PPPL--2681

DE91 000487

COLD FUSION CATALYZED BY MUONS AND ELECTRONS

Russell M. Kulsrud
Princeton Plasma Physics Laboratory
Princeton University

Abstract

Two alternative methods have been suggested to produce fusion power at low temperature. The first, muon catalyzed fusion or MCF, uses muons to spontaneously catalyze fusion through the muon mesomolecule formation. Unfortunately, this method fails to generate enough fusion energy to supply the muons, by a factor of about ten. The physics of MCF is discussed, and a possible approach to increasing the number of MCF fusions generated by each muon is mentioned. The second method, which has become known as "Cold Fusion", involves catalysis by electrons in electrolytic cells. The physics of this process, if it exists, is more mysterious than MCF. However, it now appears to be an artifact, the claims for its reality resting largely on experimental errors occurring in rather delicate experiments. However, a very low level of such fusion claimed by Jones may be real. Experiments in cold fusion will also be discussed.

1 Introduction

As we come nearer to demonstrating that magnetic fusion can actually be achieved, the practical difficulties become more apparent, and it becomes attractive to look for alternative approaches that could be easier and more economic. The primary difficulty with fusion is the very high temperatures that are required to produce the fusion reaction. By analogy with chemical reactions one can say that the fusion reaction requires a high activation energy. The response of the chemist is to look for an agent that can serve as a catalyst for the reaction, which would allow it to proceed at lower temperatures and more rapidly. For fusion it has been found that the mu meson, or muon, can catalyze the fusion reaction by forming very compact molecules of Deuterium and Tritium, the $dt\mu^+$ mesomolecular ion that fuses automatically in a very short time. Such fusion by muons is termed Muon Catalyzed

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Fusion, or MCF. This type of fusion has been known for over thirty years, and research on it has been vigorously pursued during the last ten. As a result of this effort it is now appreciated that the muon costs too much energy for the fusion it yields, and any hope for practical generation of energy by MCF is very slim.

If there were a stable heavy electron, the hope for practical fusion catalyzed by this heavy electron would be much brighter. For this reason the apparent discovery by Pons and Fleischmann that fusion could be produced by the ordinary light electron itself in electrolytic cells, generated a great deal of excitement. Unfortunately, this discovery of what was termed "Cold Fusion", proved to be very elusive. Not enough detail concerning the initial experiments of Pons and Fleischmann was given to properly duplicate the exeriment. So many experiments were performed hastily in an attempt to verify "Cold Fusion" that the results proved very confusing. This was so complicated by pressure from the press to immediately know the answer, that a number of serious mistakes were made, and announced results had to be withdrawn. However after a period of time, serious and careful experiments were performed, which led to the belief that the first encouraging results were probably the results of experimental error. It is now the general feeling, at least in the United States, that "Cold Fusion" is not a real phenomenon. (However, there is still some hope that some very slow cold fusion results obtained by Jones could be real. Although these are much too slow to be of any practical interest, they represent scientifically very interesting results.)

In this paper I shall first discuss Muon Catalyzed Fusion, and then "Cold Fusion".

2 Muon Catalyzed Fusion

It is well known that a single electron can bind a deuterium nuclei, d, and a tritium nucleus, t, into a molecular ion, dte^+, and that the mean distance between the d and t nuclei is 7×10^{-9} cm. In an analogous manner a negative muon μ^- can bind the same two nuclei into a mesomolecular ion $dt\mu^+$ for which the internuclear distance is smaller by the ratio of the masses. Since the mass of the muon is $212m_e$, the internuclear distance is only 4×10^{-11} cm. As a result the Coulomb barrier between the two nuclei is much smaller, and it can be penetrated by tunneling in a time of order 10^{-12} sec.[1] That is, the rate of fusion for such a molecule λ_F is 10^{12}sec^{-1}. By contrast the rate for the electron ionic molecule is $\lambda_F = 10^{-60}\text{sec}^{-1}$. See Figure 1.

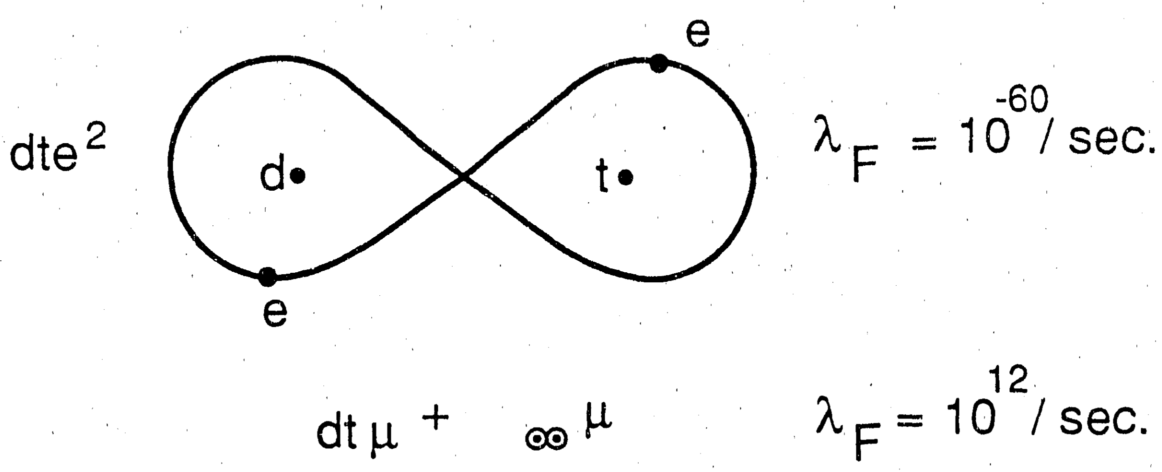

Figure 1: Muon Fusion

After the fusion occurs the muon is generally kicked free so that an alpha particle, a neutron, and a muon emerge from the fusion reaction. However, during a small fraction ω_S^0 of the fusions the muon gets captured by the rapidly moving alpha particle. Even in this case the muon may be removed from the alpha particle, as it slows down, by charge exchange or ionization if the fusion occurs in a dense target. The probability of the latter, R, is called the reactivation probability. Thus, the final probability that after a fusion occurs and the alpha particle slows down to rest, that the muon is free to perform more fusions is $\omega_S = R \times (1 - \omega_S^0)$. ω_S is called the effective sticking constant, and ω_S^0 the initial sticking constant. It turns out that ω_S is very small, less that about .005, so that the muon acts as a catalyst, and can generate a long series of fusions. After each fusion the untrapped muon leaves the fusion with a kinetic energy of approximately 10 Kev. In a dense target it quickly slows down to low energy (of order 1 ev), then forms a new $dt\mu^+$ molecular ion and produces the next fusion. The series terminates when either the muon decays into an electron and two neutrinos, or when it becomes trapped by the alpha particle being rendered inoperative as a catalyst. Thus, the muon is not truly a catalyst, since it does get used up in forming the fusion reactions, but it acts like a catalyst in making the reaction go more easily.

There are three important limitations in MCF.

1. The lifetime of the muon is 2.2×10^{-6} seconds so that in order to produce a large number of fusion cycles every process must occur very rapidly. For this reason muon fusion has to occur in very dense targets, targets whose densities are comparable to liquid hydrogen densities.
2. The formation of the $dt\mu^{+}$ molecule takes a significant time even at liquid hydrogen densities. This time must be sufficiently short that a large number fusions can occur.
3. The trapping of the muon during fusion leads to a limit on the number of fusions that can be achieved.

When MCF was first discovered[2] estimates of the molecular formation rates were very low. However, during the period 1960–1980 a resonance for this formation was predicted by the Russians[3]. This led to the prediction of very fast formation rates for the molecule, so that limitation (2) did not appear to be so serious. On the basis of this encouraging result, new experiments were carried out in a number of laboratories during the 1980's to examine MCF in more detail.[4] With regard to limitation (3), the early estimates of ω_S were about 1.0 percent. At first, it was found experimentally that an upper limit on ω_S was 0.5 percent. The fact that this was considerably less than the early theoretical estimate led to considerable optimism concerning MCF since ω_S could very well be much smaller. This parameter appeared to be the true bottleneck in fusion. Thus, during the 1980's considerable effort was devoted to the determination of this parameter. As a result of some very fine work in a number of different countries it was finally determined that the experimental and theoretical values for ω_S were about 0.5 percent This leads the conclusion that the average number of fusions generated by one muon is less than 200. It turns out that many more fusions are required in order that the resultant fusion energy is sufficient to produce a new muon. The precision with which this number is believed to be known, leads to the conclusion that pure muon fusion is uneconomic.

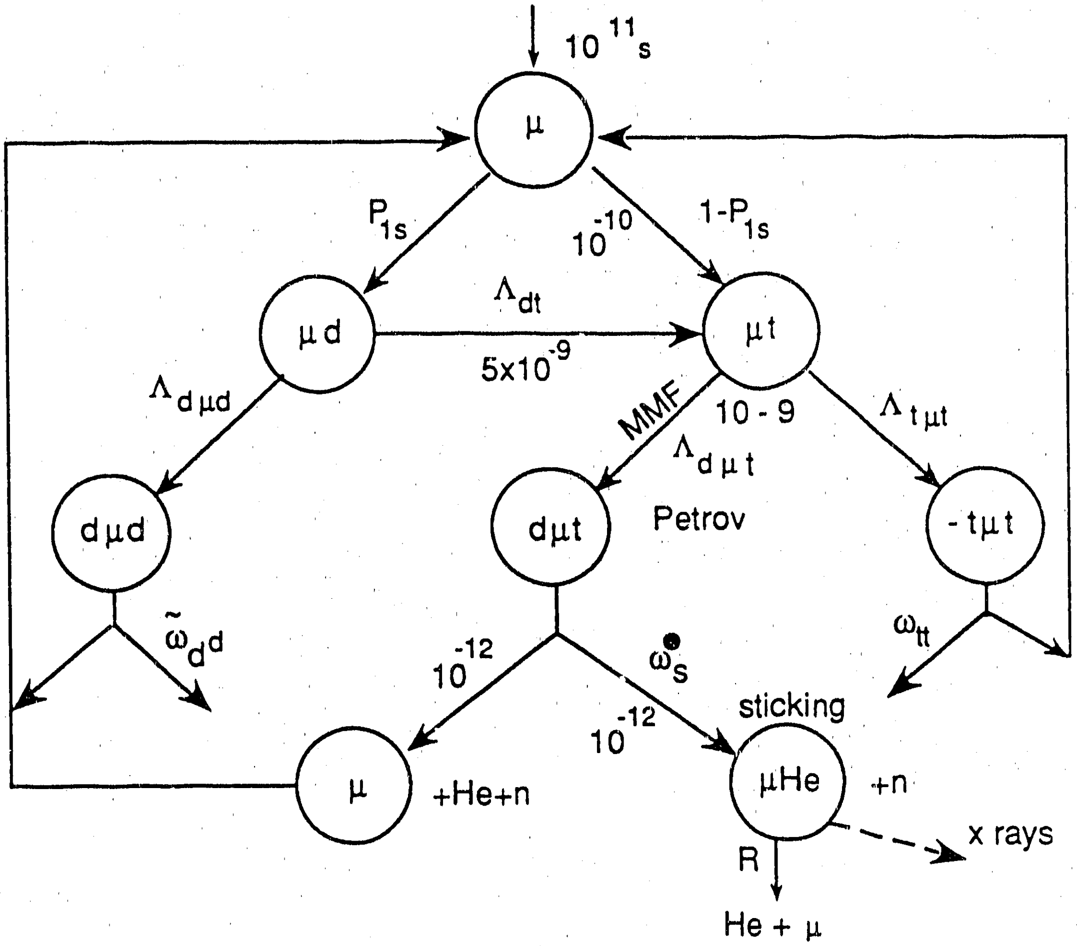

Figure 2: The muon fusion cycle

Let us examine the various physical processes in an MCF cycle. they are illustrated in Figure 2.

Each state in the cycle is indicated by a circle. Several branches are possible. The arrows indicate the reactions from state to state. The times for these reactions are indicated for a liquid hydrogen density of the target which consists of D and T in a fifty–fifty mixture.

The free muon slows down after being released from the last fusion, in 10^{12} seconds. When it reaches 1 or 2 eV. it is captured by either a d or a t nucleus with equal probability in a further 10^{-10} seconds to form a muonic atom. If it is captured by a t, then the most likely subsequent event is that it captures a d nucleus from either a DD or a DT molecule to form a $dt\mu^+$ mesomolecular ion. After the capture the original large molecule is not immediately destroyed but instead an exotic molecule is formed which consists of replacing the d nucleus in the large electronic molecule by the very small $dt\mu^+$ ion. It turns out that if this exotic molecule is excited to the second vibrational state, then the energy of this exotic molecule including the binding energy of the $dt\mu^+$ molecular ion, is almost exactly equal to the energy of the initial situation. The mismatched energy is very small of order 0.01–0.02 eV. It is positive for the DD case, and can easily be gotten rid of by collisions. It is negative for the DT case, and can be made up for by thermal motions. This resonance thus leads to a much larger molecular formation rate than had been thought possible and was the chief source of optimism for MCF. The time of formation is about 10^{-9} seconds for the DD case, and 5×10^{-9} seconds for the DT case.

Another possibility is that the μt atom combines with a t nucleus to make a $tt\mu^+$ molecular ion and produce tt fusion. Since the sticking probability for the muon in this fusion reaction is relatively large it is fortunate that the probability of this branch of the muon cycle is small.

Let us return to the case in which the muon is captured by the d nucleus to make a $d\mu$ muonic atom. The probability for this atom to capture a t nucleus is very small because it is nonresonant. The preferred reaction is a transfer of the muon to a t nucleus, and then the formation of the $dt\mu^+$ mesomolecular ion as above. This transfer takes approximately 5×10^{-9} seconds. (Again there is a low probability branch through the $dd\mu^+$ molecular ion which has a large sticking probability.)

Finally, after the dt fusion from the $dt\mu^+$ molecular ion, there is the important branching between the case where the muon is free to return to start the cycle again (with probability $1 - \omega_S^0$) and the case where the muon is captured by the 3.5 MeV alpha particle (with probability ω_S^0). Even this path branches into the case where the muon is removed from the rapidly moving alpha particle, either by charge transfer to the d or t nuclei or by ionization by collisions with them (with probability R), or the case in which it stays bound to the alpha particle till the latter comes to rest. The latter branch is the main source of loss of the muon from the cycle.

Thus, in examining the diagram of Figure 2, we see that the main bottleneck in slowing the cycle down, is the time for transfer of the muon from the d to the t nucleus in the cases when the muon is initially captured into a $d\mu$ atom. Even this case is faster than it first appears since the capture occurs first into higher levels of the $d\mu$ atom and the cross section for transfer to the t is large for these states. The actual probability of forming a $d\mu$ atom in the ground state and passing through this particular bottleneck could be considerably smaller than 50 percent.

Each time that the muon goes through the cycle of Figure 1, it may pursue

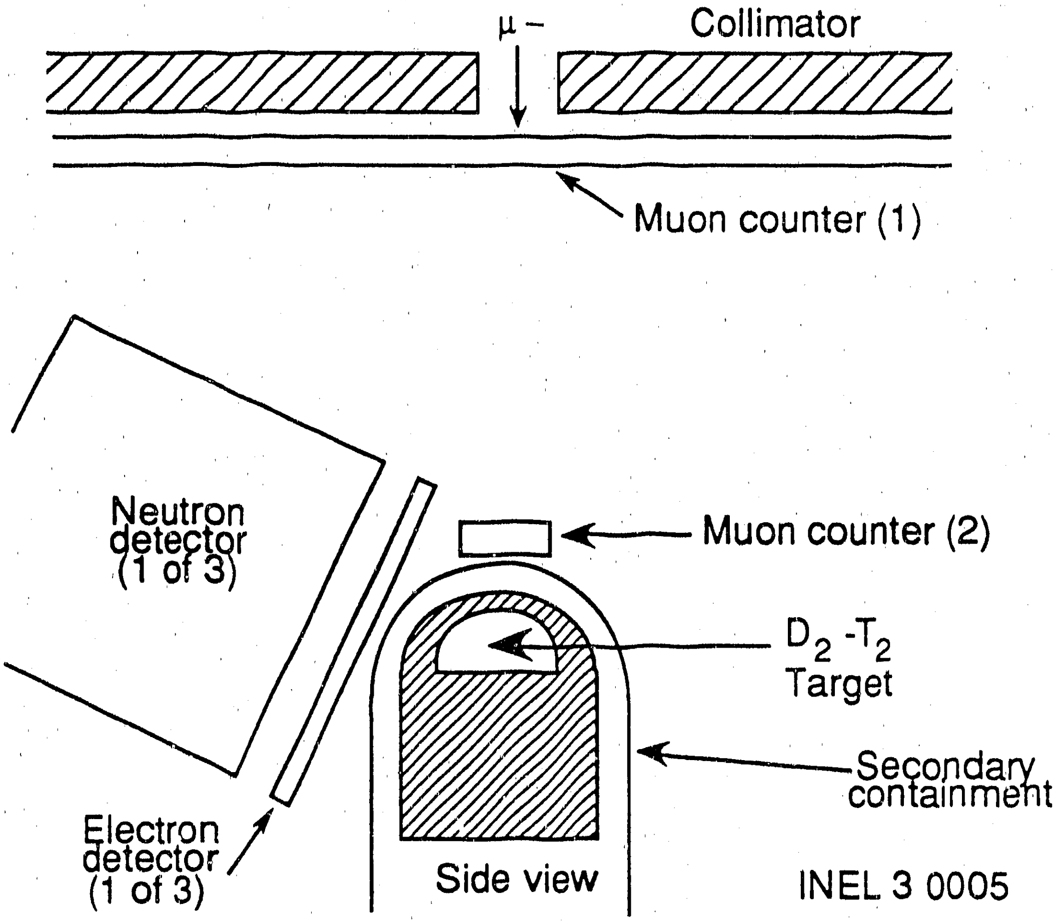

Figure 3: Experimental Setup

any of the indicated paths. The important question is, how many times per second does it go through the cycle and produce a dt fusion reaction. This rate is called the cycle rate and is denoted by λ_C. It is a directly measured quantity. The measurement of it is performed with the apparatus shown in Figure 3.

The muon enters the target chamber from the muon source above. The muon flux is low enough that only one muon is present in the chamber at any given time. The time at which the muon enters the chamber is registered. After the muon is in the chamber, it starts to catalyze dt fusions, and the resulting neutrons are detected by the neutron detectors. The time of arrival of the neutrons is recorded relative to the time of entrance of the muon. The flux of neutrons ϕ per muon, as a function of this delay time, is proportional to the probability that the muon is still present, times the cycling rate,

$$\phi = \lambda_C exp[-\lambda_D t - \lambda_C \omega_L t] \tag{1}$$

where $\lambda_D \approx 4.5 \times 10^{-5} \text{sec}^{-1}$ is the decay rate of the muon, and ω_L is the average probability of loss by any sticking process, per cycle.

From the determination of ϕ the cycling rate, λ_C, and the loss rate, ω_L, can be derived. By varying the isotopic composition of the target the rates of the various branches of the cycle are varied, in a known way. By an analysis of the variation of the cycling rate and the muon loss per cycle, with composition, the rates of the various reactions can be extracted.[4]

These rates have been compared with the theoretical calculations and the comparison is quite good. As to the important parameter ω_S, the measured loss probability ω_L only gives an upper limit for it. However, this upper limit is actually quite close to the more careful theoretical calculations of it carried out during the 1980's.[5]

Although the procedure outlined above seems relatively simple the results that have been obtained from it represent a considerable amount of very high quality research performed by several groups in a number of countries such as the Soviet Union, Switzerland, Japan, England, the USA and others.

The expected number of fusions produced by a single muon, χ, can be expressed in terms of λ_C and ω_S as

$$\chi = \frac{1}{\lambda_D/\lambda_C + \omega_S}. \qquad (2)$$

For $\omega_S = .005$ and $\lambda_C = 10^9$ sec^{-1}, one finds $\chi = 183$. The maximum number of fusions per muon that has been observed is 170. Since $\chi < 1/\omega_S$, the only way for χ to be very large is for ω_S to be much smaller than the experimental upper limit, and for the theory to be incorrect.

There was indeed some uncertainty still left in the theory, associated with the fact that the final state after the fusion, involved a strong nuclear arrangement which could somehow perturb the value of ω_S^0. Further, intensive research into this question finally led to the conclusion that this perturbation must be small, and that the value for the initial sticking probability was correct. Further research into the reactivation coefficient, R, narrowed its value down to approximately 0.3 – 0.4. The result of these two investigations led to the conclusion that the value .005 for ω_S must be close to the correct one.

Meanwhile, several direct experimental determinations of ω_S were made at the same time. These involved two approaches. The first involved looking for the number of X-rays emitted per fusion. These X-rays were emitted when the muon was captured into an excited level of the 3.5 MeV alpha particle. The number of such X-rays could be related to the total number of captures onto the alpha particle. The other direct measurement was to allow the alpha particles to leave the target chamber (by working at low density targets with thin walls), and to determine the ratio of number of singly charged alpha particles which have captured a muon, to the total number of alpha particles.

There were two separate experiments to measure the X-rays, one in Japan[6] and one in Switzerland.[7] The Japanese experiment led to a value for the effective sticking coefficient that was much lower than .005, but unfortunately, there was some difficulties with the experiment that threw doubt on this result. A second experiment carried out in Switzerland gave results in agreement with the theoretical result. The direct observation of the alpha particles, a quite difficult experiment, was a much more direct measurement of the sticking coefficient. It was completed last year and its result on the sticking coefficient also agreed with the theoretical result.[8]

Thus, on the basis of all this work, in particular on the last experiment, it has become accepted that the number of fusions generated per muon is not much larger than 200. Is this number large enough to be of practical interest? For this to be the case the fusion energy generated per muon must be more than enough to produce another negative muon. Muons are produced in accelerators by impinging an energetic beam of particles on a target, to generate pions. The muon arises when the pion decays. Both positive and negative muons are produced in this way, but only the negative muon catalyzes the fusion reaction.

By a careful analysis, it has been shown that at least $E_\mu = 5$ GeV of beam energy is necessary to generate one negative muon. The bulk of the energy generated by the fusion process is in the form of 14.5 MeV neutrons. These neutrons yield approximately $E_F = 20$ MeV of thermal energy. A fraction, η_R, of this thermal energy must be converted to energy of an accelerator beam, the rest being available for practical power generation. The efficiency of this conversion is $\eta_T \eta_A$, where η_T is the efficiency of converting thermal energy to electricity, and η_A is the accelerator efficiency. From these considerations we find that

$$\chi = \frac{E_\mu}{\eta_R \eta_T \eta_R} \approx 1500 \tag{3}$$

where we have taken $\eta_R = 0.5$, $\eta_T = 0.4$, and $\eta_A = 0.8$.

On comparison of this required value with the realistic value of $\chi_{observed} = 200$, we see that muon catalyzed fusion is essentially ruled out as a practical way to generate useful power. Further, the main difficulty with MCF can effectively be traced to the fact that ω_S is not small enough. This explains why there has been such a great interest in whether the sticking coefficient could be much smaller.

There is actually a way to reduce the effective sticking coefficient to a much smaller value[9]. One accomplishes this not by changing the initial sticking coefficient ω_S^0, but by increasing the reactivation coefficient R. While the alpha particle is rapidly moving there is a good chance to remove the muon, but the drag on the alpha particle slows it down in such a short time that the integrated chance of stripping off the muon is only about 30 percent. By keeping the alpha particle moving at a kinetic energy above 150 KeV, one can always be certain that the alpha particle can be removed.

This can be accomplished by fragmenting the DT target into thin rods as in Figure 4. The rods must consist of solid or liquid DT (at a temperature below 25° kelvin) so that no walls are required to confine them. They must be chosen with such a small radius that an alpha particle with a trapped muon produced in a rod can readily escape from the rod into the space in between the rods. In this space the alpha particle can be cyclotron accelerated by a rotating electric field and a static magnetic field parallel to the axis of the rods. As the alpha particle moves in and out of the rods, it is alternatively decelerated by drag of the target and accelerated by the electric field. The filling factor of the rods and the magnitude of the electric field can be adjusted so that the alpha particle is maintained at an energy close to the optimum energy for stripping.

The process must not take too long, else the muon would decay. To avoid this requires a rather large electric field, and a correspondingly large filling factor for the rods. Numerical simulations show that this idea should actually work, and that a reasonable estimate for the total number of fusions per muon is

$$\chi = \frac{2000}{1 + (450\mathrm{KV/cm})/E}. \qquad (4)$$

From this formula one sees that an electric field of 450 KV/cm would lead to 1000 fusions per muon.

Unfortunately, this idea does not lead to a practical scheme for MCF. The reason is the following: In order for the idea to work, it is necessary for the target to be kept at a temperature below 25^oK and thus all the heat

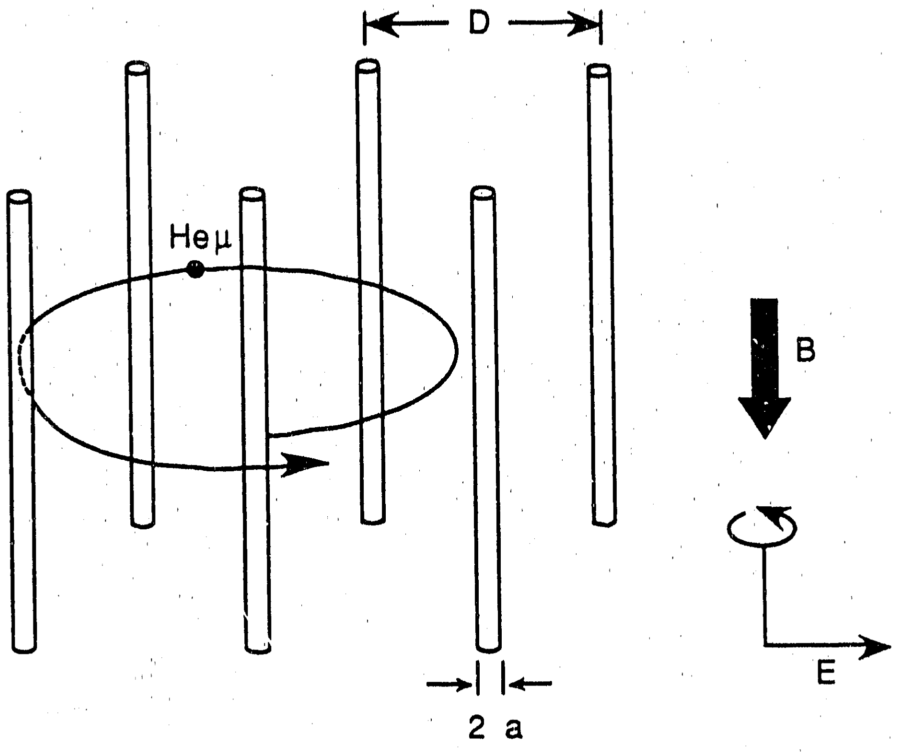

Figure 4: Scheme to increase the number of fusions per muon

generated by the alpha particle part of the fusion energy, (3.5 MeV per fusion) must be removed with a very low Carnot efficiency. Thus, it is necessary to supply at least 30 MeV per fusion to keep target cool. This is larger than the energy gained from the rest of the fusion, and so no net energy is made at all!

However, a suggestion has been made to avoid this difficulty by allowing the target to expand freely while it heats up to a very high temperature, so that the thermodynamic difficulty could be avoided .[10] This suggestion itself does not quite work because the expansion rate of the thin rods is very short. However, it is not impossible that some variant of this idea might be made to work.

In conclusion, one can say that, other than by some very exotic scheme such as this one, it is probably impossible to extract useful power from muon catalyzed fusion.

3 "Cold Fusion"

In 1986, at about the time when the hopes for MCF were fading, Steve Jones, at Brigham Young University, one of the chief workers in MCF, started thinking about the possibility that under certain conditions one might get fusions catalyzed by electrons. In particular, since fusion rates are very sensitive to the distance between the molecules, great pressure exerted on molecules might reduce this distance, and lead to more rapid fusion. Jones never considered that such an effect could lead to a practical energy source, but he thought that it could have some geological importance. It is well known that excess heat is coming from the earth's interior, and that a certain amount of He_3 is present in volcanoes. Even some tritium has been observed. Could it be that under the great pressures below the surface of the earth, fusion catalyzed by electrons was occurring at a sufficient rate to explain these results? It was not possible to exert pressures on DT molecules sufficiently large enough to produce a measurable amount of fusion, according to theoretical calculations. However, it seemed possible that an equivalent effect might be achieved by employing the phenomena of electrolysis, relying on a possible compression in the electrodes. Since palladium was noted for its absorption of hydrogen and its isotopes, electrolysis experiments employing palladium and platinum electrodes were carried out. Since it was expected that the fusion rate would be very low, it was necessary to develop techniques to measure very low neutron fluxes. This development took nearly three years. Finally, Jones obtained definite evidence for fusion, but at the very slow rate of approximately one neutron per second.[11] This corresponded to a fusion rate per pair of DD molecules of 10^{-24} per second.

Simultaneously with these experiments, Pons and Fleischmann, two chemists at the University of Utah, also found evidence for fusion in electrolytic cells. The essential difference between their results and Jones' is that they measured excess heat coming from their cells, which is large enough to be of practical interest for power generation. The fusion rates to produce this heat are 10^{-10} per DD molecule per second. They even detected neutrons and tritium, direct evidence that fusion was going on in their cells. However, the fusion rates inferred from these direct detections are some eleven orders of magnitude slower than those inferred from their excess heat measurements. That is, the conclusions one could draw from their experiments are that such a large amount of heat is being generated that it could only be of nuclear origin. But if this heat is due to nuclear fusion of the DD molecules, then the fusion had to be of a entirely different character from the familiar DD fusion reaction.

Since the situation between the two groups of experimenters has become very complicated, let me set down the history as I personally know it:

1984-1989 Pons Fleischmann experiments.

1986-1989 Jones experiments

1986 Van Sieclin-Jones paper published suggesting the possibility that fusion may be induced by electrons in the earth.[12]

Sept., 1988 Jones told me of his ideas on the telephone.

Jan. 1989 Pons and Fleischmann apply for funding and their proposal is refereed by Jones. Nuclear detection of fusion reactions is suggested by him.

Feb., 1989 Jones contacts Pons and Fleischmann and suggests simultaneous publication. Submission of independent papers to be on March 25.

March Pons and Fleischmann submit their paper to J. of Electrochemistry.

March 23 Pons and Fleischmann hold a press conference announcing their results without telling Jones.

March 23–April 10 Chaos until Pons and Fleischmann paper appears.

April 10 Pons and Fleischmann paper appears.[13]

late April Pons and Fleischmann present their results to American Chemical Society at Dallas, where they are acclaimed as discoverers of a new important process. (Furth raises the question of a control experiment.)

May 2. Special session held at American Physical Society (Baltimore) at which all of Pons and Fleischmann's claims were contradicted and conflicting results were presented.

June 30 Interim report of the Ramsey DOE committee presented giving little support to "Cold Fusion".

Nov. 16 Final report of the committee appears, concluding that Cold Fusion has not been established by any believable experiment, with the possible exception of the Jones' experiments.

The essential facts to be noted from this history are that the research of the two groups was definitely independent. Jones had documented, by a refereed publication, that he had been thinking about cold fusion for at least three years. Pons and Fleischmann claim they had been carrying out cold fusion experiments for five years, but the amount of work done could not have been extensive, since no control experiments were done during this period.

When Pons and Fleischmann announced their claim that they had achieved cold fusion in a test tube with rather moderate equipment, and had achieved power levels of practical interest, the response from the public was so strong that it interfered with a calm appraisal by the scientific community of the reality of their claims. Also because of the very important practical implications of their claims, they were very reluctant to release the details of their experiments.

This was doubly unfortunate. If their claims were actually correct, it would be important for them to share their breakthrough into a new cheap method of power production, in order it could be exploited as rapidly as possible with the aid of the entire scientific community. On the other hand if their claims were false, then it was equally important to ascertain this as soon as possible in order to lessen the public's disappointment that this method did not work. As it was, the appropriate response of the scientific committee was to wait for publication of the full details of their work, before evaluating it. But such a rational approach was not possible because the public needed to know as soon as possible whether there was any reality to this method of producing cheap energy by cold fusion. The best the interested scientists could do was, to try to guess what the results were, and to attempt to confirm or deny them by duplicating the experiments as best they could, from the information that appeared in the press and on television.

Two types of experiments were quickly performed. The first type confirmed the Pons–Fleischmann results, but because of the delicacy of the measurements, and the haste with which they were made, most of these experimental confirmations turned out to be incorrect, and had to be withdrawn. For the second type of experiments, those which failed to confirm the Pons and Fleischmann results, the failure was attributed by the proponents of cold fusion to an incorrect duplication of the Pons–Fleischman experiment. Thus, until the Pons and Fleischmann paper appeared it was not possible to contradict the Cold Fusion results.

This paper was awaited with great eagerness, but when it appeared there was considerable consternation. The paper was not sufficiently clearly written to tell precisely what procedures Pons and Fleischmann had followed to obtain their positive results in cold fusion, or even to tell exactly what these results were. Pons and Fleischmann did not specify the voltages that were employed in their electrolytic cells, nor the actual heat which they observed, nor even exactly what their definition of excess heat was. However, they did quote the various efficiencies that could be obtained from their data, and from these one could work back to this original data.

The results of this exercise are displayed in the Table shown below. To understand these numbers, one must understand the energy balance involved. There are nine different experiments represented. Three different experiments with each of three different size electrodes of diameter d and length $l = 10$ cm are listed. listed in the table. The first row is the electrical current I in amps, flowing in the cell, and the second row is the voltage V in volts applied to produce the electrolysis. The total external power applied by the battery is IV. Part of this power, IV_D, where $V_D = 1.54$ volts, is that necessary to produce the breakup of the deuterated water, while the remainder, IV_J, is the Joule heat necessary to drive the electrolytic current through the electrolyte. This is called the Joule power. This Joule power heats the electrolyte and the amount of this heating can be measured directly by submerging the electrolytic cell in a bath of constant temperature measuring the temperature of the electrolyte, and determining the power needed to lift the electrolyte to its measured temperature. This power was found to exceed the Joule power by a certain amount which Pons and Fleischmann called the excess power, although this definition is not made very clear in their paper. I denote this excess power as $I\Delta V$ in my table.

One key assumption made by Pons and Fleischmann is that all the power associated with the enthalpy of the disassociated water is carried away as deuterium and oxygen vapor and is not recovered. This assumption is not very clearly substantiated by them. It is represented by the voltage of 1.54 volts. The source of the excess heat is claimed to be the energy of nuclear fusion. However, it is seen that if a certain fraction of the vapors recombine they will release energy back into the cell, and this energy can contribute to the excess heat energy $I\Delta V$. By inspection of the table, one can see that for all but the last two cases the voltage associated with disassociation is larger than the voltage associated with the excess heat. In the next to last case, it is only slightly greater. For almost all of their cases a slight amount of recombination can easily account for the excess heat on which all the excitement was based. A further criticism leveled at the Pons–Fleischmann experiment is that they did not directly stir their electrolyte, and thus they may not have measured the correct effective temperature needed to determine the heat evolved in their cell.

Thus on the basis of the excess heat alone the Pons–Fleischmann results are in doubt. Further, their direct measurements of fusion were also shown to be in doubt. Their measurements of the neutron flux, through a determination of gamma rays produced by the neutrons was spurious. The gamma rays were actually produced by the radioactivity of radon gas present in their laboratory. Finally, the tritium they observed was most likely already present in their sample of deuterated water, before it was introduced into the cell. The final conclusions of the scientific community were that there is no sound experimental basis to believe in the cold fusion results of Pons–Fleischmann. A proper announcement of their experiment in a refereed journal could have cleared the question concerning the reality of cold fusion quietly with little of the fuss that resulted from the overpublication of their result. Perhaps, there is a lesson to be learned here.

size = $d \times 10cm$.

d	.1	.1	.1	.2	.2	.2	.4	.4	.4
I	.025	.20	1.61	.050	.40	3.21	.10	.804	6.43
V	2.5	3.59	8.13	2.67	4.25	8.54	2.88	4.864	8.680
V_D	1.54	1.54	1.54	1.54	1.54	1.54	1.54	1.54	1.54
V_J	.96	2.05	6.59	1.13	2.71	7.00	1.33	4.42	7.14
ΔV	.298	.395	.406	0.72	1.23	.939	1.35	2.189	4.166

4 Acknowledgment

This work was supported by U.S.Department of Energy Contract No. DE-AC02-76-CHO3073.

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10 / 31 / 90