Another problem was that the type of reaction they hypothesized is very rare in normal (vacuum) nuclear physics. Finally the reaction would have to overcome the "Coulomb barrier"

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[4] An electron volt is 4.45049017 × 10-26 kilowatt hours or about one hundred trillion trillionth of a unit of electricity or the energy in a typical meal. Chemical reactions produce tangible and useful amounts of energy because of the vast number of atoms taking part in the reaction.

that caused positively charged deuterons to repel each other just as like poles of magnets repel each other. The repulsion energy between two deuterons is 140 thousand electron volts, hundreds of thousands of times more than any individual chemical reaction could provide. Room temperature experiments simply could not provide the deuterons with enough energy to overcome the Coulomb barrier. Even if Pons and Fleischmann had discovered a way to overcome the Coulomb barrier, in theory, the radiation from the fusion going on in their cells would be enough to kill them several times over. Yet they were both still safe and well.

These objections were not raised at the press conference. The scientific community, led by the nuclear physicists, took some days to articulate them publicly. Most nuclear physicists and many other scientists were incredulous when they heard the story but were willing to suspend disbelief for a few weeks[5] until some of the experiments had been replicated (see chapter 4).

For many scientists, replication was the acid test. Could other independent scientists get the same results as Pons and Fleischmann when employing the same kind of apparatus? If not then the theoretical objections are probably true and Pons and Fleischmann must have made some kind of error.

Frank Close, a physicist and articulate critic of cold fusion summarised the dilemma that the critics applied to the results.

> As Fleischmann was claiming fusion on the basis primarily of his heat measurements, the simplest explanation of the ‘effect’ was that they had made an error in these measurements. However, if fusion was really taking place then there should be radiation in the form of gamma rays and neutrons coming from the cell. (Close 1990 p106)

Until the 2nd of May 1989, the scientific world was in pandemonium. Hundreds of laboratories around the world rushed to try to replicate this “simple” experiment. But there were no details available. Scientists faxed the latest newspaper articles to each other. They played videotape footage of the press conference and laboratory tour over and over to try to glean more details about the experiments. They used Pons’ wrist size to try to estimate the dimensions of the cell[6]. They shared information on electronic bulletin board systems – a precursor to the Internet. Some tried to telephone Pons and Fleischmann only to find their phones constantly engaged with other conversations and queries from the media. The University of Utah press office and

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[5] Garwin ceased the suspension of his disbelief no later than April 20, 1989 (Garwin 1989)

[6] Ironically, they were looking at a new cell design that Pons and Fleischmann had not yet tested. This new design was deemed more photogenic than the cells actually used by reporters.

the chemistry department were overwhelmed with inquiries. Some physicists phoned their colleagues in the physics department at the University of Utah only to find they knew nothing of Pons and Fleischmann's experiments. They'd assumed that Pons and Fleischmann had collaborated with University of Utah physicists to have the right expertise on the team. This led some physicists to suspect Pons and Fleischmann's neutron measurements.

In those first few weeks there were some reports of confirmations, some partial confirmations and some negative results. But then problems with the positive results began to be reported. There was a loose wire here, poor calibration there and - allegedly - thermal gradients somewhere else leading to improper temperature measurements. During April 1989, scientists slowly started becoming suspicious and puzzled. Maybe Pons and Fleischmann had made simple errors like those who retracted their early results.

The turning point came at the American Physical Society meeting on the 1st of May 1989. Experimental chemist Nathan Lewis and his theoretical physicist colleague Steven Koonin, both from Caltech, delivered a fatal double blow to cold fusion. Lewis reported on experiments that suggested that Pons and Fleischmann had made heat measurement errors by failing to stir their cells. Koonin went further claiming Pons and Fleischmann were incompetent and deluded. To the delight and applause of the assembled physicists and press, Lewis and Koonin had cast cold fusion out of mainstream science.

The final nails were put in the cold fusion coffin in November 1989. The cold fusion news had not escaped the notice of US President George H.W. Bush. By Presidential directive, a panel chaired by John Huizenga, professor of chemistry and physics at the University of Rochester, was assembled in April 1989 by the US Department of Energy to investigate the cold fusion claims. Over several months they examined the evidence for cold fusion and visited some laboratories conducting experiments. In November their report was published. The report recommended no money for cold fusion saying that there was a lack of "convincing evidence, for useful sources [of energy]" (DoE in Beaudette 2000 p93)

Cold fusion was now officially dead. It was almost impossible to get government funding in the US for experiments. Even if scientists could find a way to conduct experiments, most mainstream peer reviewed journals refused to publish papers. The US patent office refused to grant patents on any invention that mentioned or looked like cold fusion.

Cold fusion appeared to be an open and shut case: a couple of lone scientists got a bit carried away and claimed to have discovered something that ended up being a measurement error. It happens all the time in science. Now cold fusion is dead and buried we can go back to spending billions on hot fusion research (see chapter 2). Except that cold fusion wouldn't die.

For twenty years a small community of researchers around the world have continued to research cold fusion. They hold annual conferences (The 2009 International Conference on Cold Fusion (ICCF) is in Rome), they publish in peer review journals (Gordon 2009), they obtain (some) grant funding and they continue to discover more about this puzzling phenomenon. Most of the critics' scientific findings have been discredited (Krivit 2005; Mallove 1999b; Miles et al 1994; Cravens and Letts 2008; Fleischmann and Pons 1994). The mistakes detecting neutrons in Pons and Fleischmann's original experiments have been corrected (Mosier-Boss et al 2009). Evidence has emerged of scientific fraud and unethical behaviour on the part of some critics (Mallove 1999a; Beaudette 2000; Krivit and Winocur 2004). Certain cold fusion experiments can be replicated at will (Arata and Zhang 1997). The research community is slowly gaining high profile converts (Duncan 2009; Park 2009) and now, in 2009, there seems to be indisputable proof that cold fusion is real (Cravens and Letts 2008).

The question I'll be answering in this dissertation is: if cold fusion is not experimental error, why was it rejected by the mainstream scientific community in 1989 and subsequently? To answer this question, I'll first investigate *how* this rejection was achieved.

I'll argue that cold fusion's rejection from canonical science was an exercise in demarcation – the traditionally philosophical problem of how to distinguish science from non-science - rather than scientific fact-finding. It was not sufficient for the community of cold fusion critics to prove that cold fusion was factually false. Rather they acted to cast it right out of mainstream science (Goodstein 2000), to designate it a pseudo-science. Those that researched cold fusion were consequently pseudo-scientists. Cold fusion is a case study in how demarcation is achieved practically in the scientific community.

This demarcation approach provided critics with several advantages over simply proving cold fusion factually wrong. First, it allowed the scientific establishment to deny cold fusion researchers resources such as money and publication in influential journals. Second, it allowed the use of personal, social and political attacks on cold fusion researchers to count as arguments against cold fusion; scientists who are deluded or fraudulent are not taken seriously.

Third, if cold fusion is cast as a pseudo-science, then its experimental results cannot be used to challenge the existing theories of nuclear physics, or the existing budgets. Finally, after being cast as a pseudo-science, it is very difficult for cold fusion to rehabilitate itself back into a mainstream science using scientific evidence alone[7].

The critics' demarcation approach, however, does have a serious flaw. If cold fusion was rejected socially and politically rather than scientifically, it can be rehabilitated using social and political arguments. The cold fusion community thus has a strategy for getting cold fusion accepted back into the mainstream scientific community: stop making scientific arguments and start making social, political and economic ones.

This dissertation is not a scientific one. I will not be examining the scientific merit of the proponents' and the critics' evidence[8]. Rather, I will show that the main arguments used by the critics against the reality of cold fusion are not scientific arguments at all. The method by which critics presented their arguments and the reasons behind the arguments are political, social and economic, not scientific. And these arguments are used to demarcate cold fusion as a pseudo-science rather than to prove it wrong.

If the death of cold fusion is a murder, what is the motive? In the next chapter (chapter 2), we consider the Manhattan Project to build the world's first atomic bomb. The project propelled the world into the nuclear age. It propelled physicists in the US into powerful and influential positions, politically, socially and economically. To own nuclear science and its greatest achievement –nuclear fusion - was to own the greatest threat and the greatest hope of humanity. Physicists would not give these up easily. So much so that an influential cold fusion critic contradicts himself in his attempts to protect the physics establishment in the US from the political, social and budgetary danger of cold fusion. He continues to this day. Perhaps, however, there is a more subtle relationship between the physicists and cold fusion, a relationship that goes back to the first application of nuclear science: national security.

There is no evidence of subtlety in chapter 3 as I examine how the physics community banished Pons, Fleischmann and cold fusion from mainstream science. *Ad hominem* attacks were levelled at the duo as the mainstream community wrested control of the cold fusion narrative

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[7] These tactics are used by the critics and proponents of other marginal sciences e.g. para-psychology in Collins and Pinch 1979.

[8] Three independent scientists weigh the evidence for cold fusion in Appendix A. I cannot avoid some scientific considerations. In those cases, I use a scientist to speak for me.

from Pons, Fleischmann and the mainstream media. Pons and Fleischmann were labelled fraudulent incompetents and their experiments “pathological science”. Although mistakes were made by Pons and Fleischmann, the fraud and incompetence seems to be mainly on the part of the critics. Research from science studies, communication studies and media studies shows how these attacks were extreme versions of methods regularly employed by the scientific community to maintain autonomy and funding. These methods are political, not scientific.

Chapter 4 investigates the cold fusion research that has been going on for the past twenty years. Suppression of positive results by mainstream peer reviewed journals has meant that most scientists are unaware of the sheer volume of evidence behind cold fusion. Nevertheless, in this chapter, I confine myself to questions of the philosophy and sociology of scientific knowledge. I show that it was impossible to prove cold fusion either right or wrong in 1989 using experiments alone because of a conceptual problem that faces all emerging science. It forced the critics to seek refuge in demarcation rather than experimental evidence. Since the demarcation path was chosen over proof, cold fusion cannot rehabilitate itself using experimental results alone. I outline an alternative strategy.

In chapter 5, I show that over 50 years of research in nuclear physics has led to the most successful scientific theories of all time. Hence some of the most powerful arguments against cold fusion are that it doesn’t agree with generally accepted theory. Yet many of the critics who espouse these arguments are implicit or explicit positivists. In particular, they subscribe to a version of positivism that they attribute to Karl Popper. According to this positivism, theoretical arguments against cold fusion have unacceptable consequences (namely that current theory should be revised otherwise nuclear physics is not a science). There is one theoretical argument, however, to which there is no easy answer: cold fusion lacks a viable alternative theory to canon nuclear physics. Arguments over theory have led to a schism within the cold fusion community as emotional as the original split with mainstream science.

Finally, the future is considered. I show that if everyone is to enjoy the standard of living he or she is dreaming of, something like fusion is needed to power humanity. Yet the actions of the mainstream scientific community continue to thwart practical alternatives to the most popular methods of hot fusion. I also draw brief parallels between cold fusion and the development of the transistor and the steam engine.

In telling the story of cold fusion I lean heavily on journalistic accounts such as Eugene Mallove’s *Fire From Ice* (1999a); journalistic accounts with more science such as Charles Beaudette’s excellent *Excess Heat: Why Cold Fusion Research Prevailed* (2000) (for), Frank Close’s tightly argued *To Hot to Handle* (1990) (against) and John Huizenga’s *Cold Fusion: The Scientific Fiasco of the Century* (1992)(against). A more recent journalistic account used is Steven Krivit and Nadine Winocur’s *The rebirth of cold fusion: real science, real hope, real energy* (2004)(for).

Other treatments used in this dissertation include media studies (Lewenstein 1991,1992,1995; Bucchi 1998; Dearing 1995), rhetoric (Gross 1995; Taylor 1996; Sullivan 1994), sociology of scientific knowledge (Pinch 1994, Collins and Pinch 1998; Simon 1999,2001,2002) science studies (Gieryn 1999), research ethics (Fleischmann 2000; Goodstein 2000; Brockis 2000; Chubb 2000), philosophy of science (Derry 1999; Fleischmann 2000; Goodstein 2000) and the science itself (see chapter 4 for references). These will be cited in the main text as required.

Chapter 2 The Shadow of the Bomb and the Taming of the Sun

In early 1945, Adolph Hitler ordered a new kind of long-range bomber to be designed. The Horten Ho 18 Amerika bomber would be a jet powered flying wing design. It would be able to fly over 11000 km at 1000kph and drop the Nazi nuclear bomb on American cities. The Nazi bomb was scheduled to be completed in 1946 and Hitler needed a new type of aircraft to take on America (Myhra 1998).

Fortunately, the Nazis were defeated before their nuclear bomb became a reality[9]. After the Americans forced the surrender of Japan using the nuclear bombs dropped on Hiroshima and Nagasaki, the political and military balance of the world changed. It would never be the same again. The awesome destructive power of nuclear weapons would dominate strategic warfare to this day. The impact of nuclear weapons on the world was foreseen decades earlier in science fiction. H.G Wells wrote in his 1914 novel *The World Set Free*:

> And these atomic bombs which science burst upon the world that night were strange, even to the men who used them. (Quoted in Brake 2009)

Wells had predicted nuclear weapons. Mark Brake (2009) has argued that Wells not only predicted nuclear weapons, he inspired them. After reading *The World Set Free*, Leo Szilárd suggested the atomic chain reaction could be used to power a weapon. During an interview with Szilárd, Einstein wrote a letter to Roosevelt in August 1939 saying, “that it may be possible to set up a nuclear chain reaction in a large mass of uranium, by which vast amounts of power…would be generated.” (Einstein in Stoff et al 1991 p18) Roosevelt was interested and the Manhattan Project was born.

The Manhattan project cost as much as $20Billion (Lerner 2008). Its aim was to create an atomic bomb; a bomb powered by nuclear fission. In fission, very large nuclei (centres of atoms) like those of uranium and plutonium are split into smaller nuclei. This releases a great deal of energy. By the end of World War Two, the US had several functioning fission bomb designs. Some historians argue that the use of a uranium fission bomb on Hiroshima and a plutonium device on Nagasaki helped to end the War sooner. What is agreed is that these nuclear

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[9] There is doubt as to whether the Nazi nuclear weapon would have become a reality even in 1946. Heisenburg writes: “We have often been asked…why Germany made no attempt to produce atomic bombs. The simplest answer… is this: because the project could not have succeeded under German war conditions.” (Heisenburg in Hentschel 1996 p378)

weapons killed more than 112000 people directly and injured tens of thousands more (Trinity and Beyond 1995).

In addition to powering the Manhattan Project, nuclear fission is used to fuel conventional nuclear power plants. The fuel and waste products of fission are highly radioactive resulting in dangerous fallout from fission weapons and the nuclear waste problem we have today.

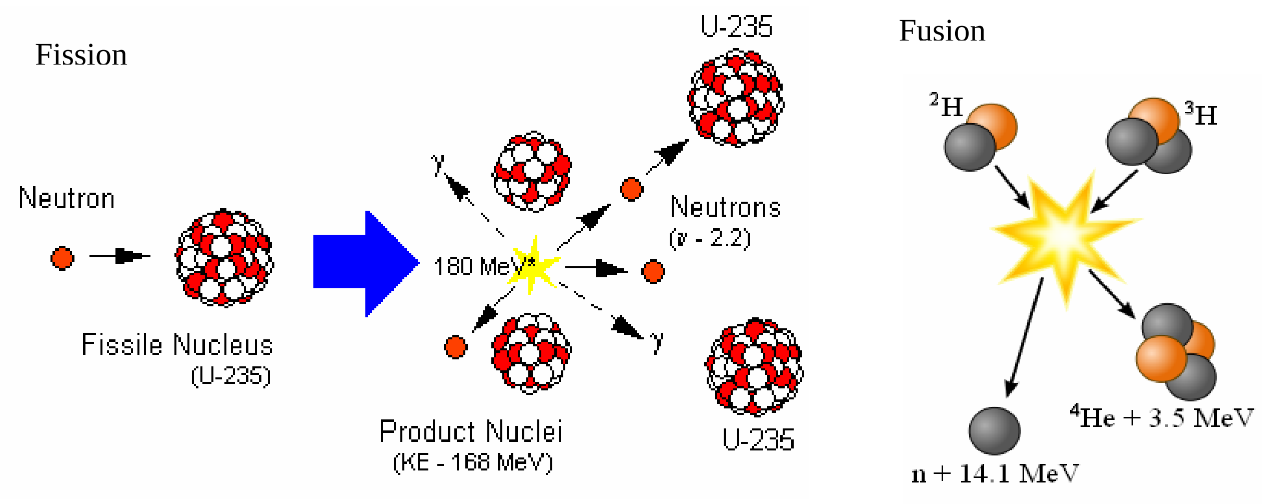

Figure 6 Nuclear Fission (left) (Space Power 2009) and Fusion (Deuterium-Tritium fusion 2009)

Fusion is the opposite process. Instead of splitting very large atoms, fusion joins together some of the smallest: two isotopes of hydrogen - deuterium and tritium. Tritium has a proton and two neutrons and is mildly radioactive. By smashing a tritium and a deuterium nucleus together at 100 million degrees Celsius, the two will fuse together to form helium and release a powerful neutron. A fusion reaction like this releases more energy than a fission reaction for the same weight of fuel.

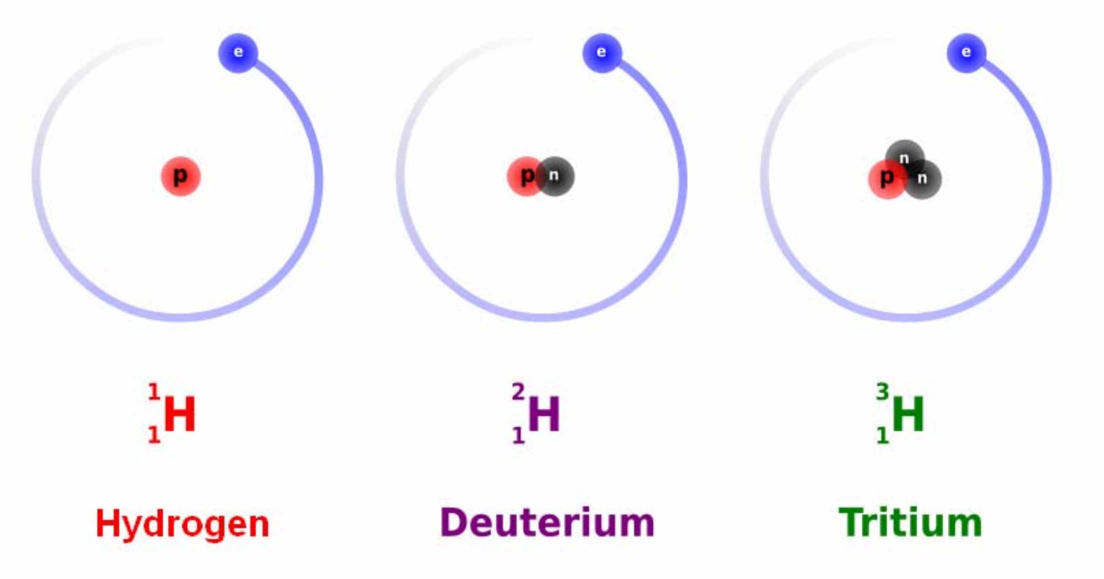

Figure 7 All three isotopes of hydrogen (Hydrogen Deuterium Tritium 2009).

Edward Teller, the father of the fusion powered hydrogen bomb, was convinced by a speech by Roosevelt during World War 2 that scientists should help the war effort by working on new weapons (Trinity and Beyond 1995). In 1952 his contribution was to test America's first fusion powered hydrogen bomb codenamed "Mike". The Soviet Union's hydrogen bomb programme was not far behind. RDS-37, the USSR's first true hydrogen bomb, was tested in 1955. The largest hydrogen bomb ever tested was the *Tsar Bomba* (codenamed Ivan) tested by the USSR in 1961. It had the destructive power of 50 million tons of TNT, 2000 times more powerful than the atomic bombs used on Japan. This massive power is more than the total power of all bombs used in World War 2 including the two fission atom bombs used by the US on Hiroshima and Nagasaki.

Figure 8 Tsar Bomba codenamed "Ivan" 1961 (Tsar Photo 2009)

A hydrogen bomb has never been used in war but the threat of their destruction has hung over the world since the 1950’s (the vast majority of US and former Soviet warheads are hydrogen devices). The spectre of the hydrogen bomb led to the “duck and cover” campaigns from the late 1940’s to the 1980’s in the US (Duck and Cover 1952). “Duck and cover” described what to do in case of a missile warning or a bright flash seen on the horizon. It also led to the MAD doctrine. Mutually Assured Destruction meant that both the US and the Soviet Union would be destroyed in a full-scale nuclear war. MAD meant that the cold war would remain cold (Parrington 1997).

Fusion became a highly charged word. It carried with it the threat of the most powerful weapons humanity has ever created. But it also carried a promise. Fusion also powers the sun, which ultimately provides us with all our energy including the food we eat. If a way could be found to control a hydrogen explosion - to bring the sun down to earth - humanity would have the ultimate energy source. Fleischmann said he had two regrets from 1989. One was that he agreed to hold that press conference the other was that he called his findings “fusion” (in Krivit 2003).

Nuclear power today, as generated by conventional fission power plants, has a number of disadvantages. The fuel, uranium, is scarce, costly to extract, toxic, radioactive and could also be used for weapons. The waste is highly radioactive for thousands of years. The reactor is capable of meltdown or explosions releasing dangerous radioactive material. The industry had already experienced two major accidents that eroded public confidence (Rothwell 2007).

Fusion had none of these problems. It has almost unlimited fuel: seawater. One out of every 6000 molecules of seawater is deuterium oxide (heavy water). Deuterium is easy to extract and safe. Tritium could be manufactured on the fly in the reactor. The waste is helium that is safe enough to fill children’s balloons. The (most popular) reactor design is inherently safe as hot fusion reactions stop as soon as the fuel is cooled very much below 100 million degrees. The reactor itself would be radioactive at the end of its life, but not nearly as radioactive as waste from a fission plant. Non-military fusion seemed to have everything going for it.

Research began in the 1930’s and by 1955 the US, Soviet Union, UK, France Germany and Japan all had programmes (ITER 2009). Hot fusion reactions are very difficult to contain and control such that useful power can be produced. There is no material that can hold deuterium and tritium gas at 100 million degrees. Fortunately, at that temperature the gas turns into charged plasma that can be contained in a magnetic “bottle”. The most successful design was the Soviet “tokomak” that pinned the plasma in a donut shaped ring of powerful magnets.

Hot fusion has never worked as a source of energy. 70 years and more than an estimated $40 billion later (Bush in Fire from Water 1994), the best hot fusion results come from the JET tokomak in England. Its fusion reaction produces 60% of the power applied to the reaction (Krivit and Winocur 2004 p38). (In comparison the Energetics Technologies 2004 cold fusion experiment yielded 2500% excess power on one occasion. (Dardik et al 2004))

The scale and expense of these hot fusion experiments is breathtaking. The International Thermonuclear Experimental Reactor (ITER) which has an initial goal of yielding the same energy as input (100% yield) into the reaction will cost an estimated $20 billion (Sample 2009). ITER will weigh 23000 tons. The reactor building will be 57m high. It will operate at 150 million degrees Celsius. It will consume 620MW when operating (ITER 2009). It will occupy a 180-hectare site in Cadarache, Southern France.

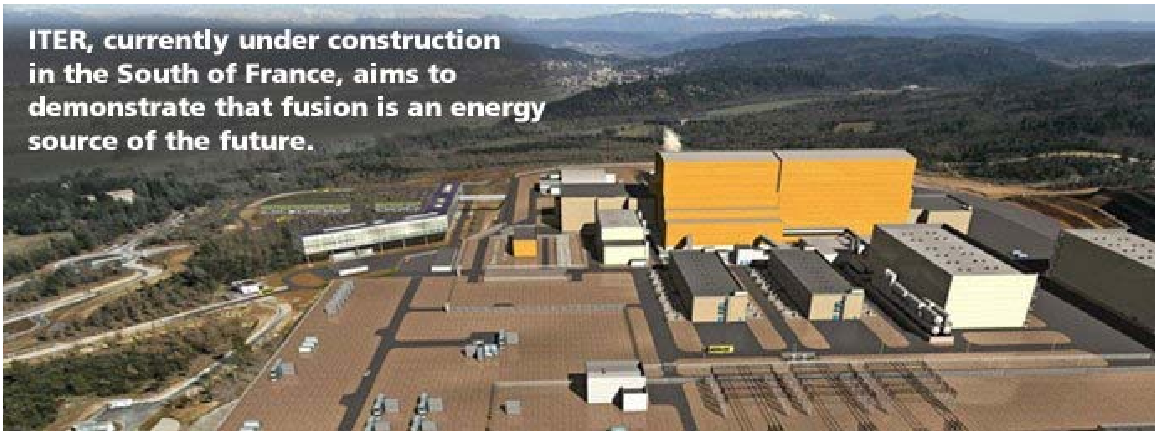

Figure 9 ITER site in Cadarache, southern France (Source ITER 2009 ©ITER used with permission)

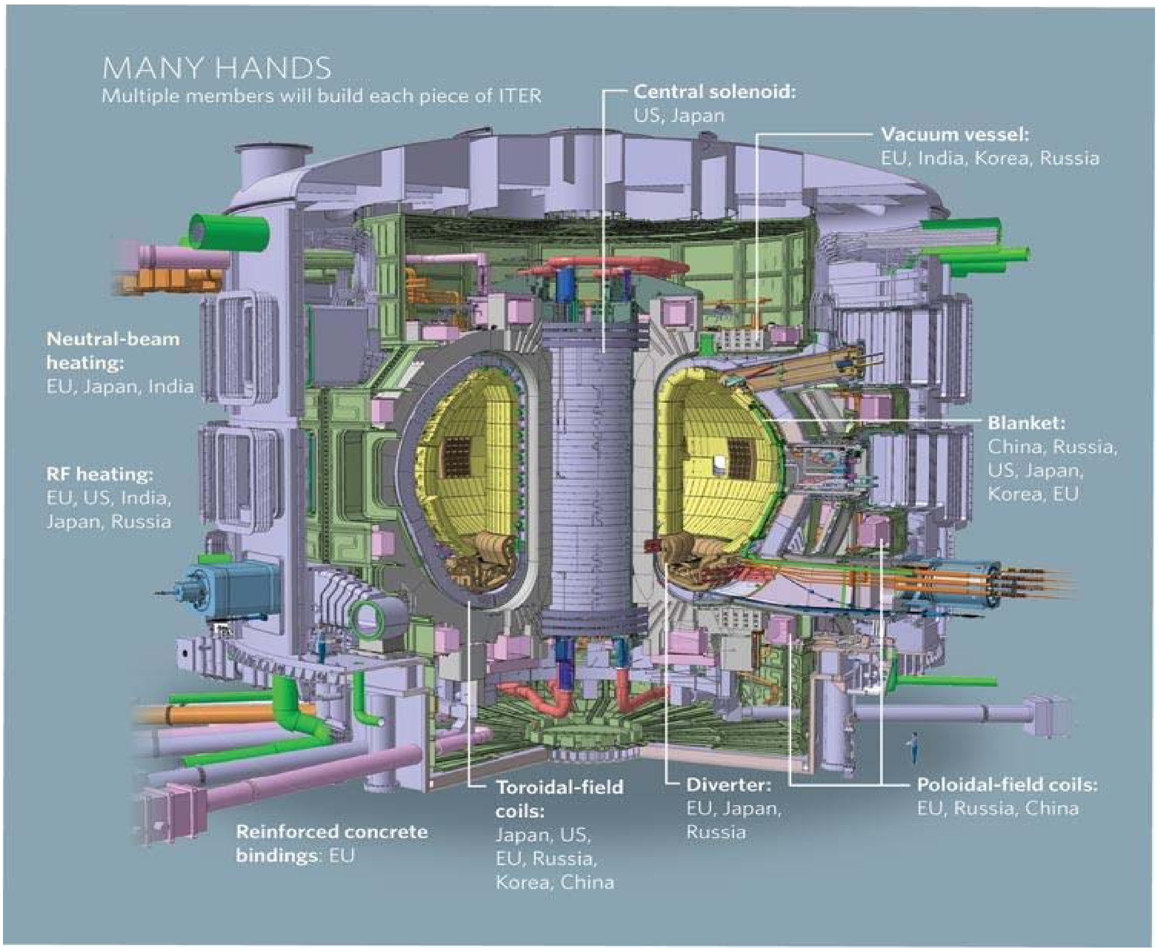

Figure 10 A diagram of the ITER reactor core (Source ITER 2009 ©ITER used with permission).

The ITER is the culmination of decades of research. But considering the vast investment required to complete the ITER by 2016, some doubt whether it will ever be built (E. Storms, private correspondence, 13 September 2009) and others doubt that it will result in more than a few reactors (Rothwell 2004 p20).

The funding of hot fusion has ebbed and flowed in the US since the 1950's. A review by Stephen Dean (2004) showed that Congress was enthusiastic about hot fusion in the 1960s and 1970s. Toward the end of the 1980s it was becoming more sceptical because of the seeming lack of progress toward a practical power source.

> 1989 was a particularly difficult time for the hot fusion community. They were under investigation. Questions were being asked why all the money had been spent and why so little progress had been made. Funding was being cut. The last thing that community wanted was the suggestion that there's a much simpler and cheaper way to achieve the same result. (McKubre 2002)

However, there were many alternatives suggested even before cold fusion came along. Amongst alternative hot fusion ideas were: inertial confinement fusion[10], electrostatic confinement fusion[11], focus fusion[12] and z-pinch fusion[13]. Congress, on the foot of various reports by distinguished scientists over the decades, had already picked their winners: tokomak fusion and inertial confinement fusion (Dean 2004).

The US commitment to inertial confinement was confirmed in 2009 with the completion of the National Ignition Facility (NIF). The NIF has the (stated) aim of achieving a sustained fusion reaction by inertial confinement. However critics have argued that the NIF is really a nuclear weapons research facility. Mini fusion explosions can be simulated in the reactor leading to some to suspect that the NIF is really a way to circumvent the Comprehensive Test Ban Treaty (Garcia 2009; Talcott 2000).

Figure 11 Installation of the “target chamber” at the NIF Inertial Confinement Fusion facility (Source NIF (CNS 2009)).

Tokomak fusion is also back on the agenda with the US rejoining the ITER consortium of counties (U.S ITER 2009).

The other types of hot fusion have been largely ignored by funding agencies. Approaches such as electrostatic confinement and focus fusion claim to need a few hundred million dollars to develop full commercial applications (Bussard 2006; Lerner 2007). Yet they continue to languish

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[10] A frozen pellet of deuterium and tritium is blasted from all sides by powerful lasers.
[11] The electromagnetic field accelerates ions into the plasma itself to confine it (Bussard 2006).
[12] The instabilities inherent in plasma are used to confine it (Lerner 2007).
[13] Current induced in the plasma confines it magnetically.

with a few hundred thousand each year. Similarly to cold fusion, these alternative hot fusion technologies promise practical solution for a fraction of the price of the preferred technologies. In addition, focus fusion and electrostatic confinement do comply with the currently accepted laws of physics. But the establishment physicists continue to recommend tokomak and inertial confinement fusion. Electrostatic confinement fusioneer Eric Bussard (2006) said of alternative approaches to hot fusion:

> The government will always turn to its government labs and the government labs will say no... The fusion community is a $2m a day rice bowl… ITER will take them into retirement. (Bussard 2006)

The question then is: why would scientists recommend expensive complex technologies that have not yet proven themselves despite decades of investment when more promising technologies with more favourable costs and benefits are ignored?

In his book *The Physicists* Daniel Kevles (1995) argues that since the Manhattan Project, physics and physicists in the US have had disproportionate political and economic power compared with other scientists. The success of the Manhattan Project and the fusion powered hydrogen bomb cemented physics as the indispensable tool of war. Being a military superpower required physics research on a massive scale. This meant not just many defence related projects but also some very large scale projects including civilian fusion research and powerful particle accelerators.

The access that physicists had to money and their trusted position as part of the military-industrial complex meant that they quickly rose to powerful advisory positions in the US government. Scientific advisory panels and task forces came to be dominated by physicists. Even today, president Obama’s energy secretary is a Nobel Laureate physicist.

Graduating with a PhD in physics meant a lucrative job for life working either directly or indirectly for the military on massive physics projects. In the 1950’s the US could not educate enough physicists. In 1956 at a the annual American Physical Society meeting “the recruiters mobbed the fifth and sixth floors of the hotel, enticing and pirating candidates for industrial, governmental and academic positions” (Kevles 1995 p370).

In the mid 1960s, physicists ranked third in occupational status in the US behind physicians and Supreme Court justices (Kelves 1995 p391). They earned more than most workers and more than their colleagues from other sciences. The public credited them with not just keeping them safe with nuclear weapons but also with inventions from the transistor to the space rocket. According to Kevles:

> 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 proper sense of that word: a set of institutions supported by tax funds, but largely on faith, and without direct responsibility to political control. (Kevles 1995 p392)

It's in this context that a man such as Richard Garwin can gain such prominence. Garwin was born in 1928 in Cleveland Ohio. He gained his bachelor's degree at Case Institute of Technology in 1947 and his PhD in nuclear physics at the University of Chicago in 1949. In 1952 he was asked by Edward Teller to design the world's first hydrogen (fusion) nuclear bomb "Mike". Since "Mike" thousands of nuclear warheads have been built in the US, Russia and other countries. But Garwin is unapologetic about his past. Instead of questioning the ideology that led to the hydrogen bomb, he has spent most of his career trying to limit its damage. "Physicist Freeman Dyson has called Garwin the conscience of physics in the second half of the 20th century" (Lane 2006). He has been advisor on national security to the US government since Eisenhower and has formidable influence on US nuclear, defence and technology policy. He is a fierce critic of nuclear proliferation and missile defence.

Figure 12 Richard Garwin

> Dick Garwin's research, creativity and fearsome intellectual capacity have helped shape much of the technological history of America's national security community over the past 50 years," said Norman Neureiter, director of the AAAS Center on Science, Technology and Security Policy. "He has also become one of our nation's scientific statesman, applying those same formidable skills to control proliferation, counter terrorism and reduce the threat from weapons of mass destruction on a global basis (quoted in Lane 2006)

Garwin is the quintessential physics establishment figure. His considerable intellectual and political success is a stark contrast to his discredited contemporary, Martin Fleischmann. Policy makers listen when Garwin speaks. On the 20th April 1989, Garwin wrote:

> Within the next few weeks, experiments will surely show whether cold nuclear fusion is taking place; if so, it will teach us much besides humility … Large heat release from fusion at room temperature would be a multi-dimensional revolution. I bet against its confirmation (Garwin 1989)

Was cold fusion as revolutionary as Garwin makes out? Rob Duncan, Vice Chancellor of Research at the University of Missouri, "was …surprised there was such a surprise in 1989." (Duncan 2009) After all, the production of helium by the electrolysis of water was first reported in 1926 by Professors Fritz Paneth and Kurt Peters (Paneth and Peters 1926). They were tying to artificially produce helium because it was a valuable gas for the safe inflation of airships. They later thought that their results were due to experimental error and eventually gave up. Another German, Alfred Cohn, reported on Paneth and Peters' experiments and this report had caught Fleischmann's attention but this was not the only precedent.

Another type of cold fusion was well known to Fleischmann and accepted by the mainstream scientific community. It is known as muon catalysed fusion[14]. The first reported cold fusion by muon catalyst was in 1956 by Luis W. Alvarez et al. (1957) at the University of California, Berkeley. Unfortunately natural muons are hard to capture and artificial ones are difficult to produce. Muon catalysed fusion has, so far, proved to be an impractical source of energy.

With such a history, it seems strange that Garwin continues to be sceptical of cold fusion when even hardened critics like Robert L. Park (2009), a physicist who was Executive Director of the Office of Public Affairs of the APS in 1989, have given up. It's strange because actually Garwin is only *publicly* sceptical of cold fusion.

In 1993 Garwin and Nathan Lewis were asked by the Pentagon and the Electric Power Research Institute (EPRI) to take another look at cold fusion experiments being performed by cold fusion researcher Michael McKubre at SRI International. The Pentagon's choice was not

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[14] Muons are super heavy electrons (electrons are particles that orbit the nucleus in an atom) that strike the earth in vast quantities from the sun and other stars. Because muons are like electrons in every way except they are heavier, every so often they are captured by atoms and displace electrons in materials on earth. If deuterium gas is cooled until it becomes a liquid (-249C), it will capture the occasional muon. The muon will displace an electron. Because a muon weighs 200 times that of an electron, it orbits 200 times closer to the deuterium nucleus. The closely orbiting muon shields other deuterium nuclei from the repulsive Coulomb force allowing them to approach the nucleus and to fuse with it. (Duncan 2009)

accidental. Garwin was the chairman and Lewis was a member of the secretive JASONS organisation, a group of mainly physicists that has advised the US government on military spending priorities since the 1950's. (Krivit and Winocur 2004)

Garwin and Lewis spent two days at McKubre's lab and confidentially reported:

> A chemical reaction involving Pd [palladium] of perhaps 1.5eV per atom would correspond to about 3.5kJ [3500 Joules] of heat; this is to be compared with the 3MJ [3000000 Joules] of "excess heat" observed, so such an excess could not be of chemical origin…
>
> We have found no specific experimental artefact responsible for the excess heat, but we would like to see … a larger effect and one that can be more reliably exhibited. (Quoted in Krivit and Winocur 2004 p136)

Garwin and Lewis effectively concede that cold fusion is not a mistake. But compare this to his public statements. In his 2009 interview with 60 minutes, Garwin said of McKubre's experiments:

> I think probably he measures the input power wrong. (More than Junk Science 2009)

Garwin was satisfied back in 1993 and, if anything, McKubre's results have become more reliable, stronger and better replicated than in 1993 (McKubre 2007).

Lewis was no better. At the Baltimore APS meeting in 1989 (see chapter 3) he accused Pons and Fleischmann of an undergraduate error: failing to stir their cells. But he found no such problems in McKubre's cells.

Why would Garwin, the public face of establishment physics, hold a position demonstrably lacking in integrity? Lewis was clearly convinced there were no errors in 1993 so why did he not retract his 1989 accusations of poor experimentation? Are these the actions of scientists or of political operators trying to protect a budget of billions?

Nothing could be more embarrassing or potentially damaging to the physics establishment than cold fusion. First of all, it threatened the monopoly on nuclear science held by the physicists since the Manhattan Project. If anyone could run a nuclear reactor invented by chemists on their desktop then what justification was there for a lavishly funded physics establishment? How could the physicists protect us from desktop nuclear weapons? How could they help us extract energy from the atom when the chemists had already done it better and for less money?

More importantly, cold fusion undermined the credibility of physicists. Why hadn't they discovered cold fusion considering all the money they'd been given? As hot and cold fusion physicist Robert Bush put it:

> Gee, that'd be marvellous if they're doing it. But on the other hand, my God, *chemists* doing it for probably pennies compared to what physicists have taken from the public coffer over the past 40 years: in the order of $40B to do hot fusion. (Fire from Water 1995)

The physicists had become dependent on the hot fusion funding. The prestigious MIT hot fusion laboratory, for example, was used to receiving tens of millions of dollars each year (see chapter 3). This would be threatened by cold fusion.

There is also a psychological resistance to cold fusion among physicists, not just to the science but to the whole approach of Fleischmann and Pons. Jeff Schmidt (2000) presented a harrowing analysis of the graduate training of physicists and their subsequent employment. Students entering graduate school often have a love of physics and idealism about physics' contribution to society. Also physics is still a great career choice in the US. There are good salaries and plenty of work researching on behalf of the military, the Department of Energy and the National Science Foundation.

Over years of gruelling coursework and exams at graduate school, slowly their idealism and their love for the subject are driven out of them. Their training culminates in a research project being assigned to them with narrow disembodied technical objectives. They are often closely managed or "ridden" by their supervisor. They are trained to crunch the numbers or design experiments within strict creative boundaries.

Alienation from their own values is good preparation for their subsequent employment as researchers. Research that will be used to develop new weapons is couched by the military funders in narrow technical terms. These technical problems are then solved by researchers who focus on the technical solution rather than its application in weapons, just as they are taught in graduate school. Often, they "adjust" their personal interests to align with the requirements of the military funding agencies that provide 99% of basic physics funding in the US (Schmidt 2000).

The training creates specialists who are experts in narrower and narrower slices of science. They are uncomfortable talking about parts of their own science outside of their expertise (e.g. Lewis and Morrison on calorimetry below), let alone political, social and philosophical questions.

Imagine being a physicist. After several years in graduate school you've had your idealism beaten out of you. You now compete with your colleagues to work on narrow technical problems assigned to you by military funding agencies. You know that your work will be used for weapons but you're too battered to fight. In fact, just like Edward Teller, you've convinced yourself that making weapons is ok; it's defending humanity (Trinity and Beyond 1995).

Now imagine the gall of a pair of electrochemists with free time and personal resources to research what they like. They can be fully creative. They are alive with idealism that their discovery could help save the world from dwindling oil supplies and global warming. They could even change physics itself with their new “unknown nuclear process”. These chemists are now famous, on the cover of *Time* and *Newsweek*. They are the celebrity scientists that one dreamed of being when one was a child. These chemists represent all the idealism and service that had to be left behind in graduate school. The feeling of loss is unbearable. And to rub salt into the wound, these upstart chemists go to Congress, trying to bypass the funding agencies you have had to grovel before for decades. And if that is not enough, they intend to steal physics' money by diverting it from the hot fusion budgets. It's an outrage! They must be stopped!

It is not that physicists were bitter and envious as people. Rather, the system of the training and employment of scientists deliberately alienates them from their natural human idealism and sense of service. It narrows their interests and their areas of expertise. It actually alienates them from the applications of the science they research and turns them into unquestioning servants of the military funding agencies. There seems to be no other reason that could explain how Richard Garwin, in clear conscience, designed a weapon with the potential to wipe out a city of millions of people in one explosion.

It is more likely that cold fusion will be used to greatly bolster conventional war capability by powering planes that fly until they wear out and ships that never need refuelling (E. Storms, private correspondence, 13 September 2009). The ability of conventional forces to free themselves from fuel supply lines is a massive advantage in war. It seems plausible that countries building their conventional capabilities such as China would pursue research into cold fusion (E. Storms, private correspondence, 13 September 2009).

Yet the relationship between cold fusion the scientific-military establishment in the USA is complex and subtle. The progress made by US cold fusion scientists in the last 20 years rests to a great extent on two sources of funding. The Electric Power Research Institute (EPRI) is a private consortium of electric utilities in the US. It was an unwavering supporter of cold fusion research by Michael McKubre at SRI International until 1998. The other source is the US Navy that has unwittingly supported 20 years of research at the Naval SPAWAR research facility near San Diego.

> While the SPAWAR group's cold fusion research was unfunded for most of these 20 years, [the Head of Navigation and Applied Sciences Frank] Gordon was able to use some discretionary funds throughout this period and his team worked on cold fusion research primarily during their own time. What Gordon had going for him that was unique was his position as a senior executive at SPAWAR and a courageous Navy group - SPAWAR - behind him (Krivit, private correspondence, 30 September 2009).

However, pronouncements about the reality of cold fusion by scientists with prestigious reputations such as Garwin and Lewis are taken seriously by science journalists (Reed 2001 and e.g. More than Junk Science 2009). Considering their private views on cold fusion, these public pronouncements should be taken as political actions, helping to secure the reputation and funding of physics and in particular hot fusion in the United States. In the next chapter, I will show just how damaging these public pronouncements were and continue to be.

Chapter 3 The Excommunication of Pons and Fleischmann

Pons and Fleischmann liked to walk. As they walked up Millcreek Canyon to the north of Salt Lake City they began to concoct a “preposterous” experiment. “It has one in a billion chance of working,” said Fleischmann (in Krivit 2003). It was 1984. Their discussions continued in the kitchen of Pons’ home in the suburbs of Salt Lake City. Catalysed by Jack Daniels whisky, they decided to undertake an experiment based on the electrolysis of heavy water using a palladium cathode. They knew that palladium could absorb enormous quantities of deuterium. As palladium was a solid, loading deuterium in this way would be equivalent to compressing the deuterium to a trillion trillionth of its original volume. At that density strange things could happen. They would fund the experiment themselves. “It was almost incorrect to ask for financial support for a project with a low probability of success,” recalls Fleischmann (Fleischmann in Mallove 1999a p46).

By late autumn 1984 they had had a cubic centimetre of palladium immersed in a bubbling flask of heavy water for several months. One evening Pons had asked his son Joey, who helped him in the laboratory, to adjust the current to the cell. Joey went home. The next morning Joey was the first to enter the lab. He found it in disarray. Here’s how graduate student Kevin Ashley witnessed the scene:

> “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 [sic] 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.” (Quoted in Beaudette 2000 p36)

Part of the palladium electrode had vaporised (melting point 1554C) and the rest had melted. Pons and Fleischmann knew that there was not enough energy in the deuterium gas to account for the damage by a chemical explosion; it had to be nuclear. But there did not seem to be any dangerous radiation. “If it had really blown up it would have been a very serious matter… it could have made the university disappear,” Fleischmann said later (in Krivit 2003). They continued experimenting but with much smaller quantities of palladium.

By 1988 Fleischmann was convinced that the research should be classified. “There were lots of military reasons why it should not have gone into the public domain…I always thought it needed a Manhattan 2 type project,” he said (in Krivit 2003). He and Pons approached the military in both Britain and the US. Neither government was interested.

Early in 1989, Pons and Fleischmann felt that with another year of research they’d be ready to publish. But they were running out of money. They applied for a research grant to the US Department of Energy (DoE). As part of the funding process, the duo’s proposal was sent to peers for review. One of those peers was Steven E. Jones of the physics department of nearby Brigham Young University only 40 miles from Salt Lake City. Jones was an expert in muon catalysed fusion (see below). Jones was working on his own experiments to create fusion at room temperature. According to Krivit and Marwin (2009), in September 1988 Jones most likely would have received a phone call from Ryszard Gajewski, project director of the Department of Energy’s Advanced Energy Projects Division who was reviewing Pons and Fleischmann’s application. Around that time Jones began working on his own cold fusion experiments using electrolytic cells.

Figure 13 Steven E. Jones (Credit: Center for the Study of Ethics at UVU)

Instead of using a sensitive calorimeter to measure the heat balance of the cells, he used a neutron counter to measure neutrons that would be created in the fusion reaction. He found that heavy water produced slightly more neutrons than light water compared to the background count, a sign that some fusion was taking place.

Krivit and Marwin (2009) write that when Pons and Fleischmann found out about Jones’ work in February 1989, they and the University of Utah sought to collaborate with him and Brigham Young University. But in early March 1989, Jones told the duo that he would announce his results at the May 1st American Physical Society Meeting, despite the duo's pleas for him to wait 18 month so they could complete their own research. With the threat of losing priority and patent rights, Pons, Fleischmann and the University of Utah decided to go public on the 23rd of March 1989. Although they were not ready to publish, they were forced to hastily pen a preliminary note and had it accepted by the Journal of Electroanalytical Chemistry.

At that time I'm not sure whether Pons and Fleischmann truly appreciated the scale of what was at stake. World energy consumption in 2006 was 500 exojoules[15] (US Energy Information Administration 2006). Assuming the world only consumed crude oil for all its energy needs[16], this represents a market of $6 trillion per year at $70 per barrel of oil. Thus energy was approximately 10% of the total world gross domestic product of $60 trillion in 2006 (US Energy Information Administration 2009). A successful cold fusion device would be worth hundreds of billions to its inventors making them and their universities very rich indeed. As it turned out, events overtook Pons and Fleischmann in 1989 and the promise of cold fusion seemed to be extinguished as quickly as it was announced. Nevertheless, it took two exceptional people to not only have the vision to create cold fusion, but also the courage to announce it to the world.

Figure 14 Pons (left) and Fleischmann in 1989 (Today in Technology 2009 Credit: unknown)

Fleischmann in particular had a reputation for being a maverick, an original thinker and a brilliant experimentalist. He was born in the Czech Republic in 1927. His mother was a Roman Catholic and his father was an orphan who was adopted by the Jewish Fleischmann family. His father's Jewish connection meant the Fleischmann family twice had to escape persecution at the hands of the Nazis. Ending up in England penniless, the family were forced to split up. Fleischmann was adopted by a bachelor and moved to south Wales.

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[15] 500 000 000 000 000 000 000 Joules. A unit of electricity is 3 600 000 Joules.

[16] This is a better estimate than it might seem at first glance. Although coal, hydro, geothermal and some other sources are cheaper than oil, this estimate does not include the cost of the massive capital equipment used in exploration, production, conversion and distribution of energy.

Fleischmann was accepted to study chemistry at the Imperial College in the University of London. He began his doctoral studies in 1948 and had earned his doctorate two years later. He married his wife Shelia Flynn straight after graduation and they have one son and two daughters.

After graduation, Fleischmann took a position teaching at Durham University. And in 1967, he was offered the chair of electrochemistry at the University of Southampton. He was just 40 years old. He was president of the International Society of Electrochemists from 1970 to 1972. In 1979 he received the Royal Society medal for electrochemistry and thermodynamics. After retiring from the University of Southampton in 1982, he received the Palladium Medal from the US Electrochemical Society in 1985. In the same year he was elected as a Fellow of the Royal Society, the highest honour for a British scientist. In 1989 Fleischmann was, perhaps, the world's foremost electrochemist.

B. Stanley Pons was also a brilliant electrochemist. Born in 1943, he was raised in the small town Valdese, North Carolina. He was the son of an industrialist. After graduation from Wake Forest University in 1965, he went on to more advanced studies at the University of Michigan. But he failed to complete his PhD and went to work in the family business for 10 years. However, his real love was chemistry and he longed for more intellectual pursuits. He entered the graduate programme at the University of Southampton and graduated with a PhD in 1978. Pons, after graduation, held several academic posts before being appointed associate professor of chemistry at the University of Utah. In 1986 he was made a full professor and in 1988 he was promoted to head of the chemistry department. By 1989 he had co-authored over 200 academic papers and was an established and respected researcher. He has six children from three marriages. Pons and Fleischmann shared interests in skiing, hiking and cooking and they often experimented in Pons' Salt Lake City kitchen.

By the 23rd of March 1989, Pons and Fleischmann were at the top of their careers. Thanks to Pons' independent wealth, they'd pursued the science that they loved. In doing so, they discovered an effect with the potential to solve humanity's energy crisis.

Fourteen years later, Fleischmann was a broken man. In 2003 Steven Krivit interviewed him. He was asked about what had happened to cold fusion during the intervening years:

> It’s been obliterated really… it’s an ongoing process…the dominant feeling I have about my life is one of failure. Everything I wanted to do has failed. (Fleischmann in Krivit 2003)

Fleischmann still attended the International Conferences on Cold Fusion but in the eyes of mainstream science, he was a pariah – a pseudo-scientist. His triumph had turned into his downfall. His reputation was smashed and he retired to a small village in the English countryside. He continues, however, to agree to interviews. In a recent one, he said that the 1989 announcement and backlash were inevitable: "It was a very unfortunate time to try to float the idea." (Fleischmann in Cartwright 2009)

Pons has stayed out of the public view. In the early 1990’s he renounced his US citizenship and moved his family to France. He has given up cold fusion except for keeping in touch with a few researchers whom he trusts. (Storms in Richardson and Richardson 2004 p211)

How could two respected scientists, leaders in their field, have their lives and reputations so completed destroyed, that one of them questions the value of his life’s work and the other has abandoned cold fusion research. The answer cannot be that, as scientists, they were simply proved wrong in the peer-reviewed literature. Mistakes are essential to science[17] and scientists, crucially, are not derided and excommunicated from the scientific community for making them. If mistakes were cause for expulsion from the community there would be precious few scientists left to make up the community. No, the answer must be a great deal more personal and, by definition, social. Membership of the scientific community is a *social* status conferred by the community itself.

Massimiano Bucchi (1998) analysed the reports of cold fusion in the Italian press in 1989. For him the standard model of science popularisation implies that the media interprets, simplifies and disseminates science knowledge in one direction only: from scientist-producers to the public consumers (c.f. Hilgartner 1990). But, he argues, scientists also rely on the media “as a source of information and professional legitimation.” (Bucchi 1998 p42) These other “uses” of public communication become all the more visible in deviant cases when:

> They become structurally part of the scientific debate and are employed to question and defend different layers of science boundaries in the most dramatic and ‘theatrical’ way possible (Bucchi 1998 p42 quotation marks original)

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[17] At least according to Popper, the critics’ favourite philosopher of science “knowledge can grow, and …science can progress - just because we can learn from our mistakes.” (Popper 2002 p(xi)) (see chapter 4)

Cold fusion was one such “deviant” case. Pons and Fleischmann played their part in the drama. First they announced their results before formally communicating them to the scientific community. They were not forthcoming with scientific information and were unavailable to scientists on the telephone so the general media became *de facto* conduit for scientific information e.g. cell dimensions were estimated by reference to Pons’ finger size in a picture in the LA Times (Lewenstein 1995; Close 1990).

In the period 23 March to 30 April 1989 scientific debates on cold fusion happened by many press conferences and newspaper articles (Lewenstein 1995). Hot fusion researchers had to publicly defend their work in the face of the revolutionary findings. Hot fusion researcher at MIT Ian Hutchinson said, “Suppose you were designing jet airplanes and suddenly you heard on CBS news that somebody had invented an antigravity machine.” (Quoted in Mallove 1999a p41)

But by the end of April the “credibility index” (Gieryn 1999 p204) of Pons and Fleischmann was slipping into negative territory. The pair appeared before the US Congress in Washington DC on the 26th of April 1989. At stake was $25 million in federal funding proposed by Utah Congressman Wayne Owens. The funding would be diverted from the DoE hot fusion budget, (alt.fusion newsgroup 1989) much to the dismay of hot fusion physicists such as those at the MIT plasma fusion lab.

For Thomas Gieryn (1999) Pons and Fleischmann’s testimony before Congress was an example of “boundary work”. In his 1983 paper, Gieryn demonstrated that scientists routinely shift between two modes of talking about science and technology, depending on what suits them politically and economically. Thus if scientists wish to demonstrate the benefits of science in order to secure more funding, they will conflate science and technology so that science can share the credit for the economic and social benefits that technology has brought humanity. But if, for example, politicians seek to control the research interests of scientists too much, thus undermining their autonomy, scientists will radically demarcate science from its technological applications. Science, they will claim, is value-free enquiry into truth. It should not be interfered with by political, social or even technological motives (c.f. Wolpert 2005).

Gieryn (1999) suggested that their testimony was part of a strategy by Pons and Fleischmann to blur the boundary between science and technology. By blurring the boundary, the public and Congress could take part in “deciding the implications and validity of a claim, and thus steering the direction of future scientific inquiry” (Gieryn 1999 p205). This blurring between science and its application meant the $25m could be used to prove science and develop technology in parallel, bringing faster results to an eager public of voters.

This engagement by the public and politicians in cold fusion science is an example of engagement advocated by Wilsdon and Willis (2004), Ronald Jackson (2005) and others. On this model, the public should be engaged early in the practical and ethical implications of new scientific discoveries. The Wilsdon-Willis-Jackson model was being played out exactly by Pons and Fleischmann in April 1989[18], at the urging of the University of Utah. That was until mainstream scientists put a stop to it. For many scientists, such an intrusion by the media and politicians into science's autonomy was intolerable.

It was not enough for the community to show that Pons and Fleischmann were wrong. They had to be "ritually separated from their place in the legitimate order and placed outside." (Garfinkil in Bucchi 1998 p58) Charles Taylor and Dale Sullivan explain how this separation was achieved rhetorically by asserting that Pons and Fleischmann had broken Merton's norms (Taylor 1996) and by comparing it to an exclusionary epideictic narrative as used in the New Testament (Sullivan 1994). Allan Gross describes the exclusionary rhetoric as a classic Aristotelian narrative (Gross 1995). Rhetorically, Pons and Fleischmann would be cast as different from the disinterested, ethical, sane physicists. They would be cast as delusional, fraudulent incompetents who were out for the money they could get from Congress.

The first step to exclusion came from a leading hot fusion facility in the US, the MIT Plasma Science and Fusion Center. The MIT lab had been asked by the US Department of Energy to replicate Pons and Fleischmann's findings (Mallove 1999b). On the 29th of April 1989, the director of the lab, Ronald Parker, gave an interview to Nick Tate of the *Boston Herald*. When the interview was published, Parker was reported as accusing Pons and Fleischmann of "scientific schlock" and fraud.

> Everything I've been able to track down has been bogus, and I think we owe it to the community of scientists to begin to smoke these guys out. (Quoted in Krivit and Winocur 2004 p104)

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[18] Fleischmann does not seem to have been consciously following an engagement agenda. In interviews, he claims his first preference was to classify cold fusion. His next preference was to publish an article in Electroanalytical Chemistry in 1990. He and Pons were pressured into holding the press conference by the news of Jones' work at nearby Brigham Young University. (Krivit 2003). Storms (in Richardson and Richardson 2004 p207) gives an idea of the pressure they were under: "They were getting hate mail. They were getting death threats, they were getting calls from people who wanted them to tell them what they had done so they could develop it and make millions and they were being called by physicists who said they didn't know what they were talking about and that they should go back into their cave. I mean, in the middle of the night! They couldn't get any sleep! It was just terrible."

Parker went on to say that MIT's scientific experiments had found no evidence of excess heat or fusion thus far[19].

Although MIT's rhetoric was clear, there was a problem with their science. Among the documents accidentally put on Mallove's desk was raw data from the MIT experiments. Analysis by an independent expert[20] showed that MIT had adjusted their raw data so that excess heat from their experiment was zeroed out. Mallove protested to the university. He later resigned when university management ignored his complaints[21]. He said: "These people before even analysing their calorimetry data, held a party for the death of cold fusion" (Mallove in Krivit and Winocur 2004 p104). It is ironic that this prestigious laboratory that had publicly accused Pons and Fleischmann of fraud may have been deceptive themselves.

The MIT results were important because they were cited by the Department of Energy Cold Fusion Panel report in 1989 that denied funding to cold fusion. They were also used by the US patent office to deny patents to cold fusion inventions since 1989 (Mallove 1999b). The MIT hot fusion laboratory has continued to receive its tens of millions of dollars per year for hot fusion ever since.

The next step toward exclusion was taken just a few days later at the American Physical Society (APS) spring meeting held in Baltimore on the 1st of May 1989 and it was decisive. The atmosphere at the meeting was tense. Dorothy Browner was a non-scientist at the meeting:

> This [meeting] really surprised me, because this was the first time I'd been at a professional meeting where people were…, well, very uncivilized…I did not expect to hear people calling each other names…I have been present …at many scientific meetings [and] …I had never seen that kind of behaviour before. (Quoted in Krivit and Winocur 2004 p86)

One of the first presentations was from Nathan Lewis who had gathered a large team of scientists, students and technicians to test the Pons and Fleischmann claims. Pons would not communicate with him directly so he built dozens of cells testing all kinds of variables. He

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[19] MIT's final report on cold fusion after 60 marathon days for experimentation was negative (Mallove 1999a p152).

[20] It was reviewed by Phillip Morrison "an MIT Professor Emeritus of physics who played a key role in the Manhattan Project." (Krivit and Winocur 2004 p109

[21] The president of the MIT in 1991, Charles Vest, brushed off Mallove's complaints. He was appointed in 1999 to head the Task Force on the Future of Science Programs at the Department of Energy. He recommended hundreds of millions of dollars for hot fusion. The MIT hot fusion laboratory continued to receive its tens of millions of dollars despite this apparent conflict of interest (Mallove 1999b).

gained information from newspaper articles, faxes, TV reports and electronic bulletin boards (Close 1990 p213).

Figure 15 Nathan Lewis (source: Los Alamos National Laboratory) and Steven Koonin (source: Department of Energy)

Based on his research Lewis concluded that Pons and Fleischmann had made calculation errors, mistakes in their calorimetry (measurement of excess heat) and had made the undergraduate mistake of failing to stir the heavy water electrolyte. “These problems may lead to errors large enough to cast serious doubts on the published determinations of excess heat.” (Lewis in Mallove 1999a p142) He found no evidence of excess heat or neutrons. (Close 1990 pp213-214)

The other significant presentation at Baltimore would go even further. Steven Koonin was Professor of Theoretical Physics at Caltech. He presented a number of calculations that proved that the Pons and Fleischmann results were theoretically impossible. Conventional nuclear reactions did not support the power levels Pons and Fleischmann were claiming. He concluded: “We are suffering from the incompetence and perhaps delusions of Drs. Pons and Fleischmann,”[22] (Koonin in Beaudette 2000 p74). After a moment’s pause the room erupted in a standing ovation by over 2000 scientists. What a relief. Pons and Fleischmann were just incompetent. The gathered scientists could go back to their safe old jobs, no longer threatened by an upstart science that tried to take a piece of their “$2m a day hot fusion rice bowl” (Bussard 2006). In the words of Caltech Vice Provost David Goodstein, Steven Koonin and Nathan Lewis "executed between them a perfect slam-dunk that cast cold fusion right out of the arena of mainstream science." (Quoted in Krivit 2007)

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[22] Koonin’s views of cold fusion are even more important today (2009). He has recently been appointed the Under Secretary for Science at the Department of Energy. He is now in charge of funding for new energy research projects making the proper funding of old fusion even more unlikely.

This view is backed up by John Huizenga (1992). He thinks that Pons and Fleischmann made scientific errors in the process of deluding themselves. Perhaps “the dream of limitless, clean and cheap energy [was] so powerful that it bred error and self deception.” (Huizenga 1992 p215) As Bart Simon (2002) points out, this implies that other “legitimate” scientists in hot fusion could also be making errors due to self-deception. Pons and Fleischmann must be cast as different in kind from these “proper” scientists. This casting-out of Pons and Fleishmann cannot itself be a scientific process. Even if Pons and Fleischmann are proved wrong scientifically, this does not exclude them from the scientific community. Instead casting-out is a social process that excludes non-scientists or demarcates scientists from non-scientists.

To achieve this demarcation, Douglas Morrison (1990), a physicist at the European physics centre CERN, compared cold fusion to a “pathological science”. This term was coined by Irving Langmuir (1989), a Nobel Laureate physical chemist who had made a study of sciences that ended up being discredited such as N-rays and polywater. In a 1953 lecture, Langmuir set out criteria that should alert the critic to a science being pathological. Morrison asserted that cold fusion met the criteria. The mud stuck. In September 1989, cold fusion was called as a pathological science in an influential article in the *New York Times* by N. P. Samios, Director of the Brookhaven National Laboratory and Robert P. Crease, assistant professor of philosophy, University of New York, Stony Brook (Beaudette 2000 p84). Both Huizenga (1992) and Taubes (1993) later used the “pathological science” argument in their books. Although Charles Beaudette (2000) carefully showed later that cold fusion does not meet Langmuir’s criteria, the damage was already done.

Not only were Pons and Fleischmann put in their place but so were the media. The separation of science and media was established early by the chairman Edward Redish, a physicist from the University of Maryland: “this is a scientific session; let us take questions from scientists only.” He then went further,

> I would like to address some comment to the ladies and gentlemen of the press. You are serving for us as interpreters for the general public, and it’s important to understand how aspects of the current debate illustrate the normal process of science. (Close 1990 p212)

This boundary work on the press degenerated into brow-beating by Lewis:

> Finally, if we are going to have … publication through press conferences, we should have peer review through press conferences too… [In reporting,] you should be prepared if you claim neutrons to tell me what the temperature dependence of the detector is…; about what the background signal from the room was, and what the signal was relative to the water control; what was the gamma spectrum at a wide range of energies, not only one small peak, so the world can see what the rest of the peaks look like and know whether or no the signal you were measuring was real or in the background… I called up Florida, told them they should be sure about chemical interferences being eliminated and if the reporters did that at the press conference, we might have saved oursel[ves] having to worry about that. (Lewis in Gieryn 1999 p221)

The establishment did not stop with Pons, Fleischmann and the media. Chemists were also in the firing line. Two colleagues of Pons, Cheves Walling and Jack Simons, from the University of Utah chemistry department sent Koonin a copy of a paper where they outlined a possible nuclear mechanism for cold fusion. Koonin replied “You have a real problem. These are all the right questions to ask. I don’t have any answers, and neither do you.” (Koonin in Beaudette 2000 p62)

The message was clear. Not only were Pons and Fleischmann incompetent but they also did not do normal science. Normal science is done by peer review in journals far away from the media. And if the media interfere, they are liable to be torn apart by Lewis and others. Furthermore, there will be no dialogue between physics and other branches of science. Nuclear physics owns fusion.

Chapter 4 The Emergence of Evidence

In 1989, Dr Edmund Storms was working at Los Alamos National Laboratory, the home of the Manhattan project and the hydrogen bomb. He was a radio-chemist; a specialist in the chemistry of radioactive atoms and molecules. After the original 1989 press conference, Los Alamos became a flurry of activity as dozens of groups tried to replicate the Fleischmann-Pons experiment. Storms began his cold fusion research with a search for tritium, the third isotope of hydrogen. The presence of tritium in cold fusion experiments is decisive evidence for fusion because tritium does not occur naturally unlike other fusion products such as helium. It also is radioactive making it easier to detect. (Close 1990 p305)

> In my case, a search for tritium production seemed to be the most logical approach because I was located in a building where some of the world's experts in the properties and detection of tritium worked. They know tritium when they see it. (Storms 1999)

Figure 16 Edmund Storms (Source: LENR.org used with permission)

Storms ran a number of experiments and was astounded to find tritium in a few of them. This discovery had a profound effect on him.

> [M]any people at LANL tried to make the claims work once they heard about the claims of Fleischmann and Pons. I, and a few others, succeeded, which gave us a reason to continue our studies. Once I was sure the claim was real, I knew it would have a great effect on my life as well as on society. (Storms 1999)

Following normal scientific practice, Storms experimented on 250 cells as he tried to prove himself wrong. But his results were inescapable.

> [T]he resulting paper was reviewed by 12 people at LANL and by several more reviewers after it was submitted to [peer-reviewed] *Fusion Technology*. Even though this intense scrutiny found no fatal flaws, the results were universally ignored. (Storms 1999)

This was exactly the kind of science that the critics had been demanding: carefully performed, peer reviewed studies that left scientists in no doubt that nuclear reactions were taking place. Gary Taubes, for example, a cold fusion critic and science reporter for *Science* magazine wrote of cold fusion experiments:

> Scientists can only judge the reliability of data through publication of these data in peer-reviewed scientific journals. What is needed is the reporting of data. (Taubes 1990)

Yet when such data was reported it was ignored. Or, worse still, the researchers were accused of fraud. John Bockris (2000) of Texas A&M had also found evidence of tritium and had reported those findings in the peer reviewed journal *Science*. But Gary Taubes hinted:

> Researchers familiar with Bockris's experiment … have suggested that his result were perhaps too good and too easy… Was it [the tritium found] inadvertent contamination? Or was it something more insidious? ... Suspicions were raised … that tritium in the A&M cells was put there by human hands. (Taubes 1990 p1299)

Science denied Bockris the right of reply to Taubes accusations. They were devastating for cold fusion because of the prestige and broad readership of *Science*. Bockris had to defend himself twice in internal investigations at Texas A&M. He was exonerated both times. But Taubes, instead of being criticised for his false accusations went on to write a book where he accused one of Bockris' students of deliberately spiking samples with tritium (Taubes 1993).

Figure 17 John Bockris (left) (Bockris 2009) and Gary Taubes (Credit: Kirsten Lara Getchell npr.org)

These experiences of cold fusion researchers were early examples of what was to become a pattern in the science. Serious and competent scientists would undertake exhaustive experiments, try to publish their results in a mainstream peer reviewed journal, have their paper rejected out of hand, publish in a more minor peer reviewed journal and then have their results universally ignored. All the time these researchers have had to fend of accusations of fraud and incompetence without the right of reply in mainstream journals[23].

Early on the editorial policy of influential peer reviewed journals such as *Nature* and *Science* as well as the more popular magazines *Scientific American* and *Chemical & Engineering News* was permanently biased against cold fusion. *Nature's* editor at the time, John Maddox, was a fierce cold fusion critic. In the 1991 Nova documentary, he said of cold fusion: "it's dead. And it will remain dead for a very long time." (Nova: ConFusion in a Jar 1991) Maddox published the negative findings at Caltech, MIT and other institutions but refused to publish critiques of the Caltech and MIT calorimetry by expert calorimetrist C.I. Noninsky. In August 1989, *Nature* refused to publish positive cold fusion results by Richard Oriani, professor emeritus at the University of Minnesota (in Beaudette 2000 p185ff). Maddox's mind was made up early. This attitude continued until his death in 2009. *Science* had a similar editorial policy. It also refused to publish the Noninsky critiques.

As Bucchi (1998) points out, scientists themselves use the popular and specialist press as a source of information and ligitamation. Thus the refusal of prestigious scientific journals to publish cold fusion papers at the same time deligitamated cold fusion as a science and starved science of much needed information about the progression of the field. Popular science journals are also relied on by popular journalists for information and ligitimation of sciences and scientists (Hansen 1994). Thus the editorial policies of these journals have done a thorough job of excluding cold fusion from science in the eyes of scientists and the general public.

Exclusion also is self-reinforcing. Because cold fusion was cast as a pseudo-science it has limited access to resources and publication. But because the research is not published in mainstream journals, scientists themselves regard this is a criterion for it not being a genuine science.

The exclusion was not absolute. Importantly, cold fusion papers were accepted early in the peer-reviewed *Fusion Technology* and slowly, over time, papers have been accepted in other publications. This has resulted in an impressive collection of literature confirming cold fusion. According to Jed Rothwell (2009) the lenr.org librarian, the library contains 3575 items including 1390 peer reviewed journal articles.

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[23] A similar pattern was found by Collins and Pinch (1979) in their investigations into another marginal science, para-psychology.

Some US laboratories have obtained some ongoing industry and government funding. The Electric Power Research Institute supported cold fusion research from 1989 to 1998 at SRI International, the Californian laboratory where Michael McKubre led a team that has delivered decisive results. Funding flowed on foot of a private report by three members of the DoE 1989 review panel (Krivit and Winocur 2004 p137) and another report by Garwin and Lewis. Both reports agreed that there was sufficient evidence of excess heat to continue funding. After several years work, McKubre (2007) demonstrated that the helium released by a cold fusion cell was proportional to the excess heat produced. Each atom had released about the right amount of energy that is predicted by the theory of fusion reactions. However the uncertainty of the result did not rule out other nuclear mechanisms for producing the excess heat such as weak nuclear interactions (see Appendix B). McKubre also revealed the critical 0.85 D/Pd loading ratio that must be surpassed to get positive results.

The US Navy Space and Naval Warfare Systems Center (SPAWAR) contains another funded cold fusion lab. Their team, led by Dr Frank Gordon (2009), have been researching cold fusion since 1991. One of Gordon’s team, Stan Szpak, came up with a new method to creating palladium electrodes that are heavily loaded with deuterium. His “co-deposition” method worked by electro-plating a silver or gold cathode with palladium while deuterium gas evolved there. This instantly loaded the cathode and cold fusion would begin in minutes rather than in days or weeks. Repeatability is very high with every experiment producing excess heat wherever it was measured. Their results have been reproduced in at least two other laboratories. But the SPAWAR team have not just been looking for excess heat; they were looking for evidence of nuclear reactions such as charged alpha particles (helium nuclei). They were surprised to find evidence of neutrons (Krivit, personal communication, 30 September 2009). Storms (personal communication, 13 September 2009) thinks that these neutrons are the result of fracto-fusion; nuclear reactions that take place in heavily deuterated palladium when it cracks.

Recent results (Mosier-Boss *et al* 2009) using solid-state CR39[24] particle detectors, they have achieved startling results. The picture below is the result of a high-energy neutron from a cold fusion reaction smashing into a carbon atom within the CR39 plastic. The neutron splits the carbon atom into three alpha particles (helium nuclei), which result in the three green spots in the picture.

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[24] A plastic often used in eyeglass lenses.

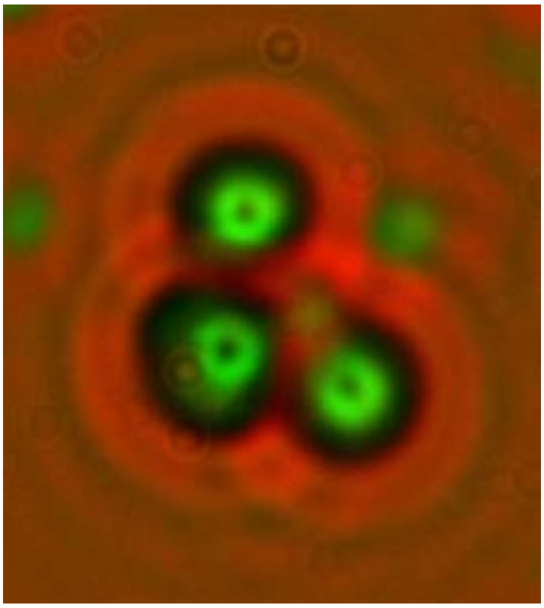

Figure 18 Enhanced photograph of CR39 after neutron bombardment from cold fusion (Mosier-Boss *et. al.* 2009)

The SPAWAR results have been published in the peer-reviewed journal *Naturwissenschaften*.

The excess heat and the high-energy nuclear particles the critics always demanded were not the only results. SPAWAR, along with 15 other laboratories around the world found evidence of transmutations (conversion of one element to another). When they analysed the cathode following a cold fusion experiment they would find other metals including aluminium and magnesium. All these results were published in peer-reviewed journals (Gordon 2009).

Japan has been more encouraging of cold fusion research than the US and some European countries. Early positive results by Japanese scientists convinced the Japanese government to fund research into cold fusion during the 1990’s. Important results by Y. Arata and Y. C. Zhang (1997) of the University of Osaka showed that a cold fusion effect could be obtained by exposing a palladium nano-particle compound to pressurised deuterium gas at room temperature. Decisively, excess heat was obtained without the need to apply an input electric current. Repeatability is now close to 100% so that Arata and Zhang were able to give a public demonstration of the effect in 2008 (Rothwell and Storms 2008)

Another study by T. Mizuno et al (2000) showed that excess heat could be obtained using tungsten electrodes (the metal used in light-bulb filaments) and input high power in an ordinary water electrolyte. Evidence of nuclear transmutations of tungsten to chlorine, calcium, titanium, iron and zinc were found.

In Italy two separate research programmes took place at ENEA, the Italian nuclear research agency. One group led by Antonella Deninno successfully tested the theories of Italian theorist Guliano Preperata (1995) (Report 41 2006). The other, led by Vittorio Violante was cooperating with McKubre's team at SRI and a commercial organisation, Energetics Technologies in Israel, to replicate electrolytic excess heat experiments.

Energetics Technologies had demonstrated up to 2500% excess heat using palladium foils prepared at ENEA in Italy (Dardik et al 2004). Energetics use a patented "super-wave" fractally-nested waveform transmitted through the input current to simultaneously load the palladium lattice, keep the current density high and provide "shocks" to the equilibrium of the system. These are now known to be requirements for a successful cold fusion result (Cravens and Letts 2008). McKubre (2008) has replicated the Energetics results.

In Russia, extensive experimentation has led to the reproducible detection of excess heat, helium, tritium, x-rays, gamma rays, neutrons, transmutations from gas loaded and light and heavy water loaded cold fusion apparatus. Electrode materials used include palladium, nickel, titanium, tungsten and stainless steel. Excess heat of up to 2000% of input power has been recorded (Goryachev and Bazhutov 2002 p5). Some of the most interesting results concern evidence of transmutations in certain kinds of bacteria. These bacteria may have applications in the processing of radioactive waste into safer materials. (Goryachev and Bazhutov 2002)

In India, early positive results led to a cold fusion programme in the early 1990's at the Bhabha Atomic Research Centre (BARC) led by director Padmanabha Krishnagopala Iyengar. It was then abandoned when Iyengar left BARC. In 2008 *Nature India* reported that BARC is reviving its cold fusion research efforts with Iyengar remarking: "We did great injustice to the country by stopping the research that was going on at the Bhabha Atomic Research Centre" but "it is not to late to revive it." (Iyengar in Jayaraman 2008)

Despite all the evidence of successful replications of cold fusion experiments, scepticism still remains. The critics' biggest objection is that cold fusion is not replicable. Take Richard Garwin in the recent 60 minutes mini-documentary on cold fusion:

> I require that you be able to make one of these things, replicate it, put it here. It heats up the cup of tea. I'll drink the tea. Then you make me another cup of tea. And I'll drink that too. (More Than Junk Science 2009)

Garwin requires the effect to be 100% replicable before he will believe it's real. The Arata and Zhang (1997) experiments with deuterium gas, for example, are 100% reproducible. Moreover the SPAWAR (Mosier-Boss et al. 2009) results are 100% reproducible and have been replicated several times.

What does "replicable" mean in the cold fusion case? Again consider the Caltech experiments by Nathan Lewis. According to Lewis and many critics, the Caltech experiments do count as replications of the Pons and Fleischmann experiments. Moreover, as replications, they disconfirm Pons and Fleischmann's results. And this disconfirmation is sufficient to consign cold fusion to the dustbin forever. As cold fusion critic David Goodstein put it:

> Popper argues that a scientific idea can never be proven true, because no matter how 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. (Goodstein 2000 p63)

The Caltech work counted as such a contrary experiment. The "Popperian rigor" (Goodstein 2000 p68) displayed by Lewis was missing from the experiments by cold fusion proponents for they discounted the unsuccessful experiments that they themselves performed. Goodstein's Popper, however, is quite different from the original Popper who wrote:

> We shall take [a theory] as being falsified only if we discover a *reproducible effect* which refutes the theory. (Popper 1959 p 86 italics original)

So actually, the falsification of cold fusion requires reproducible experiments that are themselves replications of the original Pons-Fleischmann experiments. It gets worse for Goodstein. Was the Caltech experiment a true replication of the Pons-Fleischmann experiment? According to the cold fusion community, Lewis failed to load his palladium samples beyond the requisite 0.85 loading ratio (Cravens and Letts 2008). So the Caltech experiments were not replications and therefore do not count as falsification for the cold fusion hypothesis.

Harry Collins (1992) argues that replications have increasingly confirmatory power as they begin to differ from the original experiment but only up to a limit. For example, a repetition performed by Pons and Fleischmann in their basement laboratory, does not count as a confirming replication. But Michael McKubre's experiments at SRI international are confirming because they were performed by a different experimenter at a different lab using different apparatus. The SPAWAR experiments had even more confirming power because the cells were loaded in a different way from the original Pons-Fleischmann cells. The confirmatory power of Arata and

Zhang's experiments were even stronger because they used deuterium in a different form, loaded it using a different method and used palladium in a different form. However, the experiment cannot be too different. If a soothsayer confirms cold fusion by examining the entrails of a goat, for example, this has no confirming power at all (Collins 1992).

Similarly, experimenters should be independent enough of the original team to be "objective" and not be influenced by them. Experimenters should also be sufficiently competent and trained to perform the experiment correctly. Thus the Caltech and MIT experimenters are independent and even sceptical of the Pons-Fleischmann claims but are they sufficiently competent? They have agued that their experiments were better performed than Pons and Fleischmann's (Mallove 1999a p142). But the lack of basic information about the apparatus and its configuration meant the teams were guessing at some parameters and trying to estimate others from the television screen. Few experimenters had sufficient training in the intricacies of calorimetry or electrochemistry. Those that had, like McKubre, were more successful.

Of more importance, perhaps, is the reputation of the experimenter. Disconfirmations from Caltech, MIT and Harwell were taken more seriously than confirmations from Texas A&M, SRI International, ENEA in Italy and BARC in India. Some MIT undergraduates performed the first cold fusion "disconfirmation" on the evening of the 23rd of March 1989 press conference (Nova Confusion in a Jar 1991). In 2003 high school students at MIT successfully performed replications during a summer programme (ICCF10 2009). Should the undergraduates' experiments be more persuasive than the high school students' because they have a bit more status? Or should the high school students' training with John Dash, a cold fusion proponent, be more important than undergraduate science training and information gleaned from a news item? Of course the results of properly trained experimenters should have more weight. Yet in practice, the reputations of the disconfirming laboratories continue to hold more sway in the scientific and non-scientific media, governments and the US patent office.

How could one community - the mainstream scientists - regard the Caltech experiments as a replication and the cold fusion community regard them as incorrectly performed experiments? This question is a case study of what Collins (1985) calls the "experimenters' regress". Collins asks: how do scientists tell the difference between a replication that is correctly performed but has a null outcome, and an experiment that is incorrectly performed? If we say "a correct replication of cold fusion is an experiment that produces excess heat" then this begs the question as to whether cold fusion is real. But how do we tell whether cold fusion is real? We need to replicate the original experiment and detect excess heat; and so on. Collins says:

> Experimental work only counts as a *test* if some way is found to break the circle…the experimenters' regress can only be avoided by finding some... means of defining the quality of an experiment...which is independent of output of the experiment itself. (Collins 1992 p84, italics Collins')

In the absence of clear technical criteria for a quality experiment in 1989, social criteria were used such as the reputation of the experimenter, their affiliation or membership of, for example, the physics community or the sceptical community. But now Cravens and Letts (2008) have listed clear technical criteria that the Caltech and MIT experiments did not meet. Yet these experiments are still used by the critics as evidence for the non-reality of cold fusion (Park in Krivit 2004 p147).

Collins argues that this is because even technical criteria are at bottom social:

> The model, which seems most appropriate is one that involves the transmission of a *culture* which legitimates and limits the parameters requiring control in the experimental situation. (Collins 1975 p207 italics original)

But cold fusion critics exclude themselves from this culture. Robert Park refused to even touch a cold fusion paper offered to him by Jed Rothwell and let it fall to the ground (Rothwell in Krivit and Winocur 2004 p145). In a conversation with Krivit in 2003 Park knew who the "experts" were in cold fusion: "Steve Koonin at Caltech … and …Nathan Lewis"; but admitted "I haven't gone through that [cold fusion literature] in so long, I don't know what to recommend." (Park in Krivit 2004 p147)

Although many agree with Collins' that replication is a socially governed activity, they disagree with the strong relativism that follows from claiming that even technical criteria are ultimately social. In Collins' relativism, there is no epistemic difference between the social (and political) discourse among scientists and scientific facts. Facts are relative to the social community. Larry Laudan (1982) thinks that this relativism is inconsistent with the empirical method Collins uses for his case studies and for Michael Mulkay *et. al.* (1983), Collins' relativism undermines the distinction between the social and factual aspects of science upon which Collins himself relies[25].

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[25] I don't need Collins' relativism for my argument, the experimenters' regress is sufficient. But if I accept the regress, does the relativism follow? For Collins, the realist claim that technical parameters must agree with nature rather than just the community begs the question as to how the experimenters' regress is broken in practice; we need to know what counts as

Despite philosophical criticism, some scientists agree with Collins. Chemists Jay A. Labinger and Stephen J. Weininger (2005) think that Collins' experimenters' regress cannot be dismissed out of hand in the cold fusion case:

> It is easy, but much too simplistic, to invoke irrationality to explain this persistence of [the cold fusion] heterodoxy... [I]t can sometimes be very difficult to prove its non-existence. (Labinger and Weininger 2005 p1921)

Trevor Pinch (1994) agrees. He thinks that "experiments are never alone capable of settling a controversy, other factors will always have to be brought in to break the regress." (Pinch 1994 p93) Bart Simon (2002) describes what those "other factors" are:

> The cumulative mass of experimental reports alone was not enough to dissociate cold fusion. What was needed was the mutual interaction of scientists at the APS meeting…The APS meeting produced not a definitive refutation of the cold fusion claims but an atmosphere of sceptical solidarity…uncharitable interpretations of the experimental data supporting cold fusion were thus made publicly acceptable. (Simon 2002 p71)

The consequence of these "uncharitable interpretations" was to exclude cold fusion researchers from mainstream science. This means that it doesn't matter how many replications are performed by cold fusion researchers. They are all excluded because, in the minds of critics, cold fusion researchers' experiments are no more convincing than the soothsayer's examination of a goat's entrails.

Thus the critics have three options. They can assert that the experimenters' regress has still not been broken scientifically; there still are no technical criteria that can be used to determine

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"agreeing with nature" in practical cases. Cravens and Letts (2008) have shown us what counts in the cold fusion case. Just because technical parameters are agreed by the community does not make them any less facts or less scientific. Any parameters that did not agree with nature (i.e. did not result in a positive experiment at least some of the time) would be rejected by the community. To argue that there is no difference in principle between the technical parameters and social agreements leads us into a quagmire of logical difficulties that Laudan and Mulkay et al have begun to point out.

There is no difficulty in assuming that the experimenters' regress applies in new scientific discovery until technical criteria are discovered that can be used by any sufficiently trained scientist to reproduce the experiment. These criteria can be called facts. They are facts because they fulfil a different role in the discourse of scientists than, for example, a social agreement of whether questions should be taken during a seminar or at the end. This difference means that Collins strong relativism need not follow from the experimenters' regress.

I have tried to avoid, as much as possible, a philosophical discussion on relativism. Thus I sidestep the question here: although the technical parameters fulfill a different role in the discourse, are they epistemically different? To answer this question would take another dissertation. Furthermore, the difference between facts and social conventions is treated in excruciating detail in Wittgenstein's *Philosophical Investigations* (2003) in a way that avoids relativism. I'm surprised that Collins does not understand this distinction since he acknowledges Wittgenstein as being the father of the sociology of scientific knowledge.

whether an experiment is correctly performed with a null result or incorrectly performed. In this case the Caltech and MIT results could still be counted as replications because the Cravens and Letts (2008) criteria do not exist. Then the problem is that the positive results must be excluded for *social* reasons rather than scientific ones. Thus cold fusion must continue to be called a discredited pseudo-science whose researchers are “true believers”. If cold fusion results are excluded for social reasons, then social arguments can be used to include the results once again.

The second option for the critics is to concede that the experimenters’ regress is partially broken in that some technical criteria have been found to determine whether an experiment is correctly performed, but there is not an exhaustive list. Because there is no exhaustive “recipe” that can be used by anyone to obtain a positive result, cold fusion is not real. There still are correctly performed experiments with null results. This is an untenable position. By conceding that there are technical criteria for success, the critic has conceded that cold fusion is real and it is simply a technical matter to find all the criteria for a successful experiment. Thus it is essential for the critic that *all* positive results are incorrectly performed or fraudulent experiments and that the technical criteria have not be identified.

The critics’ third option is to assert that the experimenters regress has been trivially broken by claiming that all cold fusion experiments are incorrectly performed or fraudulent in fact. They can insist that correctly performed experiments done by non-delusional experimenters would result in null findings. Now the critics have two problems. They need to show by what technical criteria they would judge an experiment to be correctly performed and they need to show every experiment ever performed fails those criteria (there could be different criteria for each experiment). This becomes more and more difficult and less and less plausible as each new method of excess heat production is demonstrated and replicated. Even in 1991 Mallove wrote:

> There is no chance that cold fusion is <u>a</u> mistake. There is an exceedingly remote that “cold fusion” is a collection of <u>many</u> mistakes made in nuclear measurements of many different kinds, in heat measurements of a great variety, and in all manner of control experiments. (Mallove 1999a p(x) emphasis original)

Each time critics point to a criterion, it is easily refuted based on the accumulated evidence. For example, Garwin claims that the input power in electrolysis experiments is measured wrongly. The Arata and Zhang (1997) gas loading experiments require no input power so it’s impossible to measure it wrongly.

Finally, critics could claim that all cold fusion researchers are delusional. If the claim is a factual one, then it is a bold one. Cold fusion researchers include Nobel Laureates, former critics and some of the most respected scientists in their fields. If the claim is that, by definition, one has to be delusional to work on cold fusion then by the Popperian positivism popular among critics, it is not a scientific claim because it can’t be proven wrong. It merely reasserts that cold fusion researchers are not part of the mainstream scientific community.

The conclusion of all these arguments is that the critics’ early experimental results are susceptible to the experimenters’ regress and that the failure to achieve 100% replication does not support the idea that cold fusion as simply experimental error. Otherwise the critics must furnish the technical criteria that *all* 3000 published positive cold fusion experiments fail to abide by, to claim that they all are incorrectly performed. This is a very difficult task that has not been attempted by the critics since 1994 (Labinger and Weininger 2005). It is becoming less and less plausible that such criteria can be found and the criteria that are sometimes mentioned like input power mis-measurement are easily dismissed.

However, the claims that all positive experiments must be incorrectly performed for theoretical reasons or based on 60 years of experience in fusion physics is not so easy to dismiss. We turn to these claims in the next chapter.

Chapter 5 Theory before Experiment

> I was in the arrival lounge at … J.F. Kennedy airport in New York …It was around 2pm Eastern Standard Time, on Thursday the 23rd of March…I had heard only verbal details in the Kennedy airport baggage hall and dismissed it as nonsense. Releasing millions of electron volts from a car-battery made no sense at all…and I thought it was an April fool's day story run a week early. (Close 1990 pp121-123)

Frank Close was not alone in his reaction to the Pons and Fleischmann announcement. Cold fusion just didn't make sense according to all the training and experience of nuclear physicists. But it was not just that the announcement was hard to understand in view of the lack of details released by Pons and Fleischmann. If "the palladium induced deuterium nuclei to fuse without the encouragement of millions of degrees of input, as advocated by hot fusion researchers,…we physicists were going to look rather foolish" (Close 1990 p126)

As details emerged about the Pons and Fleischmann experiments, it became clear that the duo had used two methods for the detection of a fusion reaction. Calorimetry was the main method. Pons and Fleischmann had decades of experience with heat measurement on electrolytic cells. The other method was neutron detection. Neutrons are the signature particle of nuclear reactions. If fusion reactions were taking place, there should be a significant number of reactions where high-energy neutrons are ejected. If the experiment were surrounded in a light water bath, some of the neutrons would be captured by the water emitting a gamma ray photon that could be detected with instruments (Mallove 1999a p131ff).

Pons and Fleischmann had very little experience in detecting neutrons. It is a process fraught with pitfalls. Everywhere on earth there is a significant background flux of neutrons from the sun and other stars as well as natural radioactive materials on earth. It's easy to mistake these background neutrons for a low-level signal.

Many physicists are expert in the detection of neutrons. But most of these experts do not understand the intricacies of calorimetry[26]. Thus the physicists mainly relied on the neutron measurements as the essential evidence for fusion. But in 1989 there was a problem. Pons and Fleischmann had made mistakes in their original experiments. Their gamma rays had the wrong

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[26] Lewis said "Heat was confusing to nearly everyone, including all my electrochemical friends." (Quoted in Krivit and Winocur 2004 p98). Also see Pons and Fleischmann's response Morrison's critique of their 1994 paper (Fleischmann and Pons 1994). Morrison did not reply. One exception is Robert Duncan, Vice Chancellor of Research at the University of Missouri. He examines the cold fusion claims in Appendix A.

energy to be due to neutron collisions with the light water bath. They also lacked the characteristic “Compton edge” due to the scattering of some neutrons (Close 1990 p98, p151).

But it got worse. According to accepted fusion theory there should be billions of times more neutrons than were detected by Pons and Fleischmann to account for the large amount of excess heat they measured. The expected neutron flux should be large enough to kill Pons and Fleischmann within hours of exposure.

These conclusions were drawn from the theory of high energy nuclear reactions in the vacuum that had been proven over and over again in experiments for over 60 years. Experiments in particle accelerators and other apparatus have showed quantum mechanics to be the most successful theory of all time in science (successful means that theory very accurately predicts experimental findings) (Feynman 1985). Quantum mechanics is also proven in modern technologies from computers to genetic engineering, from superconductors to nano-technology. It seemed almost impossible that such a successful theory could be wrong in the crystal lattice of palladium. Furthermore, Pons and Fleischmann had no alternative theory that would explain the lack of neutrons. Instead they weakly hypothesised about an “unknown nuclear process”.

Neutrons are now routinely detected in cold fusion experiments such as Mosier-Boss *et al* (2009). But they continue to be billions of time less that theory predicts. Edmund Storms thinks that that the cold fusion effect generally does not produce neutrons. Those detected by experimenters can be explained by fracto-fusion, small scale fusion that occurs when “solid materials containing deuterium are cracked or broken” (Storms, private communication, 13 September 2009). But I do not want to focus on the scientific evidence. Instead, I’ll consider two philosophical objections.

The critics are fond of Karl Popper’s philosophy of science (e.g. Goodstein 2000) and of positivism in general. Positivism is a popular philosophy of science among physicists. For example, Stephen Hawking (2001; 1998) writes passages remarkably similar to Goodstein:

> [Y]ou can disprove a theory by finding even a single observation that disagrees with the predictions of theory. (Hawking 1998 p11)
>
> Any sound scientific theory…should …be based on the most workable philosophy of science: the positivist approach put forward by Karl Popper and others. (Hawking 2001 p31)

Notwithstanding that the physicists' Popper is not the same as the original (see chapter 4) and that there are valid philosophical objections to Popper (e.g. Kuhn 1970), it seems appropriate to apply his philosophy to the theoretical objections of the critics. For Popper, "a system is…scientific only if it makes assertions which may clash with observations." (Popper 1963 p256 c.f. Hawking 1988 p11) So the theory of nuclear physics is scientific only if it allows falsification. This is exactly what the cold fusion findings do; they falsify traditional nuclear physics (Bockris in Fire from Water 1999). To not allow these observations to clash with the theory is to render the theory of nuclear physics unscientific.

The critics' only recourse is to separate off cold fusion observations from other scientific observations by the designation of cold fusion as a pseudo-science. Pseudo-scientific observations cannot clash with theory; theory wins because the observations are not scientific.

The second philosophical problem is that physicists simply assert that the theory of nuclear interactions that governs physics in a vacuum also governs the vastly different environment of the crystal lattice. They have no evidence to support this assertion. But there is evidence of differences in, for example, electronics. Physicists agree that the vacuum tube triode broadly works similarly to the solid-state transistor. But the details of how electrons flow in a vacuum verses a lattice are vastly different. Early transistors lacked a theory of how they worked because they did not seem to work in the same way as triodes and because the crystal lattice is different from the vacuum (Rothwell 1999).

Similarly, the theory of nuclear physics in a vacuum might just not apply in the lattice. As Nobel Laureate[27] and cold fusion proponent Julian Schwinger simply put it: "The circumstances of cold fusion are not those of hot fusion." (Schwinger 1991 p3)

The unwillingness of nuclear physicists to allow that their findings from the vacuum may only strictly apply in the vacuum is another example of not allowing the observations to clash constructively with the theory. Thus the same argument applies as above: nuclear physics is in danger of losing its scientific status if it ignores such clashes.

On the other hand, Fleischmann (2000) thinks that the existence of excess heat in cold fusion experiments is an "anomaly…that nature has somehow violated the paradigm-induced

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[27] Julian Schwinger (1918 - 1994) developed the theoretical basis for quantum electrodynamics, for which he shared the 1965 Nobel Prize in Physics with Richard Feynman and Shin'ichiro Tomonaga.

expectations that govern normal science" (Kuhn 1970 p52-3). Fleischmann's original aim was to create an experiment that would cause the Kuhnian overthrow of the quantum mechanical paradigm and replace it with quantum electrodynamics (Fleischmann 2003). However Kuhn also points out, "Once it has achieved the status of a paradigm, a scientific theory is declared invalid only if an alternate candidate is available to take its place." (Kuhn 1970 p77) But no suitable candidate currently exists. Even experts in QED such as Schwinger (1991) and Preparata (1995) have not been able to employ it to fully explain cold fusion satisfactorily.

More precisely there is not a lack of theories but rather too many competing theories, all of which claim to be able to explain the experimental findings (McKubre in Fire from Water 1999). There are three main groups of theories. The first is the traditional phononic-coherence theories of Fleischmann (2003), Preparata (1995), Hagelstein *et. al.* (2008) and Chubb (2008). The second group is the deuteron shielding and tunnelling theories of Chen and Li (2002), Hora *et al* (2002) and Kasagi (2008). The third theory is the weak interaction theory of Widom and Larsen (2006). Appendix B has more details on the theories.

There is more than disagreement about theory in the cold fusion community, there is a growing schism. In a recent posting on the CBS website Lewis Larsen wrote:

> Mr. Rothwell happens to be a member of a long-standing group of hard-core, diehard LENR [Low Energy Nuclear Reactions] researchers and assorted camp followers like Rothwell who comprise --- for lack of a better description --- what I call the "cold fusion cabal"…What distinguishes the 'cabal' subgroup from other active participants in the field of LENRs is their undying adherence to an ill-founded belief that some sort of fusion process ("cold" D-D fusion in particular) is the underlying mechanism that is responsible for various types of anomalous phenomena that have been repeatedly observed experimentally for more than 20 years… What is counterproductive about the activities of this very vocal, highly visible group of people is that for many years they have actively ignored and even tried to suppress any experimental data or theoretical work that directly contradicts their cherished "cold fusion" conceptual paradigm. (More than Junk Science (comments) 2009)

The traditional cold fusion community is supported by Jed Rothwell who is the librarian at the lenr-canr.org website that publishes many cold fusion academic papers and some press clippings. This community also publishes *Infinite Energy* magazine via its editor Scott Chubb who is a long time coherence theorist. In the other camp are the weak interaction supporters who think the "cold fusion" effect is real but that it's not fusion. Their cheerleader is the founder of the New Energy Institute, Steven Krivit. He edits and publishes the *New Energy Times* online magazine, a blog and an extensive Widom-Larsen theory portal. Some people have interpreted

Krivit's opinion that cold fusion is not fusion to mean that he is a cold fusion sceptic, that is, he thinks that excess heat is not real. On the contrary, Krivit is a strong advocate of the excess heat findings. He just thinks that they are due to weak nuclear interactions rather than the fusion of deuterons ("Cold Fusion" Rebirth 2009).

The theoretical question is far from settled, but in one way Larsen is right. The lack of a plausible, testable theory in cold fusion has held the discipline back and has undermined the development of practical devices. The more quickly cold fusion can be rehabilitated into a mainstream science, the more quickly the rhythms of normal scientific theorising and testing can resume. Perhaps then, a decisive experiment can be designed to differentiate among the theories. At the moment, though, the emotions that have caused havoc in the past seem destined to continue causing problems within the cold fusion community.

As for mainstream physics, it is at a theoretical impasse. If critics disavow cold fusion observations, they run the risk of making nuclear physics unscientific according to their own version of Popperian positivism. Otherwise they must argue that the observations are pseudo-science and such an argument is not *itself* a scientific act (proving my thesis). To accept cold fusion means a paradigm shift is required (at least according to traditional D-D fusion theorists). And there is no generally agreed alternative theory waiting in the wings to replace current nuclear physics.

Cold fusion calls for a more radical shift than a reworking of electroweak theory by Widom and Larsen or a transition from the standard model of quantum mechanics to QED. There are many other anomalies in, for example, the foundations of quantum mechanics (Smolin 2006) and zero point energy (Mallove 2001) that Widom and Larsen or QED cannot explain away[28]. However, there has not been a long enough "period of pronounced professional insecurity" (Kuhn 1970 p67) to create the full blown crisis in physics required to force change.

This is not to argue that the Widom-Larsen theory is necessarily wrong because it accords with the existing paradigm. But rather, Widom and Larsen are not necessarily right because of that agreement either. On the other hand, the deuterium fusion theorists should not assume that science necessarily proceeds according to the Kuhnian philosophy. Science may not progress

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[28] For example Alan Aspect showed that Bell's inequality is violated. It follows that quantum mechanics requires some form of non-locality to be consistent and this is inconsistent with Einstein's ruling out of faster than light signaling. Also see Smolin 2006.

by orderly, rational extensions to existing theory. But it might also not progress by paradigm shifts due to non-rational factors either as Kuhn thinks (Kuhn quoted in Deutsch 1997 p324).

Larry Laudan (1977) argues that science progresses neither by rational progression nor by irrational paradigm change. Science progresses by solving problems. One of the biggest problems we now face is dwindling energy supplies that threaten our planet. A theory that leads to a practical cold fusion device will have immense problem solving power. Practical cold fusion devices would also change the existing means of production of all human goods and services. For Karl Marx:

> In acquiring new productive forces men change their mode of production; and in changing their mode of production, in changing the way of earning their living, they change all their social relations. (Marx 1934 p119)

Thus to whatever philosophy of science one subscribes, practical cold fusion devices will progress science. If one were a Kuhnian, they would force a crisis on reluctant physicists; if one follows Laudan, practical devices will extend the meaning of “rational” to include the mechanism by which the devices operate; and for Marxists, practical devices will radically change the means of production.

“Practical” can be a misleading word: practical for whom, practical for what? Most cold fusion researchers envisage cold fusion powered water heaters or even electric generators that are small, inexpensive and available for anyone to buy at their local store (Rothwell 2004). They see the lack of dangerous neutrons and radiation as an advantage that cold fusion has over hot fusion. Heat and electricity can be created locally and controlled by individuals rather than created centrally and controlled by corporations and governments. This would result in the democratisation of energy generation in the same way that personal computers democratised computing and the car democratised transport. As predicted by Marx, such a democratisation would result in massive changes in society. In particular it would result in large corporations and governments losing control of the energy supply chain. Since energy is a component of all production and consumption, I’d expect corporations and governments to resist such a large loss of control.

It also seems important that devices are manufactured and marketed by companies in large volumes. There needs to be a large variety of devices to meeting differing applications and power requirements. In short: there needs to be a cold fusion market. Cold fusion will not deliver on its promise if it remains in the laboratory or available to only the select few who understand the science and technology to make a device. Ironically, the denial of cold fusion patents in the US means that no one entity controls the technology. But I also suspect that the technology is more advanced than is advertised in the literature precisely because scientists cannot patent their inventions in the US.

Thus the practical cold fusion device is a political, social and economic concept as much as a technological one. The creation of a practical device will be as much of a political struggle as the recognition of the science has been. But the promise of cold fusion should be enough motivation to try.

Chapter 6 The Future

“I think unless we get fusion to work in some fashion we are doomed, aren't we?" said Martin Fleischmann in a recent interview with *New Scientist* magazine (Cartwright 2009). Focus fusion proponent Eric Lerner echoes his views:

> The only way [all] people can be brought up to the standard of living of the industrialised country is by greatly increasing global per capita energy. To bring it up to Western European standards you have to about triple it. (Lerner 2007)

Current world power use is 16TW[29] and it would have to grow to 60TW for everyone to enjoy western European standards of living[30]. But where can the world get the additional 44TW of power required? Lerner estimates that to achieve the required additional power in 20 years, world energy production would have to grow by 6.8% per annum with a required investment of $2.2trillion per year. This is about 4.4 times the current investment in all forms of energy production: fossil, wind, solar, etc. We are approaching an energy crunch and unless a new cheap source of energy is found, the majority of the world’s people will lose their chance at a decent standard of living.

Fortunately Lerner has a solution: nuclear fusion. He’s not talking about cold fusion. He’s talking about focus fusion. Focus fusion is promising because instead of trying to control the instabilities that naturally occur in hot plasma as tokomak designs do, focus fusion harnesses the instabilities to confine the plasma for a fraction of a second. Computer models show that for a few hundred million dollars, a focus fusion power plant the size of a small lorry could be built. This plant would be safe, clean simple and small. It would be cheaper and more likely to produce net energy gain than the $20B ITER tokomak reactor (Lerner 2007).

If this sounds familiar it’s because the focus fusion story is a not-so-public mirror of cold fusion. Focus fusion was never discredited but it never needed to be. The DoE could continue to ignore it and starve it of funding and it would be no threat to the tokomak juggernaut. The parallel experience of focus fusion does prove that there is something systematically wrong with how physics is funded in the US.

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[29] A Terawatt is a trillion watts of power. A typical household electric kettle uses 3kW or 3000 watts when running. By comparison my one litre Nissan Micra car develops a maximum of 40kW of power.

[30] I sidestep the question as to whether people should use the same amount of energy as a western European. But I concede that the concept “standard of living” is ideologically loaded.

Hot tokomak fusion, inertial confinement fusion, cold fusion, focus fusion, electrostatic confinement fusion, and various types of zero point energy (ZPE)[31] are all possible future sources of energy. But only two of these get substantial funding: tokomak and inertial confinement fusions.

Investing in only two possible future energy sources seems incredibly short sighted on the part of the US government and other governments. If the US government used the proven investment model of the venture capitalist, it would invest in all these technologies with confidence that at least one of them will prove practical (Smolin 2006;Taleb 2007). It seems ironic that the cheapest technologies are precisely those that are starved of funding by the authorities. This leads to the unfortunate impression that physicists advising the US government on energy research prefer research that keeps vast armies of their colleagues in jobs for life, even if they're working on projects that have absorbed tens of billions with little to show for the money. By comparison, it is estimated that focus fusion or cold fusion could be commercialised for as little as $200 - 500M (Rothwell in More than Junk Science (comments) 2009). It is better explained by realising that politicians often invest only in the technique or technology that has the best public reputation, in the fear of appearing to waste money on unpromising approaches. Such “beauty parade” investments are regularly swamped by unexpected breakthroughs that come out of the blue. (“Black swans” Taleb 2007)

The Obama administration continues these mistakes of the past. Secretary Chu has announced that $3.4B will be spent on researching carbon capture and storage (CCS) technology (America's Power 2009). CCS technology may solve part of the global warming problem, but even with exorbitant investment, coal, wind, solar and biomass will not be able to generate the energy needed by a world with growing aspirations. The world must invest in alternative energy technologies.

Political, economic, social and psychological factors have dominated practical considerations in cold fusion. This is in part due to the mistakes made by Pons and Fleischmann and the University of Utah. If they had not rushed to hold a press conference, perhaps the outcome would have been different. If they had performed their neutron measurements more carefully, perhaps they could have convinced more physicists.

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[31] ZPE devices extract energy from the vacuum

On the other hand, I've argued that the neglect of cold fusion research is mostly due to the deliberate efforts led by the high-energy physics community to exclude cold fusion from science and cold fusion researchers from the scientific community. This demarcation has been four pronged. First, the critics discredited cold fusion and its researchers publicly with *ad hominem* attacks and doubts about Pons and Fleischmann's competence and sanity. The critics backed up these attacks with dubious experiments, lies, misrepresentation and deception. Prong two was to undermine funding for cold fusion research using the DoE report and to ensure that those scientists concerned with their reputations, would steer clear of cold fusion research. The third prong was to reject patents for cold fusion inventions at the US patent office. The final prong was to exclude cold fusion papers from the majority of mainstream peer-reviewed journals[32].

We now also know why cold fusion needed to be excluded. The budget for hot fusion is just too important for physicists to lose. The power and prestige of being masters of the nucleus was too to satisfying to forfeit. The comfort of having an unchallenged theory of nuclear physics was too much to give up. Prominent physicists tied themselves in knots privately contradicting their public statements. Although their behaviour can be explained by the limitations of physics training, their political actions should serve as a warning to, for example, science journalists who routinely defer to scientific expertise. The evidence of the cold fusion case shows that science is as political as any human endeavour and that scientists will often disguise then political pronouncements as scientific arguments. By the same token, scientists could become more consciously politically savvy.

Cold fusion is a recent example of how scientists demarcate science from non-science in practice. I have deliberately avoided a philosophical discussion of demarcation but I'd now like to make a few remarks. The history of the philosophy of science can be characterised by the ongoing attempts to demarcate science from non-science (Laudan 1983). Normally philosophers try to list necessary and sufficient conditions but Laudan (1983) has shown that all historical attempts create an adequate list have failed. Echoes of these failed attempts show up in the critics' reasons. Philosophers in the past have tried to demarcate science using induction, replication, consistency with observations from other parts of science, consistency with theory, and results found only by qualified, sane and reputable scientists. These attempts have all failed due to counter examples that show important parts of accepted science have been arrived at by

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[32] These findings mirror the tactics used by critics of Parapsychology in an analysis of its exclusion from mainstream science by Collins and Pinch (1979).

breaking the demarcation rules and that nonsense has been perpetuated by obeying these rules. This is not to say that science cannot be demarcated; the Cravens and Letts (2008) criteria show how cold fusion as a science can be demarcated from various kinds of non-science. But the criteria for demarcating science seem to be as diverse and numerous as the sciences are themselves. The combination of social and technical factors required to break the experimenters' regress means the search for universal criteria for demarcating science in general is as vain as Laudan describes.

It's important that the general scientific community and the public recognise the shabby demarcation practices of leading cold fusion critics. The conservatism of the physics community should be resisted. This community has too much political and economic power in the US. This has led to the misallocation of resources to the detriment of the poor and humanity in general. Money should be allocated to cold fusion research in an effort to build practical devices bearing in mind that what is "practical" is also a political question. Even if this research proves fruitless, the price tag will be a fraction of that already spent on hot tokomak fusion. But if the research is successful, $500M will be the biggest bargain in history.

I am confident that despite the theoretical confusion surrounding cold fusion, practical devices will be available within 30 years. I also predict that these practical devices will drag physics into the 21st century. Consider the history of the steam engine. Practical working Newcomen atmospheric steam engines were used in Cornish mines in the early 18th century (Science Museum 2009). This was long before the Watt engine, the theories of thermodynamics and statistical mechanics. The Newcomen engines were highly inefficient but they worked. This practical device not only forced science to develop the theory of thermodynamics but it also changed the means of production leading to the industrial revolution. The same thing will happen with cold fusion.

Another lesson can be learned from the history of the transistor. Early transistors were highly unstable and unreliable. A slammed laboratory door was enough to stop one from working. Scientists did not know how they worked but their value as electronic amplifiers was proven early[33]. Decades of research in materials and theory led to perhaps the most useful invention of the 20th century (Rothwell 1999). The invention of the transistor led to the computer, Internet,

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[33] Wolfgang Pauli, a founder of quantum mechanics, who did much to create the semiconductor wrote "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." Quoted in Beaudette 2002 p180

communication and electronic revolution of the second half of the 20th century. Fusion, in whichever practical guise, will do the same for the 21st century.

Cold fusion’s rejection will be historically regarded as one of science’s big mistakes. But there are lessons to be learned outside of science. The public and politicians can and should be more involved in decision-making in the allocation of resources to science. To support this participation, science journalists must stop deferring to scientists and adopt the critical attitude that journalists use successfully in reporting politics, business, crime and society. It also raises an interesting corollary: how many other sciences are languishing unfunded or are being discredited by politically motivated scientific communities? Let us hope that there are few and that the treatment of cold fusion is the exception rather than the rule.

Appendix A Evidence for cold fusion by independent scientists

These independent scientists are chosen because they are not part of the cold fusion community and they have a lot to lose socially and politically by coming out in support of it. Two, Heinz Gerischer and Robert Park were formerly ardent cold fusion critics and Robert Duncan is representative of many neutral mainstream scientists.

These observers are not die hard critics like Richard Garwin, Nathan Lewis and John Huizenga who have too much to lose if cold fusion is real. Nor are they “true believers” like Martin Fleischmann, Michael McKubre, Edmund Storms, John Bockris and Yoshiaki Arata. The true believers’ reputations have already been destroyed in the eyes of many mainstream scientists, so they do not have enough to lose.

One of the first high profile sceptical converts was Heinz Gerischer who is “widely recognized to be the leading physical electrochemist in Europe and would vie for the title on a still wider basis... [He] was, until 1988, the Director of the Max Planck Institute for Physical Chemistry in Berlin.” (Bockris in Gerischer 1991) Heinz wrote:

> In spite of my earlier conclusion, - and that of the majority of scientists, - that the phenomena reported by Fleischmann and Pons in 1989 depended either on measurement errors or were of chemical origin, there is now undoubtedly overwhelming indications that nuclear processes take place in the metal alloys. (Gerischer 1991)

Gerischer was convinced after attending the second International Conference on Cold Fusion (ICCF2) in Como, Italy. Unlike many mainstream scientists, Gerischer had taken the time to read the published (and unpublished) literature on cold fusion.

Robert L. Park was once one of cold fusion’s biggest critics. He is an emeritus professor of physics at the University of Maryland, College Park and was Executive Director of the American Physical Society (APS) in 1989. He devoted the first chapter of his book *Voodoo Science* (Park 2000) to portraying cold fusion as discredited, false, delusional or fraudulent “voodoo” science. He likened cold fusion researchers to the fictional FBI agent Fox Mulder on the TV drama *The X files*: wanting to believe in cold fusion despite all the evidence to the contrary (Park 2000 p14). Park continued to criticise cold fusion in his weekly blog *What’s new* until March 2009 when he wrote:

> Monday was the 20th anniversary of the infamous press conference called by the University of Utah in Salt Lake City to announce the discovery of Cold Fusion… Incredibly, the American chemical Society was meeting in Salt Lake City this week and there were many papers on cold fusion, or as their authors prefer LENR (low-energy nuclear reactions). These people, at least some of them, look in ever greater detail where others have not bothered to look. They say they find great mysteries, and perhaps they do. Is it important? I doubt it. But I think it's science. (Park 2009)

This is a complete *volte-face* from claiming cold fusion as the quintessential voodoo science to admitting it as a real science, albeit one that's not important.

The third independent witness is Robert Duncan, Professor of Physics and Vice Chancellor of Research at the University of Missouri. Unlike most physicists, Duncan has expertise in calorimetry, the sophisticated method used by cold fusion researchers to measure excess heat. Until late 2008 Duncan knew nothing about developments in cold fusion. He thought it had been debunked in 1989. He was invited by CBS TV documentary 60 minutes (More than Junk Science 2009) to travel to Israel and look at the research on cold fusion undertaken by Energetics Technologies Limited in 2008. In 2004, Energetics had used an innovative technique to get up to 25 times as much heat out of their experiment as energy that's put in. In 2008, after spending two days at the lab examining the experiments and talking to the scientists he concluded that "the excess heat, as I see it now, is quite real" (More than Junk Science 2009). Subsequently, Duncan has organised two symposia in which prominent cold fusion researchers presented their findings (E.g. Duncan 2009).

Gerischer, Park and Duncan have excellent reputations and have a lot to lose by coming out in behind cold fusion. They have because the evidence of nuclear scale excess heat from cold fusion experiments is now overwhelming.

Dieter Britz (Britz in Rothwell 2009) analysed the peer-reviewed papers and categorised them according to whether the conclusion supported cold fusion as being a real phenomenon or not and whether the author was undecided. Of the articles that reported experimental results 314[34] reported positive results, 218 reported negative results and 90 we undecided (Rothwell 2009 p10). According to Rothwell, there are no negative published results after 1992 although Shanahan (2002) has published a negative paper. Rothwell presumes that failed experimenters had given up by then. But positive results continue to be published in peer-reviewed journals.

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[34] I have included ICC4 proceedings as being peer reviewed. See Rothwell 2009 p3.

Dennis Cravens and Dennis Letts (2008) analysed 167 papers published between 1989 and 2007. They found that all negative experimental results could be explained by the experimenters failing to meet one or more of four enabling criteria that are now known to be required for a positive result[35]. Through a Bayesian statistical analysis of 122 of these papers, they show that cold fusion as evidenced by excess heat is a real phenomenon to a 99% confidence level (Cravens and Letts 2008 p21).

Many of these papers can be found on lenr.org and I encourage the reader to investigate for him or herself whether cold fusion is a terrible 20 year mistake. For a comprehensive review of the state of the art see Edmund Storms *The Science of Low Energy Nuclear Reaction* (2007). For an historical review of important scientific results see Marwan, J. and Steven Krivit's (eds.) *Low-Energy Nuclear Reactions Sourcebook* (2008)[36]. Details of an even more controversial aspect of cold fusion, low energy transmutations, are provided by Tadahiko Mizuno *Nuclear Transmutations: The Reality of Cold Fusion* (1998).

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[35] The criteria are Pd/D loading ratio greater than 0.85, Initial loading current density greater than 100mA/cm2, operating current density greater than 200mA/cm2 and an interruption to the equilibrium of the system (Craven and Letts 2008 p20).

[36] Interestingly, this volume is now available in the Trinity College Dublin library in the nuclear science section. This is another indication that cold fusion is being recognised as a genuine science.

Appendix B Cold fusion theories

The phonoic-coherence theories claim that the palladium metal crystal (called a lattice) is different from the vacuum in which hot fusion reactions take place. In particular, a metal lattice contains certain special sound vibrations called phonons. These phonons can become coherent, that is they can vibrate in time with each other. This coherent vibration, along with electromagnetic pressure, cause pairs of deuterons at the quantum level to become indistinguishable from helium nuclei in an excited state. If a shock is introduced to the system, the equivalence is broken and a helium nucleus results along with 23.8 MeV of energy that is transferred to the palladium lattice via the phonons vibrating more rapidly (Hagelstein et al 2008). Rapidly vibrating phonons are called heat. Hence heat is produced without the dangerous gamma radiation normally associated with deuterium fusion. These theories can explain the correlation between heat and helium production in the Miley, McKubre and ENEA experiments (McKubre 2008). They also underpin the idea Fleischmann had when he designed the experiments (Fleischmann et al 1994; Fleischmann 2000; Fleischmann 2003). However, these theories cannot explain the excess heat coming from certain ordinary (light) water cells. Nor can they explain many of the transmutation effects (Widom and Larsen 2006; Krivit 2010).

The shielding-tunnelling theories claim that the metal lattice confinement of deuterons causes large groups of them to act coherently, that is as one quantum mechanical entity. This coherence effectively reduces the Coulomb barrier or increases the probability of quantum “tunnelling” through the barrier due to quantum mechanical effects. In other variations, clouds of electrons, muons or yet undiscovered super heavy electron-like particles shield the deuterons and allows them to easily fuse. This is because the cloud of negative charge prevents the positively charged deuterons from repelling each other. (Chen and Li 2002; Hora *et al* 2002; Kasagi 2008)

The Widom and Larsen theory claims cold fusion is not fusion. That is, the heat does not come from fusing deuterons. Rather, the heat comes from a complex nuclear chain reaction of weak interactions. The weak nuclear force is used to explain radioactivity in decaying materials. For example, tritium, the third isotope of hydrogen, contains a proton and two neutrons. It is radioactive and it decays by changing one of its neutrons into a proton and an electron. The proton turns into a new hydrogen atom and the electron escapes as an energetic beta particle. Widom and Larsen argue the opposite happens in cold “fusion”. The strong electromagnetic field produced by the electric current applied to the palladium forces electrons into coherent waves near the surface of the material. These coherent electrons become highly energetic and therefore become heavier. When exposed to a shock, a heavy electron can be “captured” by a proton or deuteron to form “ultra cold” neutrons. These cold neutrons are very slow and thus become very large because of the laws of quantum mechanics. The ultra cold neutrons are then captured by nearby nuclei causing them to transmute into other elements. These larger nuclei decay and ultimately turn into harmless atoms such as helium and in the process they release heat. (Widom and Larsen 2006; Krivit 2010)

The Widom-Larsen theory can explain why there are many fewer neutrons released than predicted by “hot” nuclear theory. Importantly, it is completely consistent with the currently accepted laws of physics. It can also explain certain unexpected results in cold fusion such as transmutations and excess heat coming from ordinary (light) water cells. (Widom and Larsen 2006; Krivit 2010)

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