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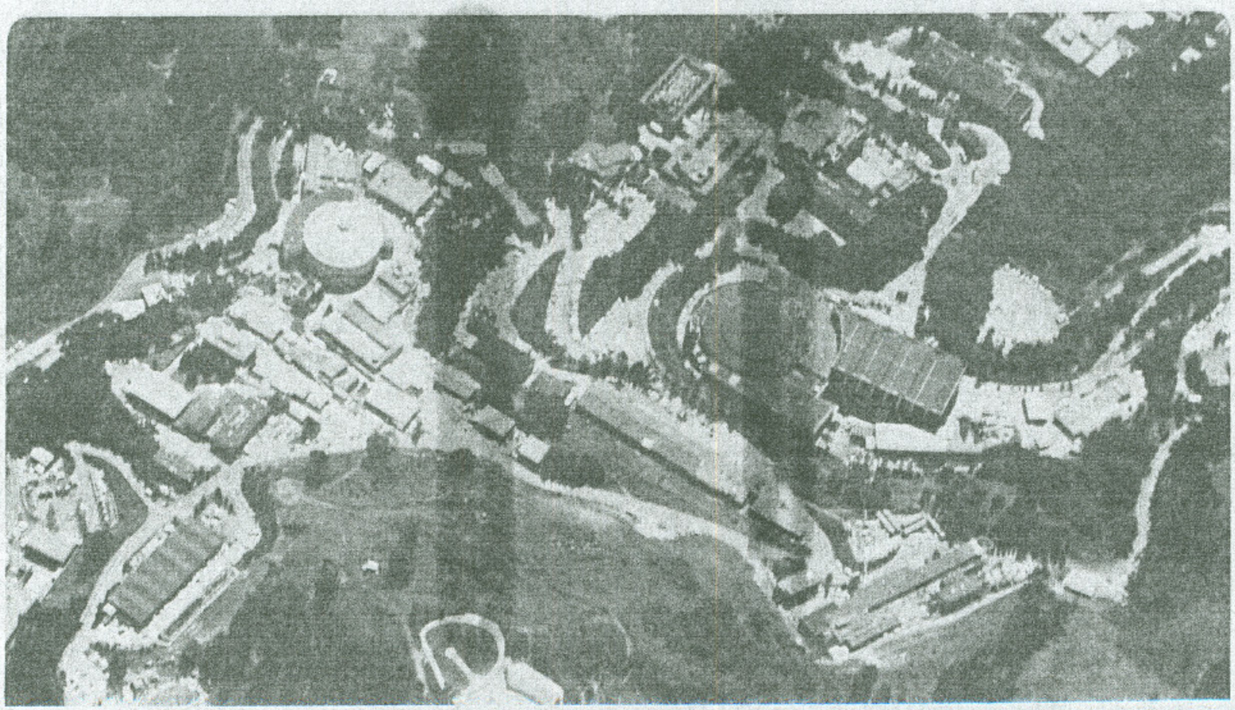
J.D. Jackson

August 1984



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August 1984 LBL-18266

MUON CATALYSIS OF FUSION: A COMMENTARY

to accompany the reprinting of

"CATALYSIS OF NUCLEAR REACTIONS BY μ MESONS"

by

L.W. Alvarez, H. Bradner, F.S. Crawford, Jr.,
J.A. Crawford, P. Falk-Vairant, M.L. Good, J.D. Gow,
A.H. Rosenfeld, F.T. Solmitz, M.L. Stevenson,
H.K. Ticho, and R.D. Tripp,

The Physical Review, Volume 105, Number 3, pp. 1127-8
(1 February 1957).

by

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1. Introduction

Discoveries in experimental science are of several kinds. Some are the results of well-focused, systematic, quantitative studies of phenomena whose qualitative, empirical aspects were at least partially known. Coulomb's and Amperé's discoveries are in this class, as are Rutherford and Soddy's elucidations of the nature of radioactive transformations. The numerous achievements of the Alvarez bubble chamber group, for which Alvarez received the Nobel Prize in Physics, are also of this type, even though the initial motivation for a program of bubble chamber studies was the study of strange particle decays. As the utilityof the technique for the study of hadronic resonant states was being established, a powerful array of detectors and analyzing tools was being developed, making the group preeminent in high energy physics in the late 1950's and 1960's.

Another class occurs when experiments intended (at least in part) to pursue an anomaly of earlier work lead to unexpected discoveries. Two examples are the discovery of CP violation and the discovery of the ψ particle in e^+e^- collisions. Another class of discoveries are the result of hunches that in retrospect were based on an erroneous interpretation of other relatively new discoveries. Becquerel's incorrect hunch about the true source of Roentgen's x-rays led to the discovery of radioactivity. Still others stem from the testing of theoretical concepts. Rutherford's desire to test J.J. Thomson's "currant bun" model of the atom led to the discovery of the atomic nucleus (and the destruction of Thomson's model). The discoveries of the Ω^- particle at Brookhaven in 1964 and of the $W^\pm$ and Z^0 particles at CERN in 1983 are similar examples, this time with theoretical expectations triumphantly confirmed. Perhaps the most dramatic in this class is the discovery of nonconservation of parity in weak interactions. Experimenters were instructed in detail on where to look and what to expect to find.

A final class of discoveries are those that, because of the state of our understanding of the laws of nature at the time, are better called "observations". Thus, by the 1940's, our understanding of quantum mechanics and quantum electrodynamics was such that few would doubt that essentially any pair of oppositely charged particles could form hydrogen-like atoms. "Exotic atoms" only awaited discovery of new charged particles (the muon and the positron were already at hand) and development of the requisite experimental techniques and the interest. The discoveries of positronium, mesic atoms and muonium are clearly different in character from the discovery of, say, radioactivity. But to call them "observations" in no way diminishes the achievement of discovery, often technical *tours de force*, or the importance of their consequences.

The observation of the catalysis of nuclear fusion by muons in a liquid hydrogen bubble chamber by Alvarez and coworkers in late 1956 [1] is a classic discovery of the last type, but differs from mesic atoms or muonium in being a discovery that was not sought. It was entirely accidental, totally unexpected, and peripheral to the group's main study of hadronic interactions. From the point of view of fundamental physics, the phenomenon is completely "understood", yet it was viewed by its discoverers and others (like me) as bizarre. It led to a flurry of speculation on energy production by "cold fusion". (See Figure 1 and refs. 1 and 2.) Then, apart from an initial few experiments and a steady but low level of interest in the problem by theorists, twenty years passed before there was a resurgence of interest. (See Figure 2.)

This commentary presents a brief and spotty survey of the subject of muon catalysis of fusion of hydrogen isotopes from its beginnings to the present day. It begins with some personal recollections. Then follows a very rapid skimming of the theoretical and experimental research, leading to a brief discussion of the reasons for the present level of interest in the subject and the prospects for useful energy production.

2. Personal recollections

During the academic year 1956-57 I was visiting the Physics Department at Princeton University on a John Simon Guggenheim Memorial Fellowship. During the fall I thought about a few problems in nuclear physics, but had not settled into any

serious research. By early December there were rumors about parity non-conservation in the experiments of Wu *et al.* and also rumors of a μ' meson at Berkeley, but nothing definite, and by the time of the long Christmas vacation, Palmer Laboratory was virtually deserted. Despite Christmas visitors, I seemed to keep going in to work. Now, one of the virtues of being within 100 miles of New York City in those days was the delivery to one's doorstep each morning of the New York Times. On the morning of 29 December 1956, I read in the New York Times a report of a paper presented the previous day at an American Physical Society meeting in Monterey, California. My browned clipping from the Times is displayed in Figure 1.

My imagination was stirred by the newspaper article, and I began to work on the nuclear fusion aspects of muonic diatomic molecular ions, the capture of the muon by the moving helium fragments after fusion, and the possibility of its liberation during the slowing down of the fragment, as well as speculations about energy production. The most important conclusions of my work were that in the energetically interesting case of the fusion of deuterons with tritons the nuclear reaction rate once the molecular ion is formed is extremely fast ($\Gamma \geq 10^{12}\ s^{-1}$), and that, whatever the rates of molecular processes, there was an upper limit of the order of 10^2 on the number of possible fusions caused by the muon because of capture by the produced alpha particle, independent of the muon's lifetime or mass. The last conclusion vitiated the remarks attributed to Alvarez at the end of the news story about the efficacy of a possible longer lived lepton.

I must have worked feverishly for my files show that a paper entitled "Catalysis of Nuclear Reactions between Hydrogen Isotopes by μ^- Mesons" [2] was sent to the Physical Review on 9 January 1957. They also contain a carbon copy of a letter to Alvarez, dated 5 January 1957, that said in part:

> *Your μ' meson and its explanation were featured in the newspapers of this area around the New Year. Having nothing better to do, I began playing with the problem. The enclosed rough draft is the result (excuse the typing in*

> *the paper – it's my own). It is more than likely that you and your group have done similar and better calculations already. I found it entertaining, anyway, to see what could be done from first principles in the way of estimating the various rates. The speculations on power production are, of course, very wild and probably wrong.*

[For what follows it is necessary to point out that in the manuscript the first reference read, "[1]L.W. Alvarez *et al.*, New York Times CVI, No. 36, B4, 1 (December 29, 1956)."]

Alvarez replied on 8 January. (In passing, note the efficiency of transcontinental postal service in 1957!) I quote the handwritten letter in its entirety:

> *Jan. 8, 1957*
>
> *Dear Dr. Jackson:*
>
> *It was good of you to send us your very interesting report. My theoretical friends are fighting over it at the moment. We are trying to estimate the number of μ's which get stuck on the recoil He^3, after a $D + D \rightarrow He^3 + n$ reaction. If the μ isn't stripped in the recoil, it can't get away. If the stripping cross-section is high enough, we'll look for a big burst of neutrons from a Dewar flask of D_2 above the large scintillator tank of the Reines-Cowan type, which is here in Berkeley. We had thought of such experiments before, but never could see how the chains would be long enough to make it interesting.*
>
> *We have a large group of molecular experts now working hard on the whole problem. Teller and McMillan have been thinking hard since the earliest days of the effect, and they have been joined by a bunch of younger people. Much of their work parallels yours, and I will let them have the fun of communicating their results directly to you, rather than trying to interpret what they have done.*
>
> *I am enclosing a preprint with a few minor changes.*

> *I have only one request and that is that you change the reference from the New York Times, to the APS meeting – Monterey, Dec. 28, 1956, and UCRL 3620. The lab is a bit sensitive on this point, since in the case of the antiproton, we received lots of letters from people who thought we held the thing too long before publishing, and from others who said, "Don't you guys publish anywhere except the New York Times?" So in the μ – catalysis, we were careful to follow the book – we presented the thing first at the APS meeting, after sending out preprints. All the news stories came out after the APS talk. – But still, we find a reference to the N.Y. Times!!*
>
> *With many thanks again for a most interesting paper,*
>
> *Sincerely,*
>
> *Luis W. Alvarez*

This was my first contact with Luis Alvarez and soon with members of his group, contacts that have flourished over the years. A not totally frivolous speculation: On what university faculty might I be now if I had not read the New York Times that December day?

The delightful story of the discovery of the muon catalysis events and their explanation is told in Alvarez's 1968 Nobel Lecture [3]. In those early days, there were no professional scanners. The physicists scanned the film themselves. As Alvarez has noted, the physicists' involvement at the scanning level was crucial for the discovery. Casual instructions to scanners about pi-mu decays would not have triggered the selection of the catalysis events for further study, if only because no one dreamt of their existence. In scanning film from the 10-inch hydrogen bubble chamber for strange particle events, the physicists noticed a few anomalously energetic negative muons. The hypothesis of pion decay in flight (just before stopping) had to be rejected when it was found that the muons all had a kinetic energy of 5.4 MeV. The particle physicists needed an astrophysicist colleague to identify the likely source of 5.4 MeV muons, namely, the fusion reaction, $p + d \rightarrow He^3 + \gamma$, with the muon being responsible for the union of the proton and deuteron in a molecular ion and for sometimes carrying off the energy release, instead of the photon. An apparent problem, the paucity of deuterium (1 part in 5000) in the chamber relative to the frequency of fusions, was solved for the Alvarez group by Edward Teller, who, in a bravura performance upon hearing about the results, pointed out that the reduced mass effect made the μd atom about 135 eV more tightly bound than the μp atom, with the consequent high probability of an exothermic charge transfer of the muon from a proton to a deuteron. The recoiling μd atom may travel some distance before forming a pμd molecular ion (thereby causing the gaps sometimes seen), after which fusion can occur.

Neither the physicists in Berkeley or I in Princeton were aware of considerably earlier theoretical work on the subject until after submission of our papers for publication.

3. Early research

As is well known by now, the first published discussion of muon-catalyzed fusion was given by F.C. Frank [4], more than nine years before its experimental observation. A solid state physicist and colleague of Powell at Bristol, Frank examined and rejected a large number of possible alternative explanations of the $\pi - \mu$ decay events discovered by Lattes, Occhialini, and Powell. One of the alternatives was the formation of muonic molecular ions, among them pμd, with its fusion energy release of 5.5 MeV. Frank rejected the mechanism because of insufficient deuterium in the emulsion and the incorrect Q value, but discussed briefly the formation of μp atoms and barrier penetration within the molecule.

The next research on the subject was apparently made in 1948 by Andrei Sakharov. It was the basis of a now legendary* unpublished report [5]. In Sakharov's own words, "Having become acquainted with a paper of Frisch (sic) in which he discussed a possible alternative interpretation of the experiment of Powell, Lattes, and Occhialini (discovery of the π meson) by means of a μ-catalysis reaction, I wrote a report considering the possibility of realizing μ catalysis of a D + D reaction on a macroscopic scale with a positive energy balance, and I made some calculations." [6]

Some years later, but still before the experimental discovery, Zel'dovich considered some aspects of the muon catalysis process [7]. As well as discussing reactions by μp or μd atoms in flight, he treated the vibrational states of the molecular ion, estimating for the dμd ion the lowest and first excited vibrational energies as − 330 eV and − 30 eV. He then said (in translation), "The presence of the oscillation level, real or virtual, with an energy very near to zero, can greatly increase the amplitude of the wave function in the hole, and at the entrance into the barrier ... . Both the probabilities of the reaction in flight and of the reaction of formation of a molecule will be increased." Zel'dovich acknowledges helpful discussions with Kompaneitz, Landau, and Sakharov. It seems clear that theorists in the Soviet Union were fully cognizant of the idea of muon-induced catalysis of fusion reactions and had done some serious thinking about it by the end of 1953.

4. From Berkeley 1957 to Jackson Hole 1984

In Figure 2 are displayed the number of papers <u>published</u> per year on the general subject of muon catalysis of fusion or closely related topics, as listed in Physics Abstracts. Sakharov's unpublished 1948 report is the one exception to the restriction to journal

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*Recently, a physicist visiting the Soviet Union and interested in seeing Sakharov's report, asked a Soviet theorist in the field about it. The reply was that copies were not available, that he had not seen it himself, but that his professor had told him that <u>his</u> professor had indeed seen the report and read it!

publications. Clearly theoretical papers dominate over the years. The experimental papers in the period 1957-59 were concerned with elaborating on the basic discovery, with experiments in the 1960's mostly having the catalysis secondary to the study of the basic weak interaction process of muon capture in hydrogen. The Dubna group of Dzhelepov studied muon catalysis in its own right. A review of the subject was published in 1960 by Zel'dovich and Gershtein [8] and in 1975 by Gershtein and Ponomarev [9].

For almost twenty years the level of interest in the subject remained rather slight. A few theorists made calculations and an occasional experiment was done. Then in the mid 1970's the situation began to change. The Soviet theorists, who had maintained over the years a far greater interest than those elsewhere, began to make highly accurate calculations of the energy states of the various molecular ions consisting of a muon and two hydrogen isotopes. By 1977, Ponomarev and his colleagues had shown [10] that in both $d\mu d$ and $d\mu t$ molecules there were bound excited states with binding energies of less than 2 eV (specifically, $J = 1$, $\nu = 1$ states at $-$ 0.64 eV for $d\mu d$ and $-$ 1.91 eV for $d\mu t$, compared with $\sim -$ 320 eV for $J = 0$, $\nu = 0$). The presence of states with binding energies in the range of <u>electronic</u> molecular energies means that there can be resonant formation of the muonic molecule whereby the thermal neutral $d\mu$ atom, say, enters easily into a T_2 electronic molecule and combines with one of the tritons. The small energy release of the transition into the $J = 1$, $\nu = 1$ state is transferred into excitation of the rotational and vibrational degrees of freedom of the electronic molecule. As Zel'dovich stated in 1954, such circumstances lead to very large cross sections for muonic molecule formation.* The calculated rate was of the order of $10^8 s^{-1}$ (normalized to liquid hydrogen density). In a year or two it was verified experimentally at Dubna.

The certainty that molecular formation rates in some circumstances could be 100 or more times faster than the muon's decay rate is the cause of the recent dramatic increase of interest, reflected in the precipitous rise since 1980 shown in Figure 2. (For 1983 and 1984, the numbers are surely far off-scale, but 1982 is the last year of complete data from Physics Abstracts.) Numerous experimental groups began major programs of study of the many fascinating sub-processes involved (Dubna, Gatchina, Los Alamos, Vancouver, Zurich). The various experimental results now available (temperature dependences, hyperfine effects, etc., etc.) form such a vast and intricate complex that I could not begin to do them justice here. Apart from a few further comments, I must refer the reader to a variety of conference papers and reviews: Ponomarev, 1978 [12], Rafelski, 1979 [13], Breunlich, 1981 [14], Fiorentini, 1981 [15], Bracci and Fiorentini, 1982 [16], Ponomarev, 1983 [17].

The papers in Figure 2 do not include those discussing possible practical uses of muon-catalyzed fusion for energy production. These, too, have shown a resurgence in the past five years. Apart from the original speculations, this aspect lay dormant until the mid 1970's [18] and only caught fire with Petrov's idea of an indirect energy source through use of the 14 MeV neutrons from the $d-t$ reaction to cause fast fission and breed fissile material [19]. By 1983, it was a major topic at the Third International Conference on Emerging Nuclear Energy Systems, Helsinki, Finland, 6-9 June 1983 [20].

A Workshop on Muon-Catalyzed Fusion was held in Jackson Hole, Wyoming on 7 and 8 June 1984. Its purpose was to discuss pressing physics issues, as well as practical applications. A backdrop to the workshop was the impressive series of experiments in $D_2 + T_2$ mixtures at high pressures and temperatures performed by an Idaho-Los Alamos group [21]. They reported as many as 90 ± 10 fusions on the average per muon. This number is impressively close to my early estimate of an upper bound of about 100 $d-t$ fusions per muon (confirmed by later, more elaborate calculations by others). With such

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*In 1967, Vesman [11] had suggested the efficacy of the transfer of energy into vibrations of the electronic molecule for small energy releases, but did not have reliable enough muonic molecular calculations to make truly quantitative statements.

experimental results, it is not easy to prevent speculation that the theorists may be wrong and that many hundreds of fusions per muon might be possible. If so, direct energy production with "cold fusion" might be economical!

The Jackson Hole workshop showed that muon catalysis of fusion is alive and well 27 years after its experimental discovery. On the physics side, there is a rich spectrum of exotic atomic and molecular processes, worthy of study in their own right and with impressive experiments meeting the challenges. Theory is hard pressed to explain all the observed effects. For example, preliminary results from the Idaho-Los Alamos group suggest a marked decrease in the "α-sticking fraction" (the fraction of muons per fusion that end up bound to a thermalized alpha particle) with increased $D_2 - T_2$ density or T_2 concentration. Taken at face value, the data are very difficult to understand (at least by this theorist), although there are reasons for expecting values somewhat less than the 0.9% of the most careful estimates so far. On another front, results from Zurich showed a large, temperature-independent rate of formation of the $d\mu t$ molecule by the μt atom in the $F = 1$ hyperfine state, while theory predicts a temperature-dependent rate a factor of four or more smaller. Clearly, there is work for experimenters and theorists on the basic physics for quite a few years to come.

The practical side looks much less promising, despite the hints of hundred of fusions per muon. *If* one could attain 500 or 1000 fusions per muon in $D_2 - T_2$ (it requires a cyclic rate in excess of $2 \times 10^8 s^{-1}$ and an α-sticking fraction of less than 0.2%), direct energy production would become feasible. There would still be, however, a limitation on the size of plant one could have, given foreseeable accelerator technology – 20 to 50 Megawatts is an upper limit often cited (I find that I stated 10^4 kw in ref. [2].). The Petrov scheme (with, say, less than 200 fusions per muon) does not seem terribly attractive. It is similar to the spallation breeder scheme, where a high intensity proton beam of 1 GeV or more bombards a target of some heavy element, causing spallation reactions with the nuclear evaporation of neutrons which are then absorbed by a suitable

breeding material. In fact, Petrov's idea was for a combined system in which spallation neutrons are produced at the same time as the pions (source of the muons). Modeling of such a scheme indicates that the muon-catalyzed fusion part of the facility would only increase modestly (by less than a factor of two) the number of neutrons available for breeding. That is not a sufficient advantage. Another negative aspect of the Petrov idea is the breeding itself. Muon-catalyzed fusion as a direct, clean, cold source of energy is one thing; as the source of neutrons for making fuel to burn in fission reactors, it is quite another. Finally, breeding of any sort is not sensible while there are cheap supplies of uranium ore adequate for all likely demands. All in all, the prospects of energy production from "cold fusion" seem quite remote.

5. Conclusion

In the 37 years since its first contemplation and the 28 years since its discovery in a hydrogen bubble chamber, muon catalysis of fusion has had a long and interesting history. The toy of a few theorists at first, it became an experimental reality in late 1956 and sparked a flurry of speculation, published and unpublished, on its potential as a new, exciting source of fusion energy. Some studies were made of the process for its own sake, but then it became a complication that needed to be understood in order to get at fundamental processes like $\mu p \rightarrow n\nu$. Theorists, most of them in the Soviet Union, continued an interest and some experiments were done at Dubna. A dramatic awakening of renewed interest occurred in the late 1970's when the theorists predicted large, resonant cross sections for formation of $d\mu d$ and $d\mu t$ molecular ions and they were soon confirmed experimentally. The dreams of a muon-catalyzed fusion power plant were resurrected: numerous experiments to study the rather complicated chains of atomic and molecular interactions and formations were mounted around the world and continue today. Diagrams far more complex than the conceit shown in Figure 3 now abound in the discussion of muonic atoms and molecules and the fusion process.

Luis Alvarez and the colleagues listed in the New York Times story (Figure 1) founded (with their little fingers, one might say) a unique and still flourishing field of experimental science. It is an Alvarez hallmark.

Acknowledgment

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Figure Captions

Figure 1 Reproduction of clipping from the New York Times of 29 December 1956 reporting the announcement of the discovery of muon-catalyzed fusion of hydrogen isotopes at Monterey the previous day.

Figure 2 Histogram of the number of physics journal publications per year on muon-catalyzed fusion or closely related topics from 1945 to the present. The arrow indicates the time of experimental discovery. The shaded rectangles are experimental papers. The dotted rectangle for 1948 represents Sakharov's unpublished report. (Source, Physics Abstracts).

Figure 3 A diagram used at colloquia in 1957 to illustrate the catalytic cycle in a $D_2 - T_2$ mixture.

Atomic Energy Produced By New, Simpler Method

Coast Scientists Achieve Reaction Without Uranium or Intense Heat—Practical Use Hinges on Further Tests

Special to The New York Times.

MONTEREY, Calif., Dec. 28—A third and revolutionary way to produce a nuclear reaction was described here today. It does not involve uranium, as in the fission reaction, or million-degree heat, as in the fusion reaction.

The new process is called "catalyzed nuclear reaction." It was discovered accidentally a few weeks ago during routine work with the huge atom-smashing bevatron at the University of California radiation laboratory.

A team of twelve scientists from the university explained the process to the American Physical Society here. The team was headed by Dr. Luis W. Alvarez, assistant director of the laboratory.

Curiously enough, it was made not at the laboratory at Livermore, where scientists are attempting to control thermonuclear reaction for practical uses, but at the Berkeley laboratory, which is devoted to fundamental research.

Thus far, the new reaction is little more than a laboratory curiosity, the scientists said. The energy it produced came from the fusion of a few hydrogen atoms, they explained, and was scarcely enough to register on highly sensitive measuring instruments.

The process has no commercial value now, though it suggests possible industrial uses of immeasurable importance. It may, scientists said, point a way toward taming the intense heat of the hydrogen bomb to make it useful for peacetime purposes.

Others in the University of California group were Dr. Hugh Bradner, Dr. Frank S. Crawford Jr., Dr. John A. Crawford, Dr. Paul Falk-Vairant, Dr. Myron L. Good, Dr. J. Don Gow, Dr. Arthur H. Rosenfeld, Dr. Frank Solmitz, Dr. M. Lynn Stevenson, Dr. Harold K. Ticho and Dr. Robert D. Tripp.

One method of obtaining nuclear reaction—the so-called "fission reaction" employed in the atom bomb—relies on the bombardment of atomic nuclei with other atomic particles.

The other—the "thermonuclear reaction" of stars and the modern hydrogen bomb—depends upon the union or fusion of two light atomic nuclei to form one heavy nucleus at temperatures of about 1,000,000 degrees.

The type described today employs a medium-weight atomic particle (known as a negative mu-meson) as a catalyst to make a hydrogen nucleus fuse with a deuterium (heavy hydrogen) nucleus. This fusion occurs at low temperatures.

One result is the formation of helium—a variety known as helium-3. Another is the release of prodigious amounts of energy, calculated at about 5,400,000 electron volts for each reaction.

The mu-meson, which triggers this change of elements, is not used up as a catalyst, but remains free to bring together other nuclei of hydrogen and deuterium, and form more helium-3 and produce more energy.

Catalyst Short-Lived

But the catalyst is extremely short-lived, Dr. Alvarez noted, and thus limits the process. The mu-meson has a life of approximately one-millionth of one second, a period sufficient to let it catalyze no more than one or two fusions before it perishes.

In commenting on the future of the new reaction, Dr. Alvarez said:

"If this is to become of practical importance, we would have to find a different catalyzing particle which has properties similar to the mu-meson but has a lifetime of at least ten or twenty minutes."

Such a particle would permit millions of energy-producing reactions and, it may be presumed, the release of enough energy to operate electric generators, motors and other heavy equipment.

In this connection, Dr. Alvarez—who recently traveled through the Soviet Union and visited scientific laboratories there—observed:

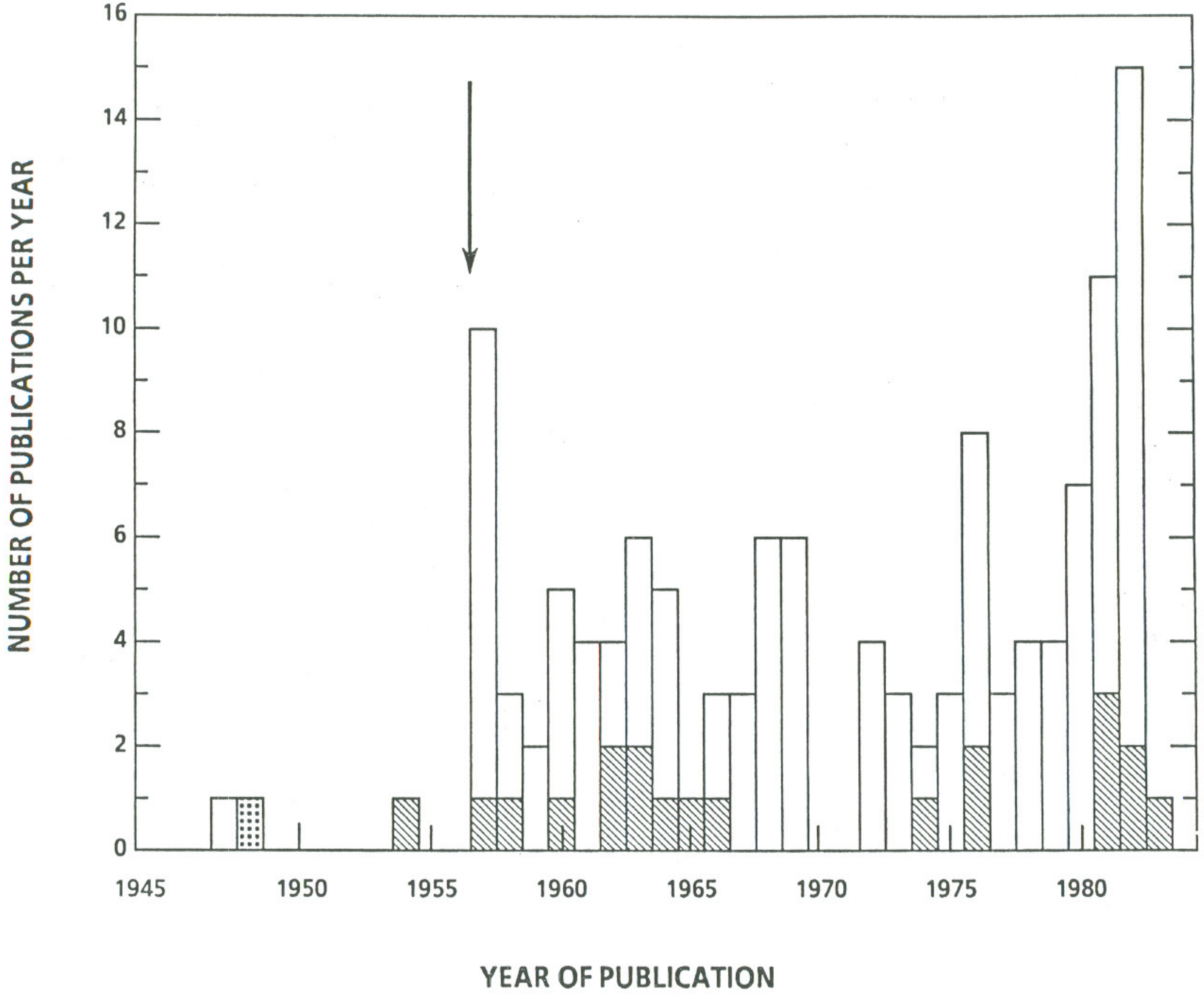
"It is interesting that Russian scientists have reported evidence that such a particle does exist in cosmic rays."

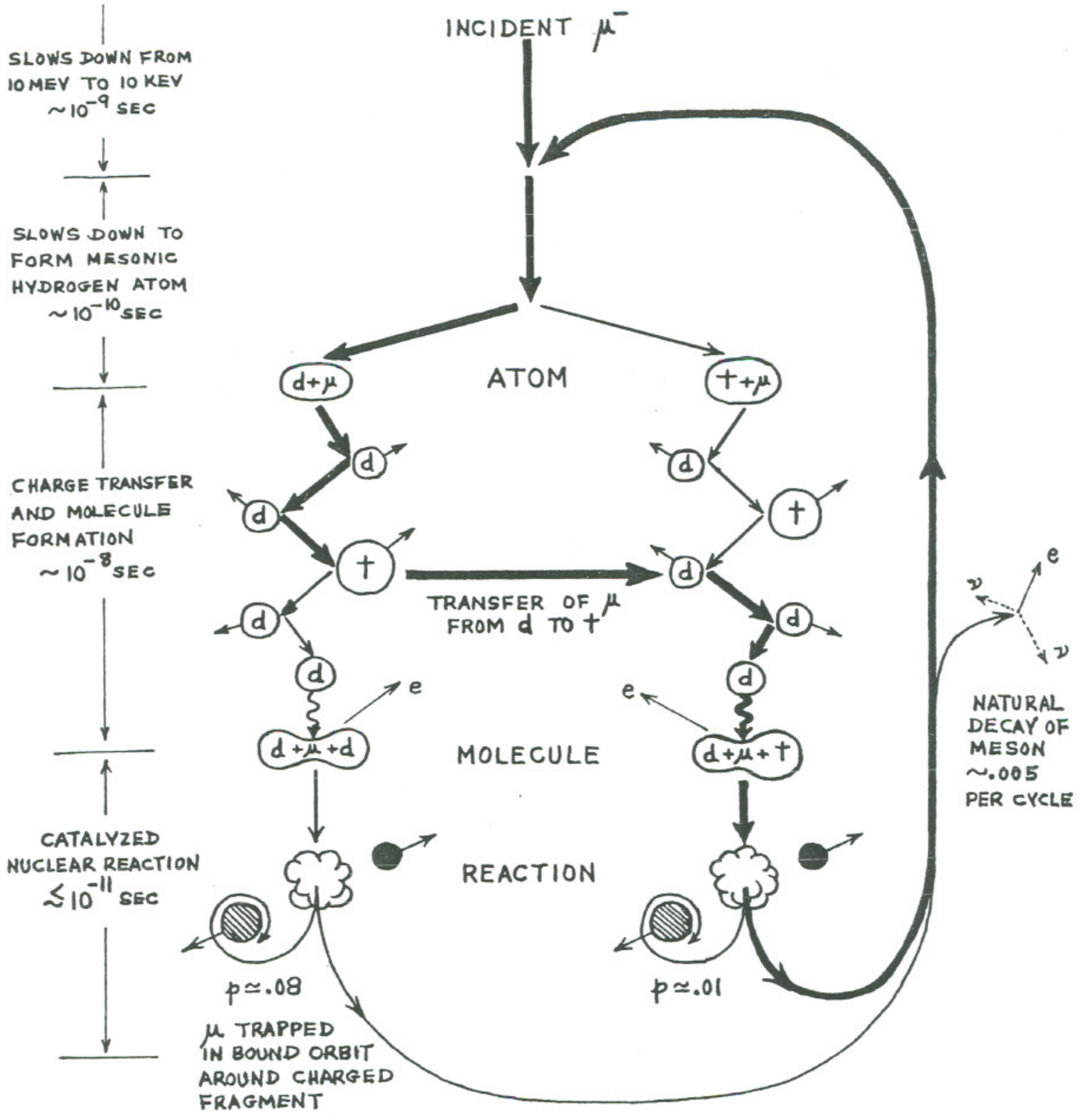
The announcement of the discovery of the "catalyzed nuclear reaction" was made simultaneously by the Atomic Energy Commission in Washington. The commission provides financial support for the fundamental research at the Berkeley Atomic Laboratory.

Published in the New York Times, 29 December 1956, and reproduced with permission from the New York Times, 229 W. 43rd Street, New York, NY 10036.

FIGURE 1

FIGURE 2


16
14
12
10
8
6
4
2
0
NUMBER OF PUBLICATIONS PER YEAR
1945
1950
1955
1960
1965
1970
1975
1980
YEAR OF PUBLICATION



CATALYTIC CYCLE OF NEGATIVE MU MESON IN LIQUID DEUTERIUM – TRITIUM MIXTURE

J.D.J.
1957

FIGURE 3