DOCUMENTATION OF DR. MAHADEVA SRINIVASAN'S COLD FUSION RESEARCH

A PROJECT OF THE LENR RESEARCH DOCUMENTATION INITIATIVE

Volume 1. Third Draft Report, Revision 1

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1 Introduction

Cold fusion (CF) was announced on March 23, 1989, by Dr. Martin Fleischman and Dr. Stanley Pons. The immense potential energy benefits of CF (also referred to as Low Energy Nuclear Reactions, LENR) were immediately recognized. Humankind's need for a source of cheap, clean, inexhaustible, and safe energy seemed to be permanently satisfied. However, LENR was rejected by mainstream science within a year or so, and it remains highly marginalized to this day. On the other hand, the phenomenon has continued to be rigorously pursued by many investigators in several countries. The mounting evidence for the reality of LENR shows that its potential benefits may yet be realized.

Because it is a "pariah" science, LENR has attracted relatively few new investigators to the field. Many of the researchers became active in the early months and years after the 1989 announcement. Now 30 years later many of these investigators are leaving the field. The results of their many years of LENR investigation are at risk of being lost, which would be extremely unfortunate not only for the field, but also potentially for humanity.

An initiative is underway to mitigate the risk of loss of research records of LENR investigators. Its objectives are to collect, organize, document, and archive these records. It is being performed by LENRGY, LLC[1], whose President is Dr. Thomas Grimshaw. The LENR Research Documentation Initiative (LRDI) assists researchers in making sure that their efforts are preserved and kept available for additional analysis and interpretation. The LRDI is described in a recent article in Infinite Energy[2] as well as on a dedicated website[3].

Dr. Mahadeva Srinivasan has been a foremost LENR researcher going back to the 1989 announcement. At the time he was a staff member and manager at India's Bhabba Atomic

[1] LENRGY: LENR Energy – Pursuing the Benefits of Cold Fusion Realization. www.lenrgyllc.com.

[2] Grimshaw, T., 2020. Documenting Cold Fusion Research: Preserving a Vital Asset for Humankind. Infinite Energy, Issue 150, March/April 2020, p. 9-13.

[3] LENR Research Documentation Initiative: Collection, Organization, Description, Archiving of LENR Research Records. www.lenr-documentation.org.

Research Center (BARC), where he had been employed since 1957. He was an experimental physicist working in nuclear science and technology. He made contributions to three U-233 fission research reactors, and he initiated fusion-related experiments, which included the Z-Pinch, Plasma Focus, and 500 MJ Capacitor Bank projects. While at BARC, he received the prestigious D.Sc. (Physics) degree at Bombay University.

When the news of the Fleischmann and Pons announcement arrived, Dr. Srinivasan was assigned by the BARC director to an unofficial coordinating role of cold fusion work at the Center. During the initial period after the 1989 announcement, BARC built up the largest research effort in the field at that time. Dr Srinivasan continued research on LENR at BARC until his retirement in 1997. However, when cold fusion became marginalized by mainstream science, India generally followed suit, particularly when a new BARC director was appointed.

According to Dr. Srinivasan, the main research contributions of BARC to the LENR field, drawing primarily from work with his associates, were in investigation of neutrons, tritium and autoradiography with a focus on confirming the nuclear origin of the phenomenon[4].

- BARC groups were the first to observe and publish the neutron to tritium ratio anomaly. It has been however speculated that neutron emission is possibly a secondary phenomenon wherein energetic tritons released in a primary d-d fusion event knocks off neutrons from the surrounding deuteride matrix.
- Our group was the first and possibly only group to have carried out a statistical analysis of neutron emission, leading to the conclusion that neutron production is not Poisson in nature but rather occurs in bunches of 10 or even as much as 100 neutrons in a single sharp burst.
- BARC groups were the first to use autoradiography as a powerful investigative tool. It has revealed the highly localized nature of tritium production sites on the surface of the host metal samples.

An integrated look at the above observations has led to the suggestion that micronuclear explosions could be occurring.

After he retired, Dr. Srinivasan continued to monitor developments in the field by attending conferences and staying in touch with researchers. Photos of Dr. Srinivasan are in Figure 1-1. A decade after his retirement, he renewed his attempts to revive LENR investigations in India by working with government officials to organize research groups in the field. He also had

[4] From the end of his autobiographical sketch in Section 2.

responsibility for organizing the 16th International Conference on Cold Fusion (ICCF-16), which took place in Chennai in 2011. Furthermore, he edited the papers for the conference proceedings.

Figure 1-1a. Dr. Srinivasan at ICCF-22, September 2019

Figure 1-1b. Dr. Srinivasan (Left) with Dr. Martin Fleischman.
(Possibly at ICCF-16, Chennai, India, 2011)

A project has been undertaken under the umbrella of the LRDI to document Dr. Srinivasan's LENR research record. A principal objective of the Srinivasan LENR Research Documentation Project (SLRDP) is to collect, organize, and present the research papers and other documents prepared for (or related to) Dr. Srinivasan's work in the field. He has also provided an autobiographical summary of his work in the field, and several interviews have been conducted with him (both for this Project and previously) for describing his research.

This report consists of two volumes: #1, Report (third draft), and #2, Copies of Publications. This Volume #1 includes a biographical sketch of Dr. Srinivasan, descriptions of three interviews, a tabulation of his LENR-related documents, potential future SLRDP opportunities and methods used in the project.

2 Autobiographical Sketch

Dr. Srinivasan has prepared a sketch of his LENR research career, mostly in the first person. A brief synopsis of his pre-LENR research is included. His academic background is summarized as follows:

- B.Sc (Physics), University of Madras (1955)
- B.Sc (Technology), University of Madras (1957)
- Postgraduate Training School at BARC, Trombay, Mumbai (One Year, 1957-58)
- M.Sc (Physics) by Research, University of Bombay (1966)
- D.Sc (Physics), University of Bombay (1984)

The six phases of Dr. Srinivasan's LENR and other nuclear research are described below, followed by a summary of BARC contributions to the LENR field.

2.1 Phase I . 1958 to 1988: Pre Cold Fusion Era

The first 30 years of my research career at BARC were as an experimental physicist working in the broad area of nuclear science and technology. A timeline is shown below.

- August 1958: On graduating from the BARC Training School I was assigned to the Nuclear Physics Division as a Scientific Officer
- 1961-62: I was deputed to the International Institute of Nuclear Science & Engineering at the Argonne National Laboratory in Illinois, USA, as a member of the newly constituted Fast Reactors Team.
- 1963 – 1969: Reactor Engineering Division of BARC. Head of the Experimental Reactor Physics (ERP) Section, which was in charge of experimental studies at Zero Energy Reactor Zerlina
- 1968-69: On Sabbatical at the Chalk River National laboratories (CRNL) of Atomic Energy of Canada (AECL) Canada
- 1970-74: Leading researcher of Pulsed Fast Reactor (PFR) Project. Designed, constructed and commissioned Plutonium Fuelled PURNIMA Small Fast Reactor
- 1974: Appointed Head Neutron Physics Division
- 1975-1988:
    - U-233 fuelled research reactors (Purnima II, Purnima III and Kamini reactors)
    - Initiated fusion-related experiments, such as Z-Pinch, Plasma Focus, 500 MJ Capacitor Bank project
- 1984 : Awarded the prestigious D.Sc. (Physics) degree by Bombay University based on my research publications in the area of "Fission Chain Reactions and Fusioning Plasmas".

A notable contribution was my development of the "Trombay Criticality Formula" (TCF) through a series of a dozen papers published in mainstream journals such as Nuclear Science & Engineering published by the American Nuclear Society.

2.2 Phase II. March 1989 to February 1997: Cold Fusion Era Prior to My Retirement

When the cold fusion announcement came on March 23, 1989, I was, so to say, "at the right place at the right time". I was head of the Neutron Physics Division of BARC, and our Division was already engaged in fusion related experiments. As I reported to the then Director of BARC, Dr. P.K. Iyengar, on all fusion research related matters, a sort of coordinating role for the BARC cold fusion work was unofficially assigned to me.

The story of how BARC rose to the occasion and built up the world's biggest cold fusion effort at that time has been chronicled in my interview given to Russ George. It was published in the second issue of "Cold Fusion" magazine in 1994 (see Appendix A[5]), which incidentally carried my photograph on the cover page. (It's a different matter that the printed picture turned out to be a mirror reflected version!)

As mentioned in that interview it was remarkable that under the inspiring leadership of Dr. Iyengar, not only did 12 groups drawn from many different divisions independently set up electrolysis experiments using whatever Pd samples they could lay their hands on, but also they all confirmed observing sporadic emission of neutrons and tritium in Pd-D2O experiments. BARC groups were the first to report observing the so-called branching ratio anomaly, namely that tritium production was a million times more prolific than neutron emission. This was in contrast to the well-known result that in d-d fusion the neutron and tritium branches have about equal probability.

(In later years Prof. Bockris of Texas A & M University and I had several friendly email exchanges as to who was the first to publish observing this anomaly – their group or the BARC groups. Eventually he conceded that BARC may have been the first to publish it in July 1989 at a conference in Germany, but he claimed they were the first to observe it experimentally although their result was published only later!)

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[5] See also #13 in the tabulation of Dr. Srinivasan's publications.

It may be noted that most of the papers published by me during 1989 to 1990 were a collaborative effort by many researchers from various divisions of BARC. Since I ended up writing review papers of this work and presenting them in international conferences, an impression may have been given that it was my individual research effort. I would like to take this opportunity to correct that erroneous impression.

The cold fusion work at BARC was referenced in two early books on the topic – "Fire from Ice"[6] and "Cold Fusion - The Scientific Fiasco of the Century"[7]. Copies of the relevant pages in these books are in Appendices C and D. In 1995 Dr. Srinivasan received a memorandum from Jed Rothwell (Appendix E) indicating a possible interview by the news network, CNN. An article that referenced BARC's cold fusion work appeared in a Russian magazine, "Est Idea", in 1986 (Appendix F).

Again as I have clearly elaborated on in my interview to Russ George it was extremely unfortunate that the person who took over as the new Director of BARC (let's call him RC) in 1991 – when Dr. Iyengar was promoted as Chairman of the Indian Atomic Energy Commission and moved out of BARC – took the opposite view of cold fusion. By then the U.S. DOE committee report[8] was out, and worldwide the majority of scientists decided to follow the point of view that cold fusion has been shown to be a nonstarter. RC was advised by his well wishers in the U.S. – and some even from India – that as he had just assumed charge as Director, he should save the good name of BARC and prevent it from getting tainted by supporting the discredited field of cold fusion.

I will never forget the incident when RC marched into my office at BARC within hours of taking over as Director of BARC and challenged me for using the words "BARC Studies in Cold Fusion". I was admonished for having thereby tainted the good name of BARC! I was informed that with immediate effect he was withdrawing institutional support for cold fusion. I could as a Senior Scientist of the Center use whatever discretionary funds I am eligible for as a Division Head and

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6 Mallove, E.F., Fire from Ice – Searching for the Truth Behind the Cold Fusion Furor. New York, John Wiley & Sons, 1991.

7 Huizenga, J.R., Cold Fusion – The Scientific Fiasco of the Century. Oxford University Press, 1993.

8 U.S. Department of Energy, Energy Research Advisory Board, 1989. "Final Report of Cold Fusion Panel of the Energy Research Advisory Board." Unpublished U.S. DOE Report, 61 p. November.

continue conducting cold fusion experiments and thereby "make a fool of myself". However, such research work would not be included in official progress reports of BARC research.

For the next 7 years from 1991 to 1996 (until my retirement in 1997), RC and I were at loggerheads on the validity and importance of cold fusion. Out of the dozen or so groups who had been studying the phenomenon, most shut down cold fusion research. However, my immediate colleagues supported me and we continued. A couple of groups in other divisions also defied RCs dictum and persisted. But all such defiant persons paid a personal price in that their promotions were delayed and at times even denied. In my case my nomination as a fellow of the Indian National Science Academy (INSA) was rejected in spite of my case being sponsored by two former BARC Directors and the then Chairman of AEC. The then-President of INSA, Prof. C.N.R. Rao, commented: "Srinivasan has done very creditable work in the area of nuclear science and technology, but he believes in cold fusion! How can we accept him as a fellow of INSA?"

During this phase, Dr. Srinivasan periodically sent documents to BARC senior managers regarding cold fusion research. Appendix G includes "Whither Cold Fusion", which was prepared in 1991. "Paradigm Shifts Which Can Drastically Affect Our Extrapolations/Projections", which Dr. Srinivasan authored in 1995, as in Appendix H.

2.3 Phase III. March 1997 to Nov 2007: "Silent Decade" following Retirement from BARC

By 1997 when I retired, all cold fusion research at BARC and in all units of the Department of Atomic Energy had been wound up. Although there was no official order to that effect, the message sent out was loud and clear. And in fact in the whole of India the perception spread that cold fusion was a big mistake and has been shown to be fully disproven.

RC went onto become Chairman of the AEC and in 1998 was responsible for overseeing the so-called Pokhran II series of nuclear explosions. This brought him very close to the then Prime Minister Atal Behari Vajpayee who shortly thereafter appointed him as the Principal Scientific Adviser (PSA) to the Government of India.

I am going through all this only to point out how – with a staunch nonbeliever and intense critic of cold fusion at the helm of affairs pertaining to all scientific activities in India – there was little chance of cold fusion ever raising its head in India again. RC continued to occupy the PSA chair in Delhi for the next 20 years (until early 2018), wielding his powerful anti-cold fusion wand and ready to quash any attempts to revive research even in the remotest corners of India at the slightest rumors of anyone talking in favor of cold fusion!

No wonder I had to give up any hope whatsoever of reviving cold fusion research in India, although at a personal level I continued attending all ICCF conferences and keeping myself abreast of progress in the field. I was thus totally silent on cold fusion for ten years after retirement from BARC.

2.4 Phase IV. 2008 -2018: Fresh Attempts to Revive CF/LENR Research in India

In October 2007 I attended ICCF-13 in Sochi, Russia. The concluding session was very upbeat and optimistic. For the first time speakers were talking of an imminent commercial era for cold fusion. On returning home I therefore felt that I must do something to revive cold fusion research in India. I had noted that many of my former colleagues who were now holding senior positions in the Department of Atomic Energy sympathized with me that such forceful killing of innovative research was not the way for science to progress. They agreed that young scientists should be allowed to explore new areas and publish their findings even if they may be negative. But they themselves expressed inability or reluctance to upset the ultimate boss.

In early 2008 I managed to convince the then Director of the National Institute of Advanced Studies (NIAS) in Bangalore to hold a one day brainstorming session on the topic of "Energy Concepts for the 21st Century". I invited Dr. Michael McKubre and Steven Krivit to come for the meeting. Ed Storms expressed inability to come due the health reasons. Travel support for these people was made available by a private University group. I was happy that a former Chairman of the Indian Atomic Energy Commission, Dr. M.R. Srinivasan[9], could participate in this meeting and sit through the whole day. A report on that meeting was later published in the journal "Current Science"[10].

[9] Not a relative of the author of this report.

[10] See #23 in the tabulation of Dr. Srinivasan's publication.

In October 2009, at ICCF-15 held in Rome, I accepted the responsibility for the ICCF-16 Conference in Chennai knowing very well that I would have no support from scientific organizations in India. On paper ICCF-16 was organized under the banner of the "Indian Physics Association". I pulled this off as the President of IPA that year, with help from my friend Dr. Bikas Sinha, a Nuclear Physicist and Director of the Variable Energy Cyclotron Centre (VECC) of Kolkata. He had been a long term believer in cold fusion. Likewise through personal contacts I managed to get the "Indian Nuclear Society" to agree to lend its name as a sponsor of ICCF-16 conference.

But the main hitch was getting government approvals. When I proudly announced to Dr. Iyengar that I have accepted the responsibility to conduct the ICCF-16 conference in Chennai in February 2011, it was he who alerted me that to conduct an international conference in India, one needs to get several Ministerial clearances, such as Ministry of External Affairs and Ministry of Home Affairs (clearance from the Ministry was concerned with the subject matter of the conference), as well as police clearances from the local state government authorities. I had not bargained for all this! Anyway, Dr. Iyengar used his contacts in the national government in Delhi to help me get all these clearances although it took me several visits to Delhi and almost a year to get over these bureaucratic hurdles.

I roped in a former colleague of mine, Dr. L.V. Krishnan, formerly of the Indira Gandhi Centre for Atomic Research (IGCAR), to serve as co-convener. We outsourced secretarial work to a private human resources firm run by a childhood friend of mine. We booked the GRT Grand Hotel in Chennai, which had a large lecture hall and excellent catering facilities.

Along with the main ICCF-16 conference, we also organized one pre-conference, a Tutorial School on LENR at IIT Madras, and two post-conference satellite meetings. One was on "Materials Science Aspects of Hydrogen Loaded Metals" at the tourist resort of Mahabalipuram near the IGCAR campus. It was held jointly with the materials specialists of IGCAR. The second one-day satellite meeting was on the topic of "Biological Transmutation", which was held at SRM University at a distant suburb of Chennai[11].

[11] See #35 in the tabulation of Dr. Srinivasan's publications.

The other challenge was the question of financial support to conduct the conference. I managed to get funding, mainly using personal contacts, from a few government agencies such as the Department of Science and Technology (DST), Defense Research and Development Organization (DRDO), and Atomic Energy Regulatory Board (AERB). It was within their financial powers to sanction funds up to 100,000 Rupees each without going to higher authorities.

But all these amounts were insufficient to cover the expected expenses. We had computed the registration fees assuming that a certain minimum number of foreign participants would register for the meeting. But if the number of foreign participants fell short of the budgeted number, I would have to shell out the shortfall from my pocket. I had already kept some term deposits ready to cash if the need arose. Luckily for me, a group of five South Korean delegates unexpectedly registered in the last minute, which really helped turn the budget corner and avoid my having to use my personal funds. After the conference, I edited the papers for the Proceedings[12].

In a sense, at the end of 2011 I could heave a sigh of relief following all these "accomplishments". Unfortunately, however, neither the one-day NIAS workshop of 2008 nor the ICCF-16 conference of 2011 seemed to have had any effect at ground level. No LENR research programs actually came to fruition in India even though there were many new sympathizers for the subject of cold fusion.

2.5 Phase V. 2011 to 2016: The Emergence of the Ni-H Based Rossi Reactor and Its Impact

Three weeks prior to the commencement of ICCF-16 Conference, namely on January 14, 2011, Andrea Rossi gave a public demonstration of his 10-Kwth Ni-H reactor. Ever since publication of Randell Mills' paper in Fusion Technology in 1991 that excess heat was observed in a simple nickel cathode and light water solution electrolysis experiment, I had been interested in the potential possibilities of Ni-H systems (as against Pd-D systems). Thus our ICCF-3 paper (1993)[13] had in fact been based on our studies on Ni-H electrolytic cells. So when Rossi revealed details of his Ni-H reactor I was quite excited. The cold fusion community is well aware of the impact Rossi has had on the course of LENR history, although not all of it of course is pleasant. In any case in February 2011 we had to quickly change the program of the inaugural session of the ICCF-16

[12] See #40 in the tabulation of Dr. Srinivasan's publications.

[13] See #11 in the tabulation of Dr. Srinivasan's publications.

conference to accommodate a couple of presentations and discussions on the new development of the Rossi Reactor.

In October 2011, Rossi gave a demonstration of his 1-Mwth reactor that was very impressive. Later we learned that a private company, namely Industrial Heat, had signed up to acquire Rossi's reactor technology for a $100M. Then came the independent third-party test on a nickel nanopowder and hydrogen gas loaded system, which was conducted at the Swiss border town of Lugano. The Lugano report gave the clear impression that the Rossi reactor had been independently validated. On top of all this, release of the book "An Impossible Invention" by science journalist Mats Lewan of Sweden in December 2014 convinced me that Rossi's reactor was a reality.

The announcement in early 2015 that Parkhamov of Russia had successfully replicated the Rossi Reactor reinforced my belief that LENR had turned the corner. Hence most of the articles written by me during the period 2011 to 2015 dealt with the emergence of the Rossi Reactor as a very important milestone in the development of LENR as real technology.

All the above events helped me to convince the Chief Editor of the Current Science Journal that the time was ripe for the journal to bring out a special issue on LENR. I invited Dr. Andrew Meulenberg, who was residing in Bangalore at that time, to serve as Co-guest Editor for this special issue. During the next few months (latter part of 2014), Meulenberg and I approached our cold fusion friends worldwide and invited them to contribute articles for this special issue. It eventually appeared as its February 15, 2015 issue. David Nagel contributed an excellent article on the Rossi Reactor.

During the period 2012 to 2015 I undertook intense lobbying with at least three multinational corporations of India involved in power generation to be prepared to take up manufacture of LENR reactors. On the other side I persuaded Industrial Heat and Defkalion Green Technologies that Indian companies would be excellent partners for them to take up manufacture and marketing of LENR-based decentralized captive power plants.

Those of us who thought that LENR had turned the corner were therefore rather disappointed that the year-long third party test of Rossi's 1-Mwth reactor during 2015-16 turned out to be a "damp squib" since the test report prepared by the independent neutral evaluator was not accepted by

Industrial Heat. We are all aware of the court dispute that ensued between Industrial Heat and Rossi, which was eventually settled out of court. The information available in the public domain since then does give the impression that Rossi had taken recourse to fraudulent means to try to cheat Industrial Heat during the year-long test.

2.6 Phase VI. 2014 -2018 : Appeal to the Politicians in Power for Intervention and Conequent LENR-India Forum Meetings of 2015 & 2016 at NIAS

As of early 2014 there was still no sign of any group showing interest in getting involved in LENR research in India. In mid-2014 an international mini-conference was organized on materials aspects of Nb-Ta alloys used in superconducting cavities for particle accelerators. It was held at the sVyasa Yoga University, located in the outskirts of Bangalore in southern India. It was thoughtful of Dr. Srikumar Banerjee, a former Director of BARC and later a Chairman the Indian AEC, to suggest that a session on cold fusion be included in that mini-conference. I was invited to organize that session. I took the opportunity to impress upon the host, Dr. H.R. Nagendra, Vice Chancellor of sVyasa University, on the importance of convincing the powers-that-be at Delhi that cold fusion research needs to be supported by the Government of India.

Dr. Nagendra happened to be the Yoga teacher to Mr. Narendra Modi, then the Chief Minister of Gujarat State and generally projected as the leading contender for the post of Prime Minister of India when the BJP party won the forthcoming parliamentary elections in May 2014. Nagendra was convinced of the importance of LENR and so, soon after Modi was sworn in as Prime Minister, he arranged for a meeting with the PM at Delhi sometime in July 2014. Although Nagendra and I showed up in Delhi on schedule, that meeting did not take place as the PM was stuck in the Parliament. We returned empty-handed that night.

However, a few months later, in November 2014, a similar meeting was arranged with the Minister for Power ,Mr. Piyush Goyal, which was very productive. This minister immediately ordered the Chief Secretary in the Ministry of New and Renewable Energy (MNRE) to release funds to the extent of half a million Rupees to conduct a brainstorming session at the LENR-India Forum at the National Institute of Advanced Studies (NIAS) in Bangalore.

At that time Dr. Baldev Raj a former Director of the Indira Gandhi Centre of Atomic Research (IGCAR) had just taken over as Director of NIAS. He was a very distinguished materials scientist in India. His consent to lead the efforts to revive LENR research in India has played a very crucial role in the recent Indian LENR story. Over 15 heads of leading scientific laboratories attended that meeting. A second such meeting was again held in NIAS in March 2016, this time with funding from the National Thermal Power Corporation (NTPC), which is India's largest public sector power producing corporation. This time working level scientists who would themselves undertake LENR research were invited to attend the meeting.

By the end of the meeting almost a dozen potential team leaders for initiating LENR research in their laboratories pledged to initiate experimental programs. But the challenge was to arrange for funding for these new research groups. I was dependent on Dr. Baldev Raj, who was a member of the Scientific Advisory Committee to the Cabinet, to use his good offices to arm twist various govenrment funding agencies to open up their pursues. As of 2018 four research groups are currently active in India – Indian Inst. of Technology, Bombay (IITB), IIT Kanpur, sVyasa University at Bangalore, and even one group at BARC, Mumbai.

So to conclude the long story I have reason to be somewhat satisfied that at long last the stigma against cold fusion has more or less been removed in India and we now have at least four research groups engaged in LENR research. All the groups are attempting to replicate the Parkhamov type Ni nanopowder and H2 gas loaded configurations operating at over 1000° C. Unfortunately I am sad to note that my good friend Dr. Baldev Raj, who was such a source of strength for me, passed away in early 2018 leaving me orphaned in my decades-long battle to revive CF research in India!

2.7 Summary of the Main Research Contributions of BARC to the LENR Field

In summary, the main research contributions of BARC to the LENR field, drawing primarily from the work of my groups, was in investigation of neutrons, tritium and autoradiography with a focus on confirming the nuclear origin of the phenomenon.

- BARC groups were the first to observe and publish the neutron to tritium ratio anomaly. It has been however speculated that neutron emission is possibly a secondary phenomenon wherein energetic tritons released in a primary d-d fusion event knocks off neutrons from the surrounding deuteride matrix.
- Our group was the first and possibly only group to have carried out a statistical analysis of neutron emission, leading to the conclusion that neutron production is not Poisson in nature but rather occurs in bunches of 10 or even as much as 100 neutrons in a single sharp burst.

- BARC groups were the first to use autoradiography as a powerful investigative tool. It has revealed the highly localized nature of tritium production sites on the surface of the host metal samples.

An integrated look at the above observations has led to the suggestion that micronuclear explosions could be occurring. This speculation has been addressed in our papers over the years.

1992-1995 – I started investigating Ni-H systems.

- It was by now clear that neutrons and tritium production were only a small side show in the cold fusion phenomenon. Clearly helium was the main product in excess heat producing electrolytic cells. But helium measurements are a specialized job. Also we felt Ni-H2O devices would be a more economical approach. Hence we started concentrating on Ni-H systems. Our primary papers at ICCF-3 and ICCF-4 were in Ni-H2O electrolytic cells. However we made a mistake in claiming to have observed excess heat in open light water cells in our Nagoya ICCF-3 paper[14]. The two balance experiments I conducted at SRI International during a 6 month sabbatical clearly showed apparent excess heat was due to H2 & O2 recombination effects[15].
- While production of tritium in deuterium-based configurations is quite expected, observation of tritium in Ni-H systems, both in electrolysis and gas loaded experiments is a surprising finding. However the mechanism of tritium production in hydrogen based systems remains unknown.

My growing interest in transmutation studies from 2008 onwards…

- I was always intrigued by reports of observation of elemental transmutations in LENR configurations, implying occurrence of alchemical processes, especially in biological (Kervran) and microbial (Vysotskii) transmutations. The fist experimental findings occurred when a group at BARC conducted the carbon arc experiment and confirmed observing anomalous production of iron. This was simultaneously and independently confirmed by Bockris' group at Texas A & M University in 1993.
- Transmutation reactions have been postulated as possible source of heat release in a variety of Ni–H systems. Miley's group was the first to report finding a large number of transmutation products in Patterson-type electrolytic cells in 1995. However his attempts to quantitatively correlate the quantum of transmutation products with quantum of excess power produced were not satisfactory.
- Attempts were also made in the Lugano experiment[16] to suggest that transmutation products could explain their excess heat observations.

[14] See #11 in the tabulation of Dr. Srinivasan's publications.

[15] See #14 and #16 in the tabulation of Dr. Srinivasan's publications.

[16] Giuseppe Levi, Giuseppe, Evelyn Foschi, and Hanno Essén, 2104. Observation of Abundant Heat Production from a Reactor Device and of Isotopic Changes in the Fuel. Online. Available: http://amsacta.unibo.it/4084/1/LuganoReportSubmit.pdf.

- On the whole the nature of the nuclear reaction responsible for heat production in Ni-H systems remains unresolved. Although Godes of Brillouin Energy Corporation[17] claims it is due to fusion reactions, but this viewpoint has not been experimentally confirmed,
- Under the circumstance the massive quantities (tons per day) of iron and silicon production in Mr. Narayanswamy's industrial plant[18] without the expected concomitant energy release has thrown open very fundamental questions: Where did the missing energy go? What is the origin of heat production in Ni-H systems? Can transmutations occur not accompanied by heat release?
- This question has relevance to related issues in alchemical processes as also biological transmutation where too transmutation seems to be occurring not accompanied by the expected heat release.

It is hoped that the above remarks would throw some light on the rationale for the various papers published by me in the post-retirement "silent decade" period following 2008.

3 Interviews

Dr. Srinivasan's LENR research career as set forth in his autobiographical sketch is amplified with additional detail in three interviews (Appendix B):

- 2019, with Dr. Grimshaw at ICCF-22 in Assisi, Italy (Appendix B1)
- 2011, with Marianne Macy for Infinite Energy magazine (Issue 95) in conjunction with Dr. Srinivasan's leadership for ICCF-16 (Appendix B2)
- 1994, with Russ George for Cold Fusion magazine, accomplished while Dr. Srinivasan was researching LENR with Dr. McKubre at SRI International (Appendix B3)

Transcripts of these interviews are included in Appendix B. Gratitude is expressed to Marianne Macy and Russ George for their permission to include the interview transcripts in this report.

[17] Robert Godes, President and Chief Technology Officer, Brillouin Corporation (http://brillouinenergy.com/).

[18] See #45 in the tabulation of Dr. Srinivasan's publications.

4 Publications and Reports

During his nearly 30 years of LENR research, Dr. Srinivasan has prepared (or been a party to) at least 45 papers and related documents related to the phenomenon. They were developed generally from 1989 to 1996 and from 2008 to 2017, with a 10-year "gap" following his retirement from BARC in 1997.

4.1 List of Documents

Table 4-1 provides a list of the publications and reports by Dr. Srinivasan identified in the SLRDP. Columns 2 and 3 are from a list that he prepared. Columns 4 and 5 present the titles of the publications and the names of the additional files located by Dr. Srinivasan. Copies of the documents are included in Volume 2 of this report and are on the SLRDP Dropbox folder as described in Section 4.2.

Table 4-1
LENR Documents by Dr. Srinivasan

No	Date	Description	Title	File Name
1	1989-0	Iyengar, P.K. Cold Fusion Results in BARC Experiments. in 5th International Conf. on Emerging Nucl. Energy Ststems. 1989. Karlsruhe, Germany.	Iyengar, P.K., 1989. Cold Fusion Results in BARC Experiments. in Fifth International Conf. on Emerging Nucl. Energy Systems. Karlsruhe, Germany.	IyengarPKcoldfusion.pdf
2	1989-1	BARC Studies in Cold Fusion - Report BARC-1500 - Compilation of papers of work done by Researchers during 1989	Iyengar, P.K. and M. Srinivasan, eds., 1989. BARC Studies in Cold Fusion, April – September. Government of India, Atomic Energy Commission, Bhabha Atomic Research Centre, Trombay, Bombay 153 pages, December.	BARC-1500 from New Energy Times.pdf
3	1989-2	Rout's Autoradiography Paper in BARC 1500 Report	Rout, R.K., M. Srinivasan, and A. Shyam, 1989. Autoradiography of Deuterated Ti and Pd Targets for Spatially Resolved Detection of Tritium Produced by Cold Fusion, in BARC Studies in Cold Fusion, P.K. Iyengar and M. Srinivasan, Editors, Atomic Energy Commission: Bombay. p. B 3.	RoutRKautoradiog.pdf
4	1990-1	ICCF-1 (Salt Lake City) Overview paper	Iyengar, P.K. and M. Srinivasan, 1990. Overview of BARC Studies in Cold Fusion. in The First Annual Conference on Cold Fusion. University of Utah	IyengarPKoverviewof.pdf

			Research Park, Salt Lake City, Utah: National Cold Fusion Institute.	
5	1990-2	ICCF-1 (Salt Lake City) Statistical Analysis	Srinivasan, M., et al., 1990. Statistical Analysis of Neutron Emissions in Cold Fusion Experiments. The First Annual Conference on Cold Fusion, Salt Lake City, Utah.	My paper ICCF 1 Stat. Analysis (1990).pdf
6	1990-3	Fusion Technology Paper with 50 Authors (Fusion Technol Aug 1990)	Iyengar, P.K., and M. Srinivasan, 1990. Summary. In Bhabha Atomic Research Centre Studies in Cold Fusion. Fusion Technology, Vol 18, no. 1, p. 34.	BARC 1990 Iyengar Srinivasan.pdf
7	1990-4	Kaushik et al, Chips IJT Dec 90	Srinivasan, M. et al., 1990. Preliminary Report on Direct Measurement of Tritium in Liquid Nitrogen Treated TiD Chips, Indian Journal of Technology, Vol. 28, p. 667-673. December.	BARC Paper Kaushik Chips IJT 90.PDF
8	1990-5	Provo Paper	Srinivasan, M., et al., 1990. Observation of Tritium in Gas/Plasma Loaded Titanium Samples. in Anomalous Nuclear Effects in Deuterium/Solid Systems, "AIP Conference Proceedings 228". Brigham Young Univ., Provo, UT: American Institute of Physics, New York.	My Paper Provo (1990).pdf
9	1991-1	My Current Science Review Paper (April 1991)	Srinivasan, M., 1991. Nuclear Fusion in an Atomic Lattice: an Update on the International Status of Cold Fusion Research. Current Science, Vol. 60, p. 417.	My Paper Current Science (April 1991).pdf
10	1991-2	Rout's Fusion Technology Paper on Plasma Focus Electrode	Rout, R.K., et al.,1991. Detection of high tritium activity on the central titanium electrode of a plasma focus device. Fusion Technol. Vol. 19, p. 391.	RoutRKdetectiono.pdf
11	1993-1	ICCF 3 (Nagoya) Ni-H Electrolysis paper	Srinivasan, M., et al. 1993. Tritium and Excess Heat Generation during Electrolysis of Aqueous Solutions of Alkali Salts with Nickel Cathode. Proceedings of the Third International Conference on Cold Fusion. Frontiers of Cold Fusion, p. 123-138.	1993-01 ICCF 3 paper Srinivasan_et.pdf
12	1993-2	ICCF 3 (Nagoya) Rout Paper	Rout, R.K., et al., 1992. Phenomenon of Low Energy Emission from Hydrogen/Deuterium Loaded Palladium. in Third International Conference on Cold Fusion, "Frontiers of Cold Fusion". Nagoya Japan: Universal Academy Press, Inc., Tokyo, Japan.	Rout ICCF 3 Paper.pdf
13	1994-1	Interview Conducted by Russ George for CF Magazine	George, Russ, 1994. The Cold Fusion Phenomenon Is Real — An Interview with Dr. Mahadeva Srinivasan Conducted by Russ George, Informal Manuscript.	GeorgeRthecoldfus(1).pdf
14	1994-2	Excess Heat Measurements at SRI in Ni-H…trolytic Cells.htm	Srinivasan, M., and M.C.H. McKubre, 1994. Two-Balance Method of Faraday Efficiency Measurement with External	My paper Excess Heat Measurements at SRI in Ni-H2O Electrolytic

			Open Cell Calorimetry for Identifying Origin of Excess Heat Ni-H2O Electrolytic Cells. Cold Fusion Magazine, Vol. 1, May. Reprinted in Steven Krivit, 1994. Review of Excess Heat in Ni-H2O Electrolytic Cells.	Cells.htm
15	1994-3	ICCF-4 (Maui) Paper, Ramamurthy et al	Ramamurthy, H., M. Srinivasan, U.K. Mukherjee, and P. Adihabu, 1994. Further Studies on Excess Heat Generation in H2O-Ni Electrolysis Cells. Informal Manuscript. Presented at Fourth International Conference on Cold Fusion.	My Paper (Ramurphy) ICCF 4 (1994).pdf
16	1994-4	SRI Two Balance Results (Cold Fusion Magazine)	Srinivasan, M., and M.C.H. McKubre, 1994. Two-Balance Method of Faraday Efficiency Measurement with External Open Cell Calorimetry for Identifying Origin of Excess Heat Ni-H2O Electrolytic Cells. Cold Fusion Magazine, Vol. 1, May. Reprinted in Steven Krivit, 1994. Review of Excess Heat in Ni-H2O Electrolytic Cells. Online. Available: http://newenergytimes.com/v2/news/2011/36/3620review.shtml.	My Paper Ni-H excess heat SRI Two Balance Results (1994).pdf
17	1995-1	ICCF-5 (Monaco) Paper Ni-H Self Heated wire (TKS et al)	Sankaranarayanan, T.K., et al., 1995. Evidence for Tritium Generation in Self-Heated Nickel Wires Subjected to Hydrogen Gas Absorption/Desorption Cycles. Fifth International Conference on Cold Fusion. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France.	BARC Paper TKS et al Ni-H Self Heated Wires (ICCF 5).pdf
18	1995-2	Shrikhande, V.K., et al. Preliminary Results on the Variation of Electrical Resistance of TiDx Wire With Deuterium Concentration. in 5th International Conference on Cold Fusion. 1995. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France. P. 465	Shrikhande, V.K., et al., 1995. Preliminary Results on the Variation of Electrical Resistance of TiDx Wire With Deuterium Concentration. Fifth International Conference on Cold Fusion. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France. p. 465.	Shrikhande ICCF-5.pdf
19	1995-3	Shyam, A., et al. Observation of High Multiplicity Bursts of Neutrons During Electrolysis of Heavy Water with Palladium Cathode Using the Dead-Time Filtering Technique. in 5th International Conference on Cold	Shyam, A., et al., 1995. Observation of High Multiplicity Bursts of Neutrons During Electrolysis of Heavy Water with Palladium Cathode Using the Dead-Time Filtering Technique. 5th International Conference on Cold Fusion. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France. p.181.	1995-3 ICCF 5 Shyam et al.pdf

		Fusion. 1995. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France. P.181		
20	1995-4	A.B. Garg, R.K. Rout, M Srinivasan, T.K.Sankaranarayanan, A. Shyam, L. V. Kulkarni Protocol For Controlled and Rapid Loadingl Unloading of H2/D2 Gas in Self-Heated Pd Wires To Trigger Nuclear Events. ICCF 5 Proc. P.461.	Garg, A.B., R.K. Rout, M. Srinivasan, T.K. Sankaranarayanan, A. Shyam, L. V. Kulkarni, 1995. Protocol For Controlled and Rapid Loadingl Unloading of H2/D2 Gas in Self-Heated Pd Wires To Trigger Nuclear Events. Proceedings of the Fifth International Conference on Cold Fusion (ICCF 5), p. 461.	Garg et al ICCF-5.pdf
21	1996-1	Sankaranarayanan, T.K., et al., Investigation of Low-level Ttritium Generation in Ni-H2O Electrolytic Cells, Fusion Technol., Vol 30, 1996 p. 349	Sankaranarayanan, T.K., et al., 1996. Investigation of Low-level Ttritium Generation in Ni-H2O Electrolytic Cells. Fusion Technology, Vol. 30, p. 349.	Sankaranarayanan Srinivasan 1996.pdf
22	1996-2	Rout, R.K., et al., Reproducible, anomalous emissions from palladium deuteride/hydride. Fusion Technol., 1996. 30: p. 273.	Rout, R.K., et al., 1996. Reproducible, anomalous emissions from palladium deuteride/hydride. Fusion Technology 30: p. 273.	RoutRKreproducib.pdf
23	2008-1	Report on NIAS one day meeting in Current Science	Srinivasan, M., 2008. Energy Concepts for the 21st Century. Meeting Report for the National Institute of Advanced Studies (NIAS). Current Science, Vol. 94, No. 7, p. 842-843.	Srinivasanmeetingrep.pdf
24	2008-2	Article for Innovative India Book	Srinivasan, M., 2008. Is There a Third Route to Generating Nuclear Energy? Manuscript. Submitted to Innovative India Book.	My Article Innovative India Submitted version (Feb 08).pdf
25	2008-3	Alchemy Myth or Reality TOI article	Srinivasan, M., 2008. Alchemy: Myth or Science? Rejoinder to “The Curious Case of an Experiment with Alchemy” by Atul Sethi. Times of India, June 1.	Alchemy - Myth or Reality (Dr. M.Srinivasan)- Revised in Nov 2018.pdf
26	2009-1	ICCF-15 (Rome) Paper on Micro Nuclear Explosion	Srinivasan, M., 2009. Hot Spots, Chain Events, & Micronuclear Explosions. Capstone Presentation. Fifteenth International Conference on Cold Fusion (ICCF15), Rome, Ital.y October.	My paper Micronuclear Explosion (ICCF 15).pdf
27	2009-2	JSE paper # 1	Srinivasan, M., 2009. Observation of High Multiplicity Neutron Emission Events from Deuterated Pd and Ti Samples at BARC: A Review. Extended Abstract. Journal of Scientific Exploration, Vol 23, Issue 4 (Special Issue), p. 477-482.	JSE Extended Abstract Multiplicty 2009.pdf

28	2009-3	JSE Paper # 2	Srinivasan, M., 2009. Observation of Neutrons and Tritium in a Wide Variety of LENR Configurations: BARC Results Revisited. Extended Abstract. Journal of Scientific Exploration, Vol 23, Issue 4 (Special Issue), p. 483-491.	JSE Extended Abstract Neutrons & Tritium 2009.pdf
29	2010-1	ACS LENR Sourcebook Vol.2 Wide Rangi…s	Srinivasan, M., 2009. Wide Ranging Studies on the Emission of Neutrons and Tritium by LENR Configurations: An Historical Review of the early BARC Results. in Low Energy Nuclear Reactions and New Energy Technology Sourcebook Vol 2, edited by Jan Marwan and Steven B Krivit. American Chemical Society, Oxford University Press.	Srinvasan's ACS paper - Peer review version.(With Figs).pdf
30	2010-2	M. Srinivasan, "Neutron Emission in Bursts and Hot Spots: Signature of Micro-nuclear Explosions?" Proc. New Energy Technologies Symposium, San Francisco, CA,USA, March 21st-22nd, 2010, American Institute of Physics	Srinivasan, M., 2011. Neutron Emission in Bursts and Hot Spots: Signature of Micro-Nuclear Explosions? J. Condensed Matter Nucl. Sci., Vol. 4, p. 161–172.	My Paper JCMNS Micronuclear.pdf
31	2010-2	Defence Science Alert Paper –I	Srinivasan, M., 2010. Nuclear Energy! Nuclear World, Energy Security. Defense and Security Alert. September. p. 22-27.	My Article DSA I published version (Sept 2010).pdf
32	2010-3	Science Reporter Article	Srinivasan, M., 2011. "Cold Fusion" Poised to Become an Industrial Reality! Science Reporter, July, p. 26-30.	My Article Science Reporter (July 2011).pdf
33	2010-4	IANCAS	Srinivasan, M., 2010. Progress in Condensed Matter Nuclear Science. Manuscript. Submitted to IANCAS.	My IANCAS Article Final.pdf
34	2011-1	Wiley Book Ch 43 Final version.	Srinivasan, M., G. Miley, and E. Storms, 2011. Low-Energy Nuclear Reactions: Transmutations. Chapter 43, Nuclear Energy Encyclopedia, Steven Krivit, Editor. John Wiley and Sons.	My Paper Wylie Book Ch 43 Final version.pdf
35	2011-2	Bio Nuclear Workshop Pamphlet .pdf	Srinivasan, M., Advisor. 2011. One Day Workshop on Biological Nuclear Transmutations : Historical Perspective and Applications. Organized by SRM University (SRMU), Kattankulathur ,Chennai and Indian Institute of Metals, Kalpakkam Chapter, IGCAR.	BioNuclear[1].pdf
36	2011-3	ICCF-16 (Chennai) Paper- Burst Neutron Emission	Srinivasan, M., 2011. Neutron Emission in Bursts and Hot Spots: Signature of Micro-Nuclear Explosions. Jour. Condensed Matter Nucl. Sci., Vol 4, p. 161-172.	My Paper JCMNS Micronuclear.pdf
37	2011-4	ICCF-16 (Chennai) Nagel	Nagel, D., and M. Srinivasan, 2012.	Nagel-Srinivasan ICCF

		& Srinivasan paper	Evidence from LENR Experiments for Bursts of Heat, Sound, EM Radiation and Particles and for Micro-Explosions. 17th International Conference on Cold Fusion.	17 Bursts Paper 31 Oct 2012 FINAL.pdf
38	2011-5	Marianne IE Article	Macy, M., 2011. ICCF16 in India: A Historic Perspective. Infinite Energy, Issue 95 (January/February), p. 1-5.	Marianne IE Article Final (29th Dec 10).pdf
39	2012-1	Defense Science Alert (DSA) Article 2	Srinivasan, M., 2012. Nickel-Hydrogen “LENR” Reactor: Novel “Energy Cat“ in the Marketplace! Manuscript to Be Published Your January 2012 Issue of “Defense and Security Alert” (DSA)	My Article DSA II Submitted version (15th Dec 2011).pdf
40	2012-2	Edited Proceedings of ICCF-16 Conference	Srinivasan, M., 2012, Editor of Proceedings of the ICCF 16 Conference, February 6–11, 2011, Chennai, India. J. Condensed Matter Nucl. Sci. Vol. 8.	Srinivasan Editor ICCF-16 Proceedings Front.pdf
41	2013-1	ICCF-17 (Korea) paper on LENR Transmtuations	Srinivasan, M., 2013. Transmutations and Isotopic Shifts in LENR Experiments: an Overview:. Journal of Condensed Matter Nuclear Science, Volume 12, Pages 1-10.	My Paper JCMNS Transmutations (2013).xps
42	2014-1	Burst Neutron Emission JSCMNS Vol 4 2011.xps	Srinivasan, M., 2011. Neutron Emission in Bursts and Hot Spots: Signature of Micro-Nuclear Explosions? J. Condensed Matter Nucl. Sci., Vol. 4, p. 161–172.	My Paper JCMNS Micronuclear.pdf
43	2016-1	ICCF-18 (Missouri) Tritium Paper	Srinivasan, M., 2014. Revisiting the Early BARC Tritium Results. Research Article, J. Condensed Matter Nucl. Sci. Vol. 15, p. 1–12.	My Paper ICCF 18 Tritium JCMNS Final (5th Jan 2015).pdf
44	2016-2	Journal of Governance Paper	Srinivasan, M., 2016. “Low Energy Nuclear Reactions” (LENR) – Emerging Clean and Compact Source of Nuclear Energy. Journal of Governance, Special Issue on Energy. January.	My Article JoG Final version (17th Dec 2015).pdf
45	2017-1	C.R.Narayanaswamy, J. Condensed Matter Nucl. Sci. 24 (2017) 1–8 Observation of Anomalous Production of Si and Fe in an Arc Furnace Driven Ferro Silicon Smelting Plant at levels of Tons per day	Narayanaswamy, C.R., 2017. Observation of Anomalous Production of Si and Fe in an Arc Furnace Driven Ferro Silicon Smelting Plant at Levels of Tons per Day. J. Condensed Matter Nucl. Sci. Vol. 24, p. 1-8.	Narayanaswamy Paper -Final to be printed version th Aug 2017.pdf

4.2 Document Copies

The 45 papers prepared by Dr. Srinivasan's LENR work comprises over 3 inches of hard-copy. A second volume separate from the main report has therefore been prepared (Figure 4-1). As noted, the reports have also been placed in the SLRDP Dropbox folder in electronic (PDF) form.

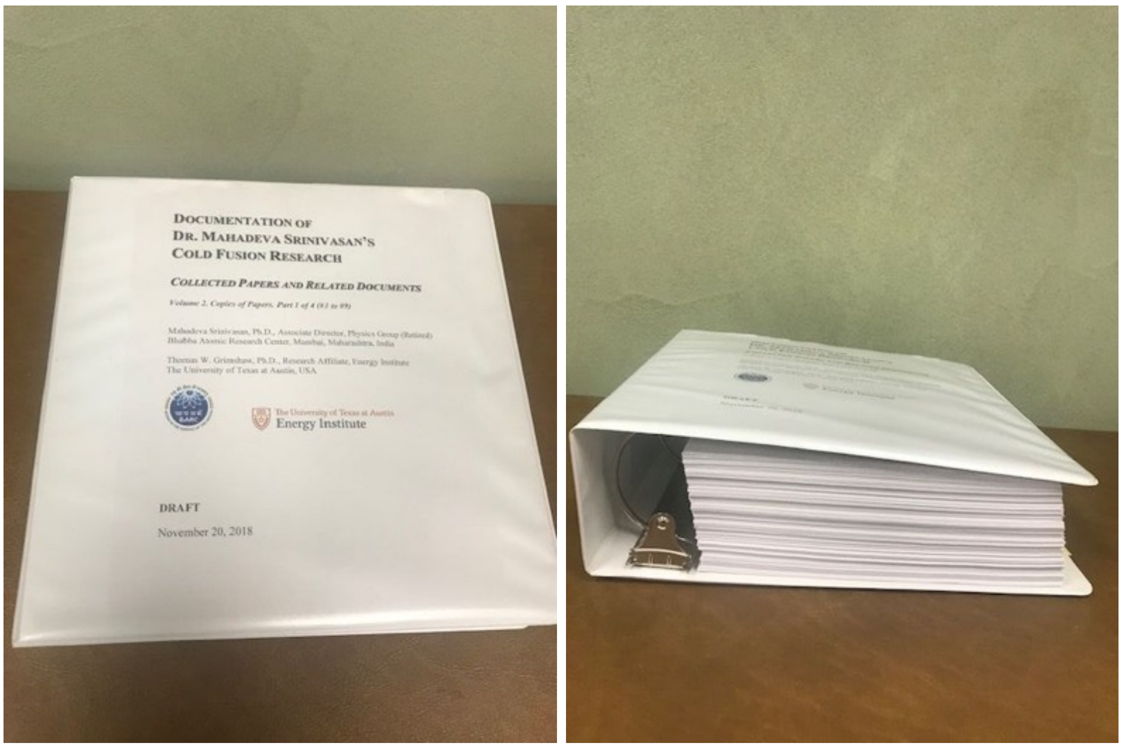

Figure 4-1. Collection of Dr. Srinivasan's Publications: Volume 2 of This Report

5 Future Opportunities

There are several opportunities to extend the documentation of Dr. Srinivasan's LENR research record. For example, interviews could be conducted with his collaborators in the field, and the transcripts could be added to the SLRDP. The existing transcripts of interviews could be reviewed and annotated to add more detail to his timeline as presented in the autobiographical sketch. Similarly, the list of publications may be more closely tied to the phases of the timeline.

6 Project Methods

The methods used in the SLRDP are based on general LRDI procedures that are modified to meet the specific requirements of individual LENR investigators[19]. The project is being performed according to accepted project management practices[20]. As noted above, the overall LRDI procedure is set forth in a recent article in Infinite Energy[21].

Two memos were prepared as progress was made in the SLRDP: 1) Printout of Your Cold Fusion Papers (November 4, 2018); and 2) Transcripts of Interviews (September 26, 2019). A Dropbox folder has also established to store files for the Project[22]. The organization of the folder (Figure 6-1) includes subfolders that are in general numbered sequentially as materials were located and progress was made. A high-capacity external hard drive is used for periodic backup of the LLRDP and other LRDI project files.

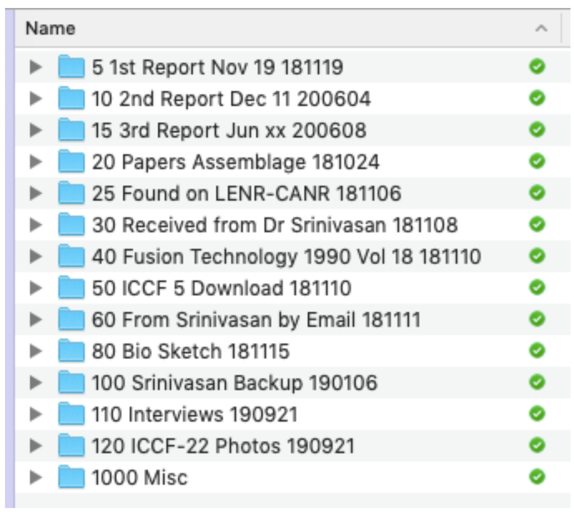

Figure 6-1
Screenshot of "Chino Papers" Folder on Dropbox

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[19] Grimshaw, T.W., 2019. Collection, Organization, and Documentation of LENR Research Results: Guideline. January.

[20] Project Management Institute, 2017. A Guide to the Project Management Body of Knowledge (PMBOK® Guide) — Sixth Edition and Agile Practice Guide (ENGLISH). Project Management Institute. Newtown Square, PA.

[21] Grimshaw, T., 2020. Documenting Cold Fusion Research: Preserving a Vital Asset for Humankind. Infinite Energy, Issue 150, March/April, p. 9-13.

[22] Dr. Srinivasan's alternate nickname to "Srini" is "Chino". The latter name is less preferred and is no longer used here.

Appendix A. LENR References and Documents

A1. "Fire from Ice": Extract Referencing BARC Cold Fusion Work

FIRE FROM ICE

Searching for the Truth Behind the Cold Fusion Furor

Eugene F. Mallove

Fire from Ice

Searching for the Truth Behind the Cold Fusion Furor

by Eugene F. Mallove

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work as advertised. These false alarms, including cold fusion, were being identified as the "Utah Effect." *The New York Times* editorialized that the University of Utah ". . . may now claim credit for the artificial heart horror show and the cold fusion circus, two milestones at least in the history of entertainment, if not science." The editorial also said, "Given the present state of evidence for cold fusion, the government would do better to put the money on a horse."* The editorial admitted, however, that there might be something to the Utah work.

One certain effect of the Baltimore meeting: Palladium prices plummeted on the commodities exchange. Then weeks and months afterward, readers of the *Bulletin of the American Physical Society* were regaled with talk of cold fusion's demise. Robert Park, not an official spokesman for the APS, but whose "What's New" news-opinion columns appear in the organization's *Bulletin* wrote: "The corpse of cold fusion will probably continue to twitch for awhile, even after two nights of unrelenting assaults at the APS Baltimore Meeting" (May 5, 1989); "Alas, experiments conducted by Sandia scientists, using multiple neutron detectors in a deep underground laboratory, would seem to bury the Brigham Young reports of cold fusion right alongside the more extravagant claims of Pons and Fleischmann."

✳ The Death of Cold Fusion: Greatly Exaggerated

Heaping scorn on scientific research, calling it "pathological science," being so self-assured as not to admit the possibility of error in one's own experiments that seemed to show others to be in error; these were some of the many thrusts used by critics of cold fusion to "kill it." The stench of the death metaphor was in the air. But cold fusion was good at playing possum. Tinkerers and scientists around the world kept plugging away with their electrochemical cells, despite skepticism by "the scientific establishment" and by a barrage of ridicule in the media. Enough money was flowing from the major electric utility research consortium, the California-based Electric Power Research Institute (EPRI), to keep some researchers afloat, a bit of leftovers from DOE, Utah money, some private efforts, and rampant bootlegging of tinkering time from defense and other research contracts. Case Western Reserve University, Stanford University, Texas A&M, and the Bhabha Atomic Research Institute in India had gotten positive results by now. There was now reason to believe and hope.

**New York Times*, April 30, 1989. But the *New York Times* editorial page was not necessarily a font of scientific wisdom. Many years earlier a *Times* editorial had excoriated the now-accepted ideas of the great American rocket pioneer, Robert H. Goddard—an editorial that they symbolically withdrew after the first manned Moon landing in 1969.

Panel member Koonin was still intrigued by Menlove's neutron data and by Wolf and others' tritium, but he was betting on contamination in the latter. Science journalist Gary Taubes was investigating whether some of the A&M tritium might have been put there "by human hands." Word of his travels spread along the scientific grapevine; the results of his investigation appeared in print the following June (see Chapter 14).

After reaching its preliminary negative view in July, the DOE panel planned to meet again in mid-October. No matter, the final results of the "autopsy" for helium ash in Utah's palladium rods were not expected before the end of September. Poor Howard Menlove at Los Alamos, who had such compelling neutron data. He would submit his work to *Nature* at the end of the summer, and there it would languish unpublished. Though Menlove had satisfied four of five reviewers, *Nature* would not publish his work. Even when he obtained additional data to satisfy *Nature*, the magazine blocked publication again. But Menlove's results were getting better. To deny the soundness of his data would become an exercise in futility. He had clearly detected real neutrons coming from a place they were not supposed to be.

Nature was receiving more papers on cold fusion than on any other single topic. By their editor's own admission, these were roughly evenly divided between supporting and nonsupporting evidence. David Lindley thought that the negative papers were better and was claiming that the positive ones were encountering referee problems. He wanted detailed and thorough papers comparable to the negative ones. I have seen firsthand some of the correspondence between Lindley and an unpublished researcher. What Lindley was doing, it appeared, was to set up the negative papers—such as Nathan Lewis's—as a "standard" against which any positive results paper would have to stand. Even when Lindley acknowledged that he could no longer tell whether proponent or skeptic was correct, still no positive papers were published.

The East was unmoved by *Nature's* quirks and the DOE panel's maneuvering. Indian researchers at the Bhabha Atomic Research Center (BARC) hadn't given up on cold fusion. In fact, some were saying that the skeptical tones being heard in the United States were a "cover" for secret intensive work here! At the end of 1989 they would publish a compendious report, "BARC-1500," which told of their extensive cold fusion studies carried out from April through September—"the first six months of the 'cold fusion era,' " as they phrased it. The Japanese were being more discreet about their efforts, although there were reliable reports that some 40 groups with a total of 150 researchers were working on cold fusion in Japan. Referring to their purported work, Robert Huggins said at a conference at the University of Utah, "There is a deafening silence across the Pacific Ocean." In mid-September, Fleischmann and Pons went to Japan to attend the annual meeting of the

If Washington couldn't bear cold fusion, Utah sure would. After about nine months of gestation, on December 6, the State Fusion Energy Advisory Council voted to approve NCFI's work, mocking the federal government's surrender. Though the DOE panel had let cold fusion down, positive results from the DOE's own laboratories, Oak Ridge and Los Alamos, were beginning to surface. Los Alamos was strong on anomalous tritium and neutrons. ORNL had gotten tritium bursts, excess heat, and low-level neutrons. Yet without winking, Huizenga claimed that the DOE panel had considered ORNL and LANL findings. How so? Pons, for one, reacted angrily to the DOE report in the *Deseret News*: "They have made their judgement: they have passed sentence on fusion and they've been proved wrong. They were wrong from day one. The DOE appointed a bunch of negative people to give a negative decision. They will continue to be proven wrong—even by their own laboratories."

✳ Foreign Influences

If cold fusion research in the United States was hampered during 1989 by undue skepticism and a virtual "climate of fear" about being associated with the topic, investigators in other countries seemed to have experienced fewer such hang-ups. One could not have gleaned this from most media accounts, however. Yes, there were reports in major newspapers that various isolated experimenters in Japan, India, or the Soviet Union had claimed this or that result, but the impression was that the same official lack of interest existed elsewhere. This was false in many cases.

Evidence was all around: In late November, Japanese scientists at Nagoya University reported a radically new cold fusion method. Writing in the English-language *Japanese Journal of Applied Physics*, Nobuhiko Wada and Kunihide Nishizawa described high levels of neutron emission—20,000 times background—apparently coming from fusion reactions that began when they applied 20,000 volts between palladium electrodes in a cell of deuterium gas. Another group at Osaka University reported a level of neutrons 2.5 million times background coming from a cold fusion cell. "We ought to kick their butts," was the reaction of Senator Hatch to the news from Japan (*Deseret News*). Fearing that the Japanese had almost certainly already filed patents on these kinds of processes, there was anger in official Utah that they weren't able to obtain as broad a patent coverage as would have been possible with federal support.

India was forging ahead too. In early January 1990, *Nature* reported that Dr. P. K. Iyengar, the director of the Bhabha Atomic Research Center, believed that "U.S. scientists are convinced that cold fusion can take place but are keeping their results secret." He perhaps got that impression because the climate of scientific intimidation and ridicule in the United States was so strong that many researchers of necessity had to conduct their cold fusion work very quietly or underground.

The December 1989 report on cold fusion studies at BARC (April through September 1989) gave straightforward interpretations of experiments underway in India and were a welcome relief to the circumspection so prevalent here. Measured scientific descriptions were mixed with an ebullient description of "the first six months of the new 'cold fusion era.' " More than 50 scientists and engineers, plus many technicians from more than 10 divisions at the Center, were apparently involved in the initial phase of the cold fusion program. The report described work going on in electrolytic cold fusion, deuterium gas loading of metals to produce cold fusion, and theoretical investigations of the new phenomena. The report concluded, "Investigations of cold fusion phenomena carried out at Trombay during April to September 1989, have positively confirmed the occurrence of (d-d) fusion reactions in both electrolytic and gas-loaded palladium and titanium metal lattices at ambient temperatures."

The Indian researchers claimed to have seen their first bursts of cold fusion neutrons generated by electrolysis as far back as April 21, 1989. As researchers elsewhere had found, not all of these early cells became active. Some of the cells that worked had cathodes of palladium, but others employed palladium-silver alloys and even pure titanium. But the Indians had discovered an important general property of the cells that did work: surprisingly low overall ratio of neutrons generated to tritium atoms produced. The ratio was in the range of one-millionth to one-billionth (10^{-6} to 10^{-9}). Just what some theoretical work was indicating and what experimenters in the United States who had gotten tritium were also finding! The report concluded that cold fusion is essentially "aneutronic"—unlike hot fusion, it did not produce copious amounts of energetic neutrons. The Indian researchers also believed that cold fusion in electrolytic cells is a phenomenon that occurs on the surface of a metal electrode, not deep within its structure.

At this peculiar time between belief and disbelief, an accounting of cold fusion experiments offered these reasons to accept that there might be something new under the sun: (1) persistence of unexplained tritium at concentrations from 10,000 to a million times normal background levels as well as numerous reports of tritium at much lower but still unexplained levels; (2) persistence of anomalous heat in many different types of calorimeters, even closed cells with forced D_2-O_2 recombination; (3) the very erratic nature of the phenomena, which added to the impression that there was something *different* to be explained, not that someone's crazy experiment was simply exhibiting a systematic error; (4)

they wrote: "In its extreme form, following the herd in editorial opinion is a manifestation of cultural fascism: the expression of convictions based on inadequately understood theories and facts. Scientific conformism is known as 'handle cranking' or 'me-too science.' Committee reports (which are editorials) are specialized ways of inducing scientific conformism. Electronic mail and fax machines are specialized ways of inducing scientific hysteria. . . . If Lindley doesn't have the time to come now to Utah to gather information firsthand, then why doesn't he at least have the sense to use that well-known shortcut of establishing the scientific credentials of the believers and non-believers, namely the Citation Index*?"

✳ Journey to Salt Lake

It was no secret that most people who had come to Salt Lake City were sympathetic to the idea of cold fusion. They were "believers," as they had disparagingly come to be known. About 230 scientists, engineers, basement experimenters, entrepreneurs, and interested citizens had come to witness more than 40 presentations and panel discussions on cold fusion experiments and theory. Many were not convinced that cold fusion was a real new physical phenomenon, but for them the evidence was provocative enough to make the trip worthwhile.

On a Wednesday evening Delta airlines flight, there were at least four others heading for the meeting, three of them from MIT, including my archskeptic friend, Richard Petrasso. After on-again, off-again plans to attend the conference, Petrasso had made a last moment decision to go. But it was clear that he really did want to find out what new developments might have occurred, as well as to become a prominent de facto spokesman for the critics. And he did that with relish.

Petrasso was still finding it hard to come to grips with the reports of tritium generation. He didn't believe them at all and thought they would eventually be attributable to inadvertent contamination. The lack of experiment repeatability and the quality of reported pre- and post-experiment analysis of composition concerned him. He had listened to reports from other skeptics that some of the scientists doing the tritium work had perhaps "gotten out of their depth" and were not doing the exacting work necessary to pin the tritium question down. But he also must have known that at least at the Bhabha Atomic Research Center in India, there were seasoned tritium measurement experts who were reporting significant levels of tritium in cold fusion experiments.

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*A computerized data base that can quickly determine how many times a scientist's papers have been cited by other researchers in their papers.

been pointed out. Steve Jones injected melodrama by posing a question while displaying a color transparency made from a July 1989 issue of the *Deseret News*, which purported that a boiling water device had been achieved: "Do you still feel that a near-term practical device is possible?" With neither emotion nor faltering Pons said, "All our present research is aimed at it." Inconsistently, Jones favored explaining some of Earth's internal heat by cold fusion, but he could not suffer talk of more intense heat being generated in cold fusion jars.

The morning was devoted largely to heat measurements, the formal term being calorimetry. What better follow-up to the open-cell work of Fleischmann and Pons than to hear about heat measurements done in completely closed cells, a technique instantly recognizable in the air-tightness of its approach. Michael McKubre of SRI International, whose cold fusion group is still being funded by the nearby Electric Power Research Institute, discussed the high-precision measurements that his team had made with deuterium-palladium electrochemical cells pressurized with deuterium gas to 60 times atmospheric pressure. They intended to infuse as much deuterium as possible into the Pd lattice. SRI had begun its work soon after the Fleischmann-Pons announcement and was attempting to discover whether the phenomenon was a surface or bulk effect, arriving at the tentative conclusion that it was in the bulk.

The team emphasized determining the ratio of number of deuterium to palladium atoms (D/Pd) in the electrodes during the course of their experiments with two different kinds of calorimeters. This they did by continuously measuring the electrical resistance of the palladium electrodes and knowing how it varies with the D/Pd loading. Getting D/Pd as high as one could—over 1.0 if possible—had become the empirical figure of merit in many of the cold fusion efforts. McKubre reported bursts of heat lasting several hours or tens of hours, producing excess energy amounting to several hundred thousand joules. The bursts were irrefutable. Often excess power would die away and then reappear within a single day. In the printed proceedings the group would state: ". . . we consider it unlikely that [the] excess energies . . . can be accounted for by chemical processes."

McKubre added a new and puzzling twist—one that the Indian group at BARC had reported originally. About a week after one of their palladium electrodes was removed from its cell, they placed it between layers of photographic film. After 12 days' exposure, the developed black and white film showed intense white "hot spots" of fogging, with nebulous interconnecting filaments. It was very cosmic, looking much like an astronomical photograph of a cluster of stars and surrounding nebulosity. Using an ancient image, McKubre described the wondrous thing as looking like the Shroud of Turin. Indeed, skeptics probably the distinct smell of a nuclear process. But proving what the hidden nuclear reaction products were was much tougher than anyone had bargained for. The process was a victim of its own success. It simply does not take all that many fusion reactions (compared to the total number of palladium or deuterium atoms in a system) to get these levels of energy release.* Was it the lithium reacting with deuterium to form helium, or the lower abundance ^6Li isotope fusion "burning" with deuterium to create ^7Li plus hydrogen? Possibilities, but no evidence.

At a rate approaching $2 million per year, EPRI was funding a number of these projects: McKubre's effort at SRI, Huggins's calorimetry at Stanford, some work at Texas A&M, and various consultant and support service work. Manager Joseph Santucci spoke of the EPRI initiative and was "very comfortable saying that the excess heat is not due to chemistry," that "the source of the heat and the tritium may be different." In his view, "It's very puzzling and we have to get to the heart of it." In his summary he had mentioned "tantalizing" evidence of X rays coming off the spent electrodes—by implication connected with the puzzling SRI autoradiograph. X-ray expert Petrasso took offense at such an offhand remark about potentially significant confirming evidence: "I don't think you can be *alluding* to X rays—this is a bit irresponsible."

The words from India did not disappoint those who anticipated challenging news from the mysterious East. P. K. Iyengar, director of the BARC in Bombay, where more than 1,000 scientists work on various nuclear energy projects, told how acceptance of cold fusion had sprung naturally from his culture: "We in India believed in this because of our traditions. . . . Our experiments further strengthen our belief that cold fusion is occurring." Experiments had begun in earnest at BARC in April, with positive results coming in fast and furious almost from the beginning—in great contrast to the halting Western experience. This led some skeptics to believe that the vast holdings of heavy water (used as a neutron moderator in their nuclear reactors) with its attendant tritium impurities might be a source of inadvertent contamination. Bhabha claimed an amazing overall 70 percent success rate in measuring tritium production and bursts of neutrons coming from a wide variety of palladium-titanium-deuterium systems—gas cells and liquid cells. In the Indian paper submitted for the proceedings, the scientists concluded: "Experiments carried out by a number of totally independent groups

*Since one MeV—about the amount of energy released in a typical fusion reaction—is equivalent to 1.6×10^{-19} joules or 1.6×10^{-13} megajoules (MJ), it only takes 10^{13} reactions to exceed a megajoule of energy output. But a cubic centimeter of palladium has 7×10^{22} atoms, so if the lattice has a 1/1 ratio of D to Pd atoms, only a minute fraction of all lattice sites containing deuterium will have to experience fusions each second.

employing diverse experimental setups have unambiguously confirmed the production of neutrons and tritium both in electrolytically loaded and gas loaded Pd/Ti lattices."

The BARC experiments, as outlined by Iyengar's animated colleague M. Srinivasan of the Neutron Physics Division, were extraordinary in both the number of variations attempted and the relatively consistent finding that tritium atom generation was ahead of neutron production on the order of a million to a billionfold. On the very first day of their experiments (April 21, 1989), bursts of neutrons and tritium had come from eight out of eleven cells. Half of the electrolysis experiments were "doubly successful" in giving forth *both* neutrons and tritium. Tritium increases were often seen right after neutron bursts. To critics who claimed that the copious tritium could be coming from the large amounts of tritium-laden heavy water in Bhabha's fission reactors, Srinivasan replied, "Just because we have a heavy water reactor doesn't mean that the whole world is coated with tritium." These were experienced measurers of tritium and neutrons, whose results were not fairly dismissed by cultural prejudices about science in the developing world.

To cap these provocative results with visual proof that something very unusual and probably nuclear had been found, Srinivasan displayed an autoradiograph made from palladium-silver foil that had been infused with deuterium gas. It resembled the one from SRI. It was presumptive evidence of perhaps ionizing radiation from tritium decay. Srinivasan showed other intriguing autoradiographs obtained from a variety of related deuterated metals experiments. If the pictures weren't lying, India had snagged on film the footprints of the cold fusion Genie.

✳ Sharp Theories

Fiery Italian physicist Giuliano Preparata had been one of the first to propose mechanisms to explain cold fusion; he and his colleagues published a paper in *Il Nuovo Cimento* in May 1989. "The humble work of a theoretician trying to make sense out of something that 'does not make sense' " was how he described his work to the conference. Even before news about cold fusion broke, Preparata had been thinking about *coherent* oscillations of particles in solids. Those ideas were conceived to explain cold fusion about the same time that Peter Hagelstein on the other side of the Atlantic was coming up with *his* coherent fusion theory.

Other than using the terminology of coherence, the two theorists were really suggesting quite different cold fusion mechanisms. Despite the lack of evidence for a helium-4 reaction product in numerous heat-producing experiments, Preparata still believed that helium was emerging undetected from the palladium into the gas phase. While Hagelstein spoke of proton-burning to get heat (from the trace amounts of ordinary

He also explained how tritium could simultaneously be generated within the lattice at a rate that roughly spanned the range of experimental reports.

Schwinger's theory went directly against the grain of hot fusion dogma. "The correct treatment of cold fusion will be free of the collision-dominated mentality of the hot fusioneers. . . . It is clear that cold fusion and hot fusion are qualitatively different phenomena," he emphasized. "What you have heard, of course, is not the end of an investigation, it's the beginning."

Why, I asked Schwinger on the side after his talk, was a Nobel laureate physicist striking out in a direction that had become so disreputable in the general physics community? His simple answer: "My nature is that when everybody is going one way, I like to go the other way."

✳ Tritium, Neutrons, and More

On Friday, the conference considered nuclear products in greater depth. John Bockris presented the Texas A&M tritium findings in electrochemical cells, which were among the most spectacular of all cold fusion data that had been obtained—results that were being darkly questioned in back rooms by the skeptics. Journalist Gary Taubes hovered about the press room, waiting to spring his "tritium as *deliberate* contamination" theory within the next few months (see Chapter 13). The A&M team had observed tritium concentrations of 10^4 to over 10^7 disintegrations per minute per milliliter of electrolyte—approaching a million times background. This corresponded to tens of billions of tritium atoms being created each second for every square centimeter of electrode surface. (Results from some tests at BARC in India had produced as many as 10^{12} tritium atoms.)

Bockris and his colleagues believed that there was much evidence that cold fusion was a surface effect, that it was not occurring in the bulk palladium. They theorized that high electric fields near the tips of dendrites might be accelerating and smashing deuterons together at the surface, making them fuse to produce tritium.

Bockris acknowledged that the A&M track record of repeatability was not as good as he would have liked. Fifteen out of 53 electrodes had produced tritium; thinner electrodes (less than a millimeter diameter) had more like a 70 percent success rate; only five out of 28 electrodes had produced excess heat. It was possible, Bockris said, that in "failed" cells tritium could have been missed and "sparged out" [mixed with other gas and removed] in the gas flow of the open cells, because tritium ordinarily came in bursts—perhaps not always detected by the irregular sampling schedule. In one cell, on two occasions there appeared to be a correlation between two heat rises and tritium bursts. Adding to the strangeness of the phenomena was that A&M required weeks before the initiation of tritium evolution in a cell, whereas at BARC tritium typically came up very quickly within a day of electrolysis start.

Ed Storms and Carol Talcott of Los Alamos National Laboratory had been working on the tritium problem in closed electrochemical cells for many months. Though their success rate was only about 10 percent at this time, and they could not pin down the parameters that would induce a cell to turn on, they stood solidly behind their tritium. They were talking about 150 cells and 5,000 tritium measurements, an experimental tour de force with many kinds of checks for contamination in the environment. It was an Edison-like trial and error approach in treating electrodes with gunk and grime to track down what would boost them into activity. Said Storms to critics, "We can put aside the question as to whether it's real."

Charles Scott brought more news about tritium from Oak Ridge National Laboratory. His group had gotten not only extended periods of excess heat in closed cells, but tritium and neutrons as well. "In every test greater than 200 hours we get excess power," Scott said. "Excess power does occur—unequivocally—and can be extended for hundreds of hours." One run had already topped 2,000 hours. The Los Alamos and Oak Ridge tritium was high enough over background to convince the researchers that it was not contamination, but it was admittedly far below the levels that Texas A&M and India had gotten.

In a panel discussion to sum up what had gone before, Fleischmann said, "I think that it's now pretty clear that there is steady state heat production from Pd electrodes." Electrochemist Charles Yeager from Case Western supported Fleischmann: "This will be noted as a decisive turning point in the history of the affair. . . . What we are seeing cannot be explained by trivial mathematical error. . . . Let's hope that we can take the effect up by many orders of magnitude."

With all the provocative evidence—the heat and the tritium, one would have thought the sternest skeptic would wither. Not so, as CERN's Douglas Morrison revealed: "I started out as a believer [in April 1989]. It's fair to say I'm now a skeptic." Mike McKubre shot back at Morrison's suggestion that theory could not account for what was being reported: "Morrison misunderstands the purpose of theory." At which point Yeager recalled the seminal Michelson-Morely experiment of 1887 that found the speed of light to be constant despite the motion of Earth through the much-discussed invisible "aether"—contrary to expectation. (Incidentally, Morely was a chemist!) Yeager had the wisdom of the long view in science: "The thing will work its way out, but it will take time and be very expensive."

indeed, real. His further work reported at the BYU conference corroborates Cecil's work. Kevin Wolf, however, who only recently began trying to replicate such experiments, had no positive results to report.

Howard Menlove from LANL said that he was still seeing the neutron bursts that he had discussed at the Cold Fusion Workshop in May 1989 at Santa Fe. M. Srinivasan of the Bhabha Atomic Research Center noted BARC's similar results and commented that the neutron bursts might be coming from just a few of the individual titanium chips, as his group had found. Menlove agreed. Srinivasan displayed a slide made from an "autoradiograph" of a deuterium-gas-activated chip in which apparently a significant quantity of tritium—a millicurie—had formed.

Steve Jones reported extensions of his earlier reported neutron work, such as putting his experiments in a deep mine and doing further checks for various kind of interference. Jones expressed confidence that he was continuing to see neutrons, both from deuterium gas cells and electrochemical cells. Kevin Wolf from Texas A&M reported more positive neutron results, as did F. Scaramuzzi, Dr. Zhu from the Institute for Atomic Energy in Beijing, and Dr. A. Takahashi from Osaka University and the Matsushita Electric Industrial Company in Japan.

Tom Claytor from LANL reported increasingly refined experiments in his deuterium gas cells that employ high voltages to generate tritium. His group, he said, is now able to generate tritium reproducibly. They did not get it whenever ordinary hydrogen was used.

✳ Open Questions

The compelling evidence for nuclear effects in deuterated metal systems gives enormous credibility to a possible nuclear explanation for excess heat measured in similar experiments—despite persistent attempts by Steven Jones and others to dissociate the two effects. After all, if the heat comes in such profusion that it seems to require a nuclear explanation, and if at the same time there are indisputable nuclear effects detected, even if these do not specifically explain the excess heat *quantitatively*, this seems strong presumptive evidence that the basis for the excess power may be nuclear.

Still the question remains: What is causing the excess heat if not ordinary chemistry or some bizarre, unknown form of mechanical energy storage and release? If the process be nuclear, what is the "fuel" and what are the reaction products—the "ash" of the hidden nuclear "fire." An answer could come in a two-step process: (1) First, experiments to identify the fuel and end product(s) irrespective of the complex mechanism by which the presumed fusion is brought about and (2) elucidating the physical mechanism for the reactions, for example, the specific type of lattice-deuteron behavior that promotes the reaction.

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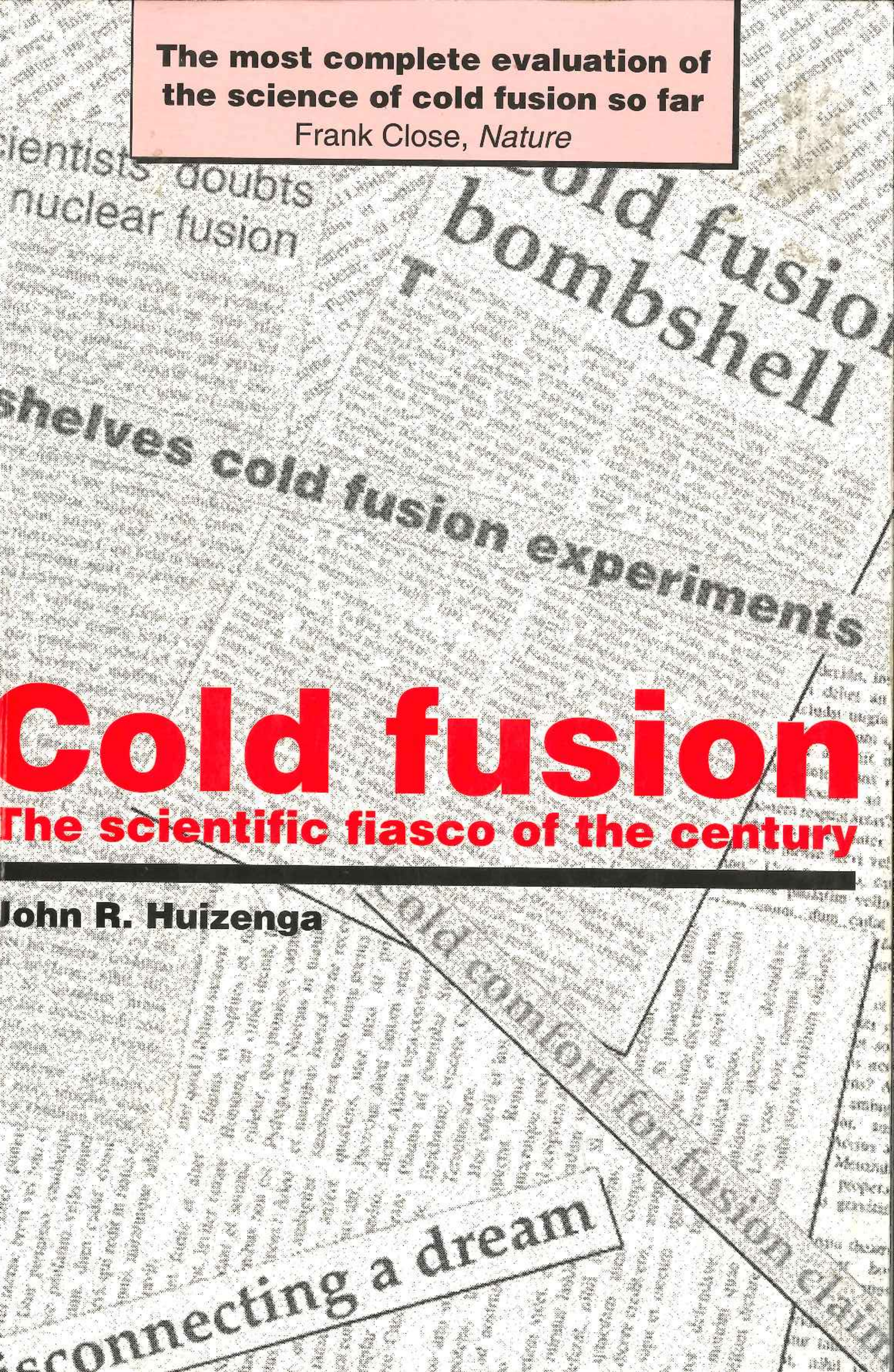
A2. "Cold Fusion – Scientific Fiasco of the Century": Extract Referencing BARC Cold Fusion Work

The most complete evaluation of the science of cold fusion so far
Frank Close, *Nature*



Cold fusion

The scientific fiasco of the century

John R. Huizenga

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Wolf here concurred with the analysis of our DOE Panel that none of the tritium found in any of the Bockris experiments was produced by the known D+D reaction. As of October 1989, however, Wolf *et al.* had not identified the source of the tritium contamination.

The above conclusion that tritium was not produced by D_2O electrolysis is in agreement also with experiments which searched directly for the 3–MeV protons produced in the D+D $\rightarrow$ T+p reaction. A variety of experimental techniques had been used in these searches for protons; all of these studies set very low limits of fusion occurring via the D+D $\rightarrow$ T+p reaction. For example, Price *et al.* [*Phys. Rev. Lett.* **63** 1926 (1989)] set an upper limit of 8×10^{-26} fusions per DD pair per second for the D+D $\rightarrow$ T+p reaction. These direct searches for charged particles confirms that the tritium activity reported by Bockris, and others was not due to D+D fusion. The Price *et al.* limit on the cold fusion rate was also considerably less than the fusion rate inferred from the neutron measurements of Jones *et al.* via the D+D $\rightarrow$ ^{3}He+n reaction.

The only other laboratory in the world that had reported large amounts of tritium comparable to that of the Bockris group was the Bhabha Atomic Research Center (BARC) in Trombay (near Bombay), India. At BARC, a wide variety of experiments were carried out by twelve independent teams of scientists employing both electrolytic and gas phase loading of deuterium in palladium and titanium metals to study cold fusion. These results were published in a long report BARC–1500 (December, 1989) which was reproduced in *Fusion Technology* **18** 32–94 (1990), and also in abbreviated form, in a paper published in the *Proceedings of the First Annual Conference on Cold Fusion*, p. 62 (March 28–31, 1990). Our DOE/ERAB panel had these results in the form of a preprint during the time we were preparing our final report.

The BARC experiments were limited to the detection of fusion products with the aim of verifying the nuclear origin of cold fusion, rather than the measurement of "excess heat" since they stated that the heat measurements required intricate calorimetry. This expressed opinion on calorimetry was a striking admission when compared to many early pronouncements describing the experiment as so simple that it could be done in a high-school laboratory. One of the unique features of the BARC experiments was that bursts of neutrons and tritium were reported to have occurred in eight out of eleven cells on the very first day of initiating electrolysis! Compare this to the view of Bockris expressed in a letter to me where he stated:

> As to those expert and respected scientists (e.g., at Yale, MIT, Caltech, etc.) who denied the existence of nuclear electrochemical effects, it is easy to understand the absence of their results: the effects, now seen by so many, do not switch on for many weeks of electrolysis.

Pons and Fleischmann had made similar statements, namely:

> Our calorimetric measurements of the palladium-deuterium system . . . showed that it is necessary to make measurements on a large number of electrodes for long times (the mean time chosen for a measurement cycle has been three months).

Hence, in contrast to other experimenters, the different BARC groups were unusually successful, reporting significant amounts of both neutrons and tritium from a large variety of different types of cell configurations in times as short as a day, compared to months for Bockris, Pons and Fleischmann. The above statements of these authors are, however, inconsistent with some of their earlier reports. The Bockris group, for example, was also very successful in producing tritium in their early experiments as described previously. It was later that their success rate decreased markedly.

A second striking feature of the BARC groups' results was that the measured neutron to tritium yield ratios in eight out of eleven doubly successful experiments was in the range of 10^{-6} to 10^{-9}. Even neglecting reaction (1a) shown on p. 6, which is the primary source of neutrons, such small neutron to tritium ratios leads to a serious inconsistency. The tritons (T) produced in the D+D reaction have an energy of 1.01 MeV. These tritons bombard the deuterium in the cathode, as well as the deuterium in the surrounding heavy water (D_2O) in the electrolytic cell. The reaction T+D $\rightarrow$ ^{4}He+n is a copious source of 14–MeV secondary neutrons. For tritons stopped in the deuterium loaded in the palladium cathode, the secondary neutron yield is approximately 2×10^{-5} neutrons per triton, while for tritons stopping in heavy water the yield is approximately 9×10^{-5} neutrons per triton (U.S. Department of Energy Report DOE/S–0073, November, 1989). Hence, the above neutron to tritium ratios reported by the BARC scientists were some 20 to 20,000 times smaller than expected from the secondary neutrons alone!

The reported tritium/neutron (T/n) ratio in the range of 10^6 to 10^9 (note that the ratio reported here is inverted from that in the above paragraph) was in direct conflict with the well-established ratio of approximately one and led cold fusion proponents to invoke miracle number two, the branching-ratio miracle. In this particular case, however, the proponents' appeal to miracle two was pointless because the experimental absence of secondary neutrons ruled out the possibility that the reported high yields of tritium were due to the reaction D+D $\rightarrow$ T+p. Most nuclear scientists, therefore, concluded that the tritium measured by the BARC scientists in the above eight cells was due to some form of contamination.

A third unusual feature of the BARC experiments was the success rate of their experiments which employed a large variety of electrolytic cells and electrolytes. For example, all five experiments using NaOD electrolyte (in contrast to the usual LiOD) produced neutrons and tritium after a short period of electrical charging. This was in sharp contrast with findings of U.S. groups who reported failures with NaOD. Relative to the many tens of groups that had searched for fusion products the world over, the claimed success rate of the BARC groups for producing both tritium and neutrons was nothing less than sensational.

A fourth feature of the BARC experiments was the magnitude of the tritium and neutron production. While the tritium levels are higher than any group in the world except the Bockris group, with which they are comparable, the BARC neutron production rate exceeded by more than a factor of 100 the rate reported by Jones *et al*.

In summary, when compared to experimental results from around the world, the BARC results were too good to be believable. They were neither consistent with the many previous negative results nor with previous positive results. Their reported very large ratios (10^6 to 10^9) of tritium to neutrons served as evidence that the tritium in their cold fusion experiments was not being produced by deuterium fusion. A final comment on the BARC results may be of interest. Even if one assumed the tritium in the BARC cells was due to deuterium fusion, the resulting associated energy was so many orders of magnitude less than the Fleischmann-Pons reported excess heat, that by no stretch of one's imagination could a causal connection be made between the two phenomena.

Recently, Kevin Wolf has begun to unravel the mystery surrounding the tritium at Texas A&M. In his talk at the NSF/EPRI meeting, he had already concluded that there was no evidence for the D+D $\rightarrow$ T+p two-body reaction occurring in the palladium electrode during electrolysis and hence, the tritium did not arise from cold fusion. This, however, still left open the question about the origin of the tritium. In a partial answer to this question, Wolf found that some of his palladium electrodes, purchased from Hoover and Strong, were contaminated with tritium prior to arrival in his laboratory. He concluded "It's pretty clear that our low-level tritium was due to contamination" [*Science* **248** 1301 (1990)]. These results throw a shadow of doubt on some of the reported tritium work. Recall that the one- and three-millimeter cathodes giving tritium in Bockris' laboratory were also from Hoover and Strong. The Bockris cells, however, showed tritium levels much higher than that expected on the basis of the contamination found by Wolf. In trying to solve the origin of the high tritium levels in the Bockris cells, Wolf made a very astute observation. On analysis of the electrolyte from a Bockris cell that had previously shown high tritium, he found that the cell contained a large amount of light water (H_2O). This finding, although not proof, was consistent with the cell having been spiked with tritiated water which contains mainly light water. This particular cell had been stored for some months in a sealed container and, unless contaminated, should not have contained the observed amounts of light water. After learning of Wolf's results, the Bockris group examined more of their cells and found again in some other cells large amounts of light water. Wolf responded "It's just incredible, I don't understand it." He found less than 1% light water in the cells in his own laboratory. Again the question, was tritiated water, known to be stored in the laboratory, the source of the light water? Wolf said "The proper conclusion is that things in the Bockris laboratory were so uncontrolled and so sloppy that those studies don't mean anything" [*Science* **248** 1301 (1990)].

In a recent abstract submitted to the BYU meeting on Anomalous Nuclear Effects in Deuterium/Solid Systems (October 22–24, 1990), Wolf *et al.* were very forthright in their statement that tritium was not being produced by D_2O electrolysis. The authors stated:

> A test of the reproducibility of tritium production in D_2O electrolysis with Pd–Ni–LiOD cells has proved to be negative. The results of Packham *et al.* [*J. Electroanal. Chem* **270** 451 (1989)] are considered to be spurious, and are attributed to tritium contamination. An extensive study with over 100 electrolytic cells has shown that the frequency of occurrence of tritium sightings is explained by the tritium contamination found in the palladium metal stock. A cold fusion mechanism is not supported and implications are discussed for many reported tritium sightings in other studies which were conducted without sufficient blank and control experiments.

Neither the Martin nor the Wolf groups, both at Texas A&M University, observed any electrochemically produced tritium in their respective large series of experiments run under well-controlled conditions. These authors have struck a death blow to the often repeated reports by the Bockris-Packham group of large amounts of tritium in their cold fusion cells.

The evidence supporting the conclusion that the high levels of tritium reported by the BARC and Bockris groups is *not* nuclear in origin (i.e. not coming from the D+D $\rightarrow$ T+p reaction) is extremely persuasive and summarized below.

(1) Direct searches for the three-MeV protons from the D+D $\rightarrow$ T+p reaction were all negative, with upper limits orders of magnitude below the reported tritium intensity. One experiment pushed the upper limit for protons to a value much smaller than the neutron yield reported by Jones *et al*. for the D+D $\rightarrow$ ^{3}He+n reaction.

(2) The reported neutron to tritium (n/T) branching ratios of 10^{-6} to 10^{-9} disagree by factors of 10^6 to 10^9 with measurements that showed that the ^{3}He+n and T+p branches [see reactions (1a) and (1b) on p. 6] from

D+D fusion are approximately equal at energies of a few keV. Calculations confirmed that the magnitude of the Oppenheimer-Phillips effect is less than a few percent for D targets bombarded with low energy deuterons and that this process is irrelevant for enhancing the T+p branch in cold fusion.

(3) The reported neutron to tritium (n/T) branching ratios of 10^{-6} to 10^{-9} are incompatible with the D+D → T+p reaction occurring within a palladium cathode surrounded by D_2O because of the secondary neutrons (14–MeV) that are generated by the 1.01 MeV tritons (D+T → ^{4}He+n).

(4) Direct searches for the Coulomb-excitation gamma rays expected from the interaction of the three-MeV protons (see reaction 1b on p. 6) with palladium have all been negative.

(5) Tritium impurity has been found by Kevin Wolf in palladium purchased from Hoover and Strong.

(6) None of the approximately 200 cells which were run under well-controlled conditions by the Martin and Wolf groups, colleagues of Bockris at Texas A&M University, showed any nuclear-produced tritium.

In spite of the negative evidence, Bockris *et al.* in a recent paper [*Fusion Technology* **18** 11 (1990)] set forth what they considered to be the two main characteristics of cold fusion. They stated "One is the large tritium to neutron ratio, on the order of 10^8 and the other is the sporadicity and irreproducibility of the phenomena." Such a characterization of cold fusion will, I am sure, surprise almost everyone, friend and foe of the phenomenon. Bockris *et al.* went on to say "A suitable cold fusion theory or model *must* explain both of these two features." Bockris' dislike for conventional nuclear physics and nuclear theory have been stated previously. He wrote to the DOE/ERAB Panel on June 12, 1989 and stated that:

> We are particularly unenthusiastic in the discussion of the application of present theories of fusion in plasmas to the idea of fusion in electrochemical confinement because we think that the difference of conditions, particularly in respect to screening by electrons of the deuterium deuterium interactions is an extreme one . . . Historically, when new science is emerging, it is often reviled and denigrated until the new paradigm is accepted . . . We think that, in attempts to verify a newly claimed phenomena, negative results have much less value than positive ones. Negative results can be obtained without skill and experience.

I find Bockris' statement about negative results, which were obtained by a large number of outstanding research groups working on cold fusion, one of the most eccentric evaluations of experimental results that I've ever heard of. There is ample evidence, however, that Bockris' strange attitude on negative results is held by other proponents of cold fusion also. During the spring meeting of the Electrochemical Society (5/8/89) in Los Angeles, a call for papers issued for a session on cold fusion, specified that only papers confirming cold fusion would be accepted. The organizers justified this decision on the grounds that "since the session is on cold fusion, research that doesn't find fusion would not be relevant." The same restricted selection of data was attempted at the Santa Fe Workshop when an evening session on May 23, 1989, was announced requesting only those papers with positive results. A rebellion by a small group of people resulted in the acceptance of a negative paper. Bockris in the above paragraph also displays his strong belief that the chemical environment will alter the well-known nuclear properties of the D+D fusion reaction.

As one might have expected, out of the smorgasbord of models listed by Bockris *et al.*, only their model "can effectively explain both the sporadicity and irreproducibility of the cold fusion experiments . . . and also the large tritium to neutron ratio." How is it possible for any model to explain or account for these assigned most unusual characteristics of cold fusion? First, consider why the proponents of cold fusion have placed so much importance on tritium. David Worledge of the Electric Power Research Institute (EPRI) stated the case for tritium clearly. He wrote "We shall see that the evidence on tritium generation is the strongest of the three types of evidence for cold nuclear reactions, i.e., tritium, neutrons and heat. It seems no longer reasonable to assume these results are necessarily wrong solely because of theoretical improbability based on current understanding" (*Proc. of the First Annual Conference on Cold Fusion*, p. 252, 1989). This is a striking case where the observation of the BARC and Bockris groups came into direct conflict with the long-established branching ratio of approximately unity for the yield ratio of the T+p and ^{3}He+n branches from the D+D reaction, as measured down to deuteron energies of a few kilo-electron volts (keV). Established nuclear theory predicted that this branching ratio would change by only a small amount when the deuteron energy is reduced to thermal energies. Even though no other research groups in the world had been able to produce these high levels of tritium in their experiments and the BARC and Bockris tritium to neutron branching ratios were more than a million (Bockris stated on the order of 10^8) times larger than predicted on the basis of established experiments and nuclear theory, the proponents of cold fusion chose to accept miracle two, and to make this large ratio one of the characteristics of cold fusion. In addition, this large ratio was internally inconsistent due to the absence of secondary neutrons that would result from the tritium interacting with the deuterium in and around the palladium cathode. Therefore, it was ironic that the large tritium to neutron ratio claimed by the proponents to be an important characteristic of cold fusion, actually served to prove that the tritium was not coming from the D+D fusion reaction! Still Worledge of EPRI (an organization that funded cold fusion research) thought that tritium was the strongest evidence for cold fusion.

On what basis did the proponents of cold deuterium fusion justify the large branching ratio in favor of the T+p branch over the ^{3}He +n branch [see reactions (1b) and (1a) on p. 6]? Here the proponents relied on *conventional* nuclear physics theory, in particular, the well-known Oppenheimer-Phillips process. Oppenheimer and Phillips [*Phys. Rev.* **48** 500 (1938)] noted over fifty years ago that the Coulomb field of the target nucleus acts only on the proton in the deuteron and not on the deuteron's center of mass. This leads to an effective polarization of the deuteron, and a mechanism for enhancing the D+D $\rightarrow$ T+p reaction relative to the D+D $\rightarrow$ ^{3}He+n reaction, in the direction required by the BARC and Bockris reported data. Crucial, however, is the magnitude of the Oppenheimer-Phillips effect and its variation with energy, particularly at the very small energies of interest here. Koonin and Mukerjee [*Phys. Rev.* **C42** 1639 (1990)] found that the effect is less than a few percent and irrelevant to low-energy D+D reactions. Recall that the claims of cold fusion proponents at BARC and Texas A&M require an enhancement in the T/n branching ratio of one hundred million. For such a result, miracle two is indispensable.

And what about the other main characteristic of cold fusion, namely that it is sporadic and irreproducible? The Bockris group had suggested that fusion occurred on the cathode surface where dendrites (whisker-like projections) grow during prolonged electrolysis on the electrode surface. As evidence of a surface model, they appealed to palladium isotope ratio changes during electrolysis, data that were presented at the NSF/EPRI Workshop, but now known to be incorrect (*Proc. of the First Annual Conference on Cold Fusion*, p. 272, 1990). Reproducibility is an important factor in science, especially so when insufficient blanks and controls have been run and all the associated questions have not been completely resolved. Hence, to have as one of two main characteristics of cold fusion its irreproducibility is to put the phenomenon on a subjective, non-scientific basis. To be accepted as an established phenomenon, an experiment in physical science must be reproducible. A set of instructions must be available to allow a competent and suitably-equipped scientist to perform the experiments and obtain essentially the same result.

It is not at all unusual for pioneering experiments to be affected by poorly understood factors. However, when there is a real, new phenomenon involved, this variability can be shown to others. For example, in the early days of semiconductor science, it was easy to demonstrate that some germanium crystals were better electrical conductors than others. The fact was indisputable, since the pieces were in hand and their resistance could be measured again and again. For real, new phenomena, the sources of variability in an experiment are systematically discovered and brought under control; for example, by the control of trace impurities in semiconductors. There has been no sign of this growth of understanding of cold fusion either in the production of fusion products or excess heat.

Dramatic new effects in laboratory science often meet a skeptical scientific community. This is as it should be since systematic skepticism is very critical in science and serves to minimize individual subjectivity. Whether new findings are ultimately accepted depends not on how closely they conform to the prevailing scientific dogma but on whether they can be reproduced at will anywhere in the world. Pasteur's discovery of right- and left-handed molecules and Rayleigh and Ramsay's discovery of the rare gases are well-known discoveries which, by the overwhelming force of reproducible experiments, overcame bitter resistance in the scientific community. The few research groups that have claimed to produce large amounts of tritium have not demonstrated it to scientifically competent outside observers. In fact all of the available evidence indicated that the high levels of tritium claimed by the BARC and Bockris groups were not due to deuterium fusion via the reaction $D+D \rightarrow T+p$.

As Wolf *et al.* have stated, many of the cold fusion experiments reporting tritium have been carried out without sufficient blank and control experiments. In an effort to establish credibility for cold fusion, most proponents are much more interested in counting the numbers of groups claiming positive results than examining whether their claims have any validity. For example, in an effort to nullify an outstanding paper reporting negative results, the editor of *Fusion Facts* stated ". . . that over ninety scientists have replicated cold fusion . . ." [*Fusion Facts* **2**, no. 4, October (1990), p. 30]. In a September (1990) preprint (lecture at the World Hydrogen Energy Conference in Honolulu, July 24, 1990) entitled, "Is there evidence for fusion under solid state confinement?", Bockris claimed that seventy-nine groups in sixty laboratories in twelve countries have been successful in reproducing cold fusion. Anyone familiar with the field can strike case after case from these lists, because either the authors have retracted their original claim or the claim is unsubstantiated and not reproducible. A single well-researched and careful experiment with sufficient blank and control experiments giving a consistent set of positive results would be much more impressive. One such result, however, with excess heat and a commensurate amount of fusion products still does not exist!

In view of the above conclusive evidence that the high amounts of tritium were not coming from the $D+D \rightarrow T+p$ reaction, the most likely source is contamination. However, the Bockris' levels of tritium greatly exceed the level of contamination in palladium found by Wolf. This suggests that the Bockris cells were either run under slipshod conditions facilitating high-level tritium contamination or the tritium was knowingly introduced. Bockris raised the latter possibility himself in his first paper on tritium [*J. of Electroanal. Chem* **270** 451 (1989)], but went on to argue that it wasn't likely (see previous discussion). The recent discovery by Wolf that some Bockris cells rich in tritium contain large amounts of light water has raised the issue of possible fraud and brought the discussion out in the open [*Science* **248** 1299 (1990)]. The presence of light water is consistent with the hypothesis that the Bockris cells were spiked with tritiated water, however, does not prove it. Fraud has often been defined such as to encompass a wide spectrum of behaviors. It can range from selecting only those data that support a hypothesis while concealing other data to outright fabrication of results. The incidence of fraud in physical science is usually assumed to be low, because these experiments are reproducible and can usually be verified in a short time, in contrast to some areas of the life sciences. The characterization of cold fusion as irreproducible leaves it vulnerable to the possible charge of fraud. The effects of fraud on other scientists and the public can be devastating. However, as other investigators skeptically review and attempt to verify previously reported data, fraudulent data and hypothesis are eventually uncovered. Valuable time and research funds are wasted in setting the scientific record straight.

Serious questions had been raised about the high levels of tritium in the Bockris cells from the beginning. Suspicions are bound to be raised when the reported data violate established nuclear physics and large amounts of tritium cannot be reproduced by any experimental group in the world except experimenters at BARC (and here under very different conditions). How was it possible that these two groups could produce large amounts of tritium, within short times after initiating cold fusion experiments, when no other groups in the world have been able, up to this time, to reproduce their miraculous results? In view of the cloud of suspicion that hung over the Bockris experiments, he did little to insure that his cells were adequately protected from possible interference. At one point Bockris removed a fifth-year graduate student from the tritium work, only to have him reappear some three months later with yet two more cells containing tritium! Others in the Bockris laboratory relayed their suspicions about the source of tritium to Bockris only to be rebuffed by Bockris with the declaration that a number of groups had verified his work. The only recourse for unhappy researchers was to leave the Bockris laboratory [*Science* **248** 1299 (1990)].

The Texas A&M administrators at all levels were reluctant to investigate what was occurring in the Bockris laboratory. Professor Charles Martin, a colleague of Bockris in the Texas A&M Chemistry Department, went to Dr. Michael B. Hall, Head of the Chemistry Department, and voiced his suspicions. "I warned Hall that I thought there was a very good chance the experimental results were the result of fraud" [*Science* **248** 1299 (1990)].

to "explain" unconfirmed data which could not be reproduced in most laboratories. Advocates even suggested that high-energy nuclear gamma rays could be concealed by a miraculous process in which the entire reaction energy was transferred into the solid lattice! The 'theory' of Walling and Simons (see Chapter III) of the University of Utah is the ultimate example of Langmuir's fourth criteria of pathological science. It is ironic that following this contribution Simons, one of the co-inventors of cold fusion, was named the first Henry Eyring Professor of Chemistry at the University of Utah (*Chemical and Engineering News*, August 28, 1989). Recall that Blondlot received the Leconte prize after Wood's exposé of N rays.

The cold fusion claims have been riddled with inconsistencies. The many scientific groups that could neither produce excess heat nor produce fusion products in cold fusion experiments were deluged with excuses for their failure from the believers. These excuses ranged the full gamut from "it's hard for them to see the effects because they are doing it wrong" to the caustic statement that negative results are of little significance because "they do not require any special skill or expertise in electrochemistry." Bockris and others claimed the effect does not turn on for many weeks of electrolysis and this explained to the believers the negative results from 'Eastern establishment' universities who ran for shorter times. However, this explanation was inconsistent with the results of the BARC groups who had success the first day of electrolysis. Some believers claimed high currents were required while the original Fleischmann and Pons' paper reported excess heat with currents as low as 8 and 64 milliamperes per square centimeter of palladium. When Pons was faced with the evidence that his cells produced no fusion products during a critical five-week period in May and June, 1989, he retorted that his cells were not producing excess heat during this period, even though this was inconsistent with a statement he made on August 16. Some proponents of cold fusion claimed negative results were due to the D/Pd atom ratio being below unity, while at the same time many of the believers were reporting success with ratios considerably below one. Measured against Langmuir's fifth criteria, cold fusion certainly exhibits this symptom of pathological science.

The ratio of the worldwide positive results on cold fusion to negative results[29] peaked at approximately 50% some five to six weeks after the March 23, 1989 press conference, qualitatively in agreement with Langmuir's sixth criteria. Morrison[29] has shown, however, that the positive to negative ratio is very sensitive to the geographical region under consideration. Since many of the negative results have not been published and most groups getting negative results soon returned to their own fields of research, most present activity in cold fusion is limited to believers. Fritz Will, the

29 Douglas Morrison, *Cold Fusion News* No. 20, October 20, 1989.

Director of the National Cold Fusion Institute, has prepared a list of some 100 groups that claim to have produced at least one of the phenomena associated with cold fusion, i.e. either heat or one of the fusion products. This list, however, doesn't contain a single entry where the claimed heat is accompanied by a commensurate number of fusion products. Most of the entries in the list do not meet the standards of the peer-review process for journal publication. It is unfortunate that so much emphasis has been placed by the proponents on the length of this list of claims rather than on the confirmation and reproducibility of a single claim. When discussing the quantity of evidence, one must remember that in killing polywater Deryagin wrote off ten years of work in a published two-sentence retraction.

Morrison has extended Langmuir's study of wrong results in science and has added a number of additional characteristics over the last fifteen years to help in the identification of pathological science. In his detailed analysis, Morrison finds that cold fusion has a large fraction of his characteristics which define pathological science. He concludes that cold fusion is best explained as an example of pathological science (Lecture at the World Hydrogen Energy Conference, July 24, 1990, Honolulu; CERN preprint, CERN/PPE 90–159).

Believers in cold fusion will continue, at least for some period of time, to report claims of success. This pattern of continuing claims has been well established for other areas of pathological science such as N rays and polywater. What is surprising, however, is that these far-out claims are still reported with a positive flavor by leading newspapers (*Wall Street Journal*, April 8, 1991, and *New York Times*, April 14, 1991) before the most elementary confirmation checks have been made. Both of these newspaper articles describe recent experiments by a group of chemists at the Naval Weapons Center in China Lake, California, and give some scientific respectability to the chemists claim of producing ^4He in an electrochemical cell (see Chapter VIII). The human desire for a way to turn hydrogen, the most abundant fuel in the universe, into a limitless supply of clean, cheap fuel is so strong that people want to believe that there is something scientific about cold fusion.

One of the latest episodes to hit the press is a release by Randall L. Mills announcing a new method for generating enormous amounts of energy during the electrolysis of water, not by a nuclear process as claimed by Fleischmann and Pons, but by a *chemical* process (a power output of 37 times the power input!). In Mills' process the source of heat is the "electrocatalytically induced reaction whereby hydrogen atoms undergo transitions to quantized energy levels of lower energy than the conventional ground state"! Aside from the claimed excess power gain of hundreds of times that of Fleischmann and Pons, one of the most striking things about this unconventional claim is that it will be published in the *Journal of Fusion Technology*.

More than two years have elapsed since Fleischmann and Pons made their startling claim to have observed nuclear fusion at room temperature. At the present time the ratio of experimental papers reporting "positive" to "negative" results is much larger than that during the early months. Proponents of cold fusion have attached particular importance to this trend suggesting that the experimental evidence for cold fusion has improved significantly over the last two years. A more realistic interpretation of the present larger ratio acknowledges the sharp decline in the number of negative papers due to the fact that many of the earlier experimenters failed to produce any evidence for cold fusion and returned to their own special fields of research. Hence, the present experimental and theoretical work in the field of cold fusion is limited mainly to believers who continue to report claims of positive results. The membership in this club of believers reporting positive results has remained essentially unchanged over the last year.

Two recent reviews of cold fusion illustrate that the cold fusion phenomenon lives on, at least in the minds of believers. One review by M. Srinivasan of BARC is entitled "Nuclear Fusion in an Atomic Lattice: An Update on the International Status of Cold Fusion Research" is published in *Current Science* [60 417 (1991)]. The other review by Edmund Storms of the Los Alamos National Laboratory is entitled "Review of Experimental Observations About the Cold Fusion Effect" and is published in *Fusion Technology* [20 433 (1991)]. These two reviews serve a useful purpose by compiling 174 and 366 references, respectively, to the cold fusion literature. However, neither of these compilations include all results nor do they critically compare the data presented. A more extensive bibliography with currently over seven hundred references to cold fusion has been compiled by Dieter Britz of Aarhus University (this bibliography which usually gives a concise summary of each entry, is distributed electronically; a copy can be obtained through the Cornell Cold Fusion Archives).

Srinivasan included an interesting explanatory letter with his review where he states:

> [I]n my judgement (there is) very convincing evidence for the authenticity of the (cold fusion) phenomenon and its nuclear origin . . . I am amazed to see the intense anti-cold fusion propaganda launched by a handful of influential people at the international level.

Srinivasan's belief that the negative point of view on cold fusion which has developed over the last two years is due to a handful of influential skeptics is strong evidence of self-deception, a characteristic of pathological science. In sharp contrast to Srinivasan's view on cold fusion, most scientists have written off cold fusion at least at the level claimed by Srinivasan, where the process produces watts of excess power due to a nuclear process. By Srinivisan's own testimony only a small band of "about 600 scientists" the world over continue to study cold fusion (this number seems rather large). If the scientific community at large believed that cold fusion was a potential source of abundant, cheap and clean energy can you imagine most scientists the world over abandoning cold fusion research? Hence, it is entirely unrealistic to place the blame for the current "back-to-the-wall" predicament of the cold fusion proponents onto a highly vocal small group of dissenters. For example, Srinivisan's attempt to dismiss Frank Close's negative book (*Too Hot to Handle*, Princeton University Press, 1991) as the work of a hot fusion advocate is based on the fact that this book was published by Princeton University Press (and it so happens that Princeton University operates the hot fusion facility known as Tokamak)! The same malicious and false accusation of hot fusion interest was leveled by several believers against members of our DOE/ERAB panel in an attempt to discredit our study of cold fusion. For example, Eugene Mallove, one of the more ardent believers in cold fusion, inferred that my negative stand on cold fusion claims is due to the large inertial confinement fusion project at the University of Rochester. Had he taken the trouble to check with anyone at the University of Rochester, he would have learned that I have no association with this project. To assume that skeptics of cold fusion are hot fusion proponents is simply faulty logic and a desperate move to discredit those who find that the evidence for cold fusion is not compelling.

Storms' review is equally optimistic to that of Srinivisan. He concludes,

> The number and variety of careful experimental measurements of heat, tritium, neutron and helium production strongly support the occurrence of nuclear reactions in a metal lattice near room temperature as proposed by Pons, Fleischmann and Jones.

What is the meaning of this sentence where the high-level power claims of Pons and Fleischmann are lumped together in a single sentence with Jones' claim of low-level neutrons? Since these two claims differ by a factor of more than a thousand billion, what am I to believe is strongly supported? In a letter to me accompanying his manuscript, Storms stated:

> Since the ERAB Cold Fusion Panel report was written, considerable information has been published that strongly supports the claims of Pons, Fleischmann and Jones. In addition, a number of new aspects to the phenomenon have been discovered. In view of this new information, the great importance [of] this discovery, and the generally negative evaluation given in the early ERAB report, I would like to suggest that a second look be made by the ERAB panel.

If compelling evidence for cold fusion were discovered in the interval following our panel's final report, Storms' suggestion is a very reasonable one. A thorough study of these two reviews along with the original papers that are referenced therein shows, at least in my opinion, that there is still no persuasive experimental evidence for the phenomenon known as cold fusion.

How is it possible for different people to examine the same experimental claims and reach opposite conclusions? First of all, both believers and skeptics acknowledge that there are a large number of experimental claims (it is immaterial whether the number is 50, 100 or some other number, depending on how the independent claims are counted) supporting some aspect of cold fusion. On the basis of the sheer number of positive claims, it is tempting to conclude, as many believers have, that there must be some truth to cold fusion. Numbers of claims alone, however, are not definitive in science. Recall that several hundred papers were published in support of both N rays and polywater, two classic cases of pathological science. It is imperative, therefore, to dissect each claim and examine its validity. This close in-depth analysis of individual claims often leads skeptics and believers to diametrically opposite conclusions.

The unmistakable signature for the occurrence of nuclear fusion of deuterons (D+D) is the production of fusion products (see Table 1 on p. 109). Heat, if due to nuclear fusion, must be accompanied by a commensurate amount of fusion products. Once one abandons this equality, one has left science as it is normally practiced. One careful experiment showing an equality between heat and fusion products would settle the issue. However, two years have elapsed and there is still *not a single claim* where the reported heat is accompanied by a commensurate amount[30] of fusion products! In fact the two quantities differ by many orders of magnitude. This acknowledged large inequality, however, hasn't convinced the believers that the heat cannot be nuclear in origin. Some believers even use the very low-level neutron claims of Jones *et al.* as proof that the reported heat is due to a nuclear fusion process, even though the two claims may differ by up to thirteen orders of magnitude. Based on these very different interpretations of the same reported observations, it is understandable how believers and skeptics, respectively, conclude that the claimed excess heat *is* and *is not* due to nuclear fusion. Fusion heat without fusion products is a high-order miracle which skeptics are not willing to swallow based on the presently

[30] The recent rehashed claim of Bush *et al.* [*J. of Electroanalytical Chemistry* 304 271 (1991)] of a commensurate amount of ^{4}He in the effluent gases from an electrolytic cell is unconfirmed. First, no evidence for the commensurate intensity of the 23.8-MeV gamma rays was presented. Secondly, no ^{3}He was observed as required. Its absence requires a miraculous alteration of conventional low-energy D+D fusion.

reported fragmentary and sometimes contradictory experimental evidence for both heat and particles.

The reviews of Srinivasan and Storms conclude that the present observational claims of producing neutrons, protons and helium from cold fusion experiments employing electrochemical and high-pressure gas cells strongly support the existence of room-temperature fusion in a metal lattice. I disagree completely with this conclusion. My arguments for concluding that no compelling experimental evidence exists for the production of fusion products in metals at room temperature are given in Chapter VIII. Individual scientists will have to examine the primary literature themselves in order to make their own conclusion about the validity of the various claims. The most careful searches for fusion products have been for neutrons. Although fusion neutrons at a very low level cannot be categorically ruled out, the present evidence for room-temperature fusion neutrons is also not at all convincing.

In Section 22 of Srinivasan's review, he lists nine "puzzles" that summarize the experimental claims of cold fusion advocates and that will need to be explained by any cold fusion theory. Each of these several "puzzles" in the nuclear area contradict conventional nuclear physics and require acceptance of a series of "miracles" (see Chapter VIII). Srinivasan does a service to the cold fusion controversy by admitting that the evidence for cold fusion requires acceptance of this series of "puzzles", like, for example, the anomalous T/n ratio of about 10^8 rather than one. Believers and skeptics look at this type of experimental evidence very differently, especially when only two laboratories in the world have claimed high levels of tritium in experiments which are not reproducible. When miracle after miracle are required to accept the claims of heat from room-temperature fusion which are contrary to experience, one is likely dealing with pathological science.

The Second Annual Conference on Cold Fusion was held in Como, Italy, on June 29–July 4, 1991. On the International Advisory Committee were Bockris, Fleischmann, Menlove, Pons, Preparata, Scaramuzzi, Srinivasan and Will, all familiar names as strong believers in cold fusion. One name missing from the International Advisory Committee is Steven E. Jones. Although invited to participate, Jones declined for two reasons. First, he disagreed with the premise that the claimed "excess heat" was due to nuclear reactions. Jones stated that his own research showed that the nuclear products were many orders of magnitude too small to correlate with "excess heat", refuting the notion of Fleischmann and Pons that the excess power is due to fusion. Secondly, at least one member of the committee had used threats of legal action against fellow scientists and colleagues. Jones stated that he could not in good conscience join with such a member on the Advisory Committee.

results are inconsistent with those of both Mills and Noninski! In *Fusion Technology* [22 301 (1992)], Bush states:

> Bush and Eagleton ran a sodium carbonate cell, expecting to see no excess heat. To our surprise, it gave about twice the excess power as a comparable potassium carbonate cell.

Hence, Bush rejects the novel chemistry of Mills and Farrell and substitutes his own model which 'explains' excess heat also with sodium carbonate. Recall that positive excess heat with sodium carbonate violates the Mills-Kneizys-Farrell theory. Now we have the typical cold fusion situation where proponents' experimental results directly contradict one another.

The most sensational results from electrolysis of light-water solutions of alkali salts have come from groups at BARC. These data were presented by M. Srinivasan at the Third International Conference on Cold Fusion. Not only were they extremely successful at producing excess power in most of their cells with H_2O solutions of alkali salts; many cells also generated tritium! The BARC groups now have the distinction of going on record claiming production of tritium with both light-water and heavy-water cells. I discussed their claim of tritium production with heavy water previously and showed that it was probably due to contamination (see pp. 119 to 123). Without confirmation, one must assume the tritium from light-water electrolysis is also due to contamination.

R. Notoya from Hokkaido University had a light water cold fusion demonstration operating on a table outside the lecture hall at the Third International Conference. She had two cells; in one cell was a resistive heater (the control cell), and in the other cell a nickel cathode in a light-water solution of potassium carbonate. The power input into the control cell was smaller by an amount equivalent to the power necessary for electrolysis of H_2O. Hence, if no recombination of the hydrogen and oxygen gases occurred, the power into the control cell matched the joule heating in the open electrolysis cell. Notoya demonstrated that the electrolysis cell was running considerably hotter to the touch than the control cell, implying several watts of excess power. Proponents hailed this demonstration as proof positive for cold fusion with the added bonus that it was occurring in light water.

There was a catch, however. David Buehler (BYU) noticed that the electrical leads into the two cells had very different thicknesses![46] In test experiments at BYU, Buehler and Jones concluded that a large fraction of the power associated with Notoya's control cell was being dissipated outside

[46] This bit of detective work is reminiscent of Wood's exposure of Blondlot's experiment as described on page 202.

A3. Rothwell Memo to Dr. Srinivasan Regarding CNN Interview

314 313

Cold Fusion Research Advocates
2060 Peachtree Industrial Court, Suite 313
Chamblee, Georgia 30341
Phone: 404-451-9890 Fax: 404-458-2404 CompuServe 72240,1256

May 4, 1995

Memo from Jed Rothwell

I met today with Tom Johnson, President of CNN. He asked for the names of the top five cold fusion scientists in the world. After some thought, I faxed him the names and addresses of:

Prof. Martin Fleischmann
Dr. Edmund K. Storms
Dr. Dennis Cravens
Dr. Hideo Ikegami
Dr. Mahadeva Srinivasan

So CNN may contact you. His address is:

Mr. Tom Johnson
President, CNN
One CNN Center, Box 105366
Atlanta, GA 30348-5366

Tel: 404-827-1311 Fax: 404-827-4215

Jed

A4. "Est Idea", Russian Magazine, Article Describing Cold Fusion Research at BARC

Ядерная реакция в майонезной банке

Неожиданную активность читателей вызвала статья «Холодный ядерный синтез - будущее энергетики» («Есть идея!» № 9 (52). Нам часто звонят в редакцию, задают множество вопросов и настоятельно просят дать координаты главного героя этой публикации.

Несколько слов о нем. Ю. Н. БАЖУТОВ - кандидат физико-математических наук, член президиума Российского физического общества, председатель Оргкомитета Российской конференции по холодному синтезу и трансмутации ядер, выпускник МИФИ, работал в Институте им. Курчатова, НИИЯФ (МГУ), ЦНИИМАШ. Сегодня он директор Научно-исследовательского центра физико-технических проблем «Эрзион». Красивое это название от слова эрзя. Так зовется одно из древних мордовских плем... скульптора Степана Э... окрестил эрзионом н... го ядерного синтеза... пользовании этого...

В этой статье ... ктивах.

ПРОШЛО уже ... М. Флейшман ... (ХЯС). Автор... стрировали поток не... подводимую электри... гать, что выделение ... процесса при комнат... синтез, в отличие от и... шего при температуре 10⁷ К.

Пример «горячего» ядерного синтеза - взрыв водородной бомбы. Чтобы использовать в мирных целях огромную энергию взрыва, нужно создать сверхсильные магнитные и электрические поля, сверхвысокие температуры и удержать образующуюся плазму в течение достаточно долгого времени. Над этим пока без существенных успехов бьются лучшие умы человечества уже около 40 лет. Необходимо отметить также, что планируемая стоимость промышленной установки оценивается в полмиллиарда долларов, а срок создания отодвигается за 2030 год.

Открытие М. Флейшмана и С. Понса сулило перспективы реализации ядерного синтеза при комнатной температуре буквально в «майонезной» банке. Это открывало возможности создания дешевого и неисчерпаемого источника энергии.

This article has been written by Dr. U.N. Bazhutov, Director, Scientific Research Centre of Physics Engineering "ERZION" near Moscow. Dr. Bazhutov is a member of the Presidium of the Russian Physics Society and is Chairman of the Organising Committee of Annual Russian Conference on Cold Fusion & Nuclear Transmutation (RCCFNT) held in Russia every year. The article appeared in "Est Idea!", No. 12, sometime in April-May, 1996. "Est Idea" is a popular Science Magazine of Russia

мости от экспериментальных результатов и теоретических представлений. Те исследователи, которые имели в эксперименте большую интенсивность процесса или разрабатыва-

ли теоре... Первым ... ХЯС и ... Бомбее ... ным эко... получил ... да в Киото ...

NUCLEAR REACTION IN A MAYONNAISE JAR

... будущее ХЯС. ...ъ в реакциях ...го центра в ... перспектив... м этот тезис ... ябре 1989 го-

Интересна... ...ий. Если в первом сообщении Флейшмана и Понса в 1989 г. был достигнут уровень дополнительного энерговыделения 10 Вт/см³, а через год на I международной конференции по ХЯС в работе Ориани сообщалось о достижении - 100 Вт/см³, то в настоящее время во многих экспериментах достигнут уровень 1000 Вт/см³. Правда, тут же следует заметить, что результаты эти не имеют пока долговременного характера (до часа продолжительностью) и не обладают 100%-ной воспроизводимостью. Физика процесса до сих пор пока непонятна, и единой общепризнанной теоретической версии ХЯС не существует. Хотя на более низком уровне интенсивности протекания реакций уже достигнута воспроизводимость 70 ÷ 100%.

В силу указанных «каверз» и противоречий, все работы по изучению ХЯС сегодня под ...едь со стороны ученых, действующих в рамках ...процессах. Однако явление это продолжает

...ренции по ХЯС (следующая, 6-я МКХЯС ...ых собирается научная общественность ...о ХЯС изучается в США, Японии, Ита... ...вно работают около 100 научных групп

... 10 независимых научных группах. Рос... ...жегодные конференции по холодному ...брау-Дюрсо-93, Сочи-94, Дагомыс-95, ...только исследователи различных науч... ...(Украина, Белоруссия, США, Япония, ...уществует и работает Межведомствен... ...(при вице-президенте РАН, академике

"The first scientists who after Fleishman & Pons got high intensity in the Cold Fusion reactions and proved suppresion of neutron yield were the physicists from Bhabha Atomic Research Centre, Bombay (June, 1989). They were the first ones to draw a conclusion that Cold Fusion can become a prospective eco-friendly power source for the power engineering of XXI century".

Однако финансовые проблемы не позволяют нашим ученым развернуться в полную мощь. Иначе уже были бы в России свои энергоустановки, работающие на основе этого удивительного явления, утилизирующие радиоактивные отходы, появились бы новые технологии для различных отраслей промышленности. Ведь холодный ядерный синтез - это новая физика, а значит - новая технология.

На снимке: ведущие сотрудники центра «Эрзион». Справа налево - Ю. Н. Бажутов, С. П. Смирнов, В. П. Корецкий, А. Б. Кузнецов.

Фото Б. Савина

A5. "Whither Cold Fusion" – Prepared by Dr. Srinivasan for BARC Senior Management, 1991

Government of India
Bhabha Atomic Research Centre
Neutron Physic Division

8th April 1991

Sub: Whither "Cold Fusion"?

Dear friends and colleagues,

It is now two years since the phenomenon of "cold fusion" burst out into the open. Many of you are aware of the intense activity this had triggered at BARC and how the initial BARC work won international acclaim and recognition. As one who was deeply involved in this effort, I had hoped that by now, two years after it all began, the intense controversy that was generated by the initial announcement of Fleischmann and Pons would have died down and things settled to a phase of quiet and unruffled scientific activity. Indeed things have settled down but for a very different reason - most people have simply written off cold fusion as a dead horse and have tried to forget it! However a small band (estimated to be about 600 scientists at present) of dedicated researchers the world over have continued to study the subject passionately and have indeed obtained, at least in my judgement, very convincing evidence for the authenticity of the phenomenon and its nuclear origins. Unfortunately much of the new results have not yet received the publicity they deserve. As most of you know it takes several months to over a year between submission of a paper and its appearing in print in a journal or conference proceedings. The enclosed review article which is to appear in the April 25th 1991 issue of "Current Science" is an attempt to draw the attention of the Indian scientific community to the very interesting results that have come out over the last one year alone.

I must admit that I am amazed to see the intense anti-cold fusion propoganda launched by a handful of influential people at an international level. Most of the highly critical articles on cold fusion can be traced to a few writers such as Maddox, the Editor of "Nature", Robert Pool of "Science", Douglas Morrison of CERN, Gary Taubes the investigative science journalist and most recently Frank Close a physicist of Rutherford Appleton Laboratory, U.K. who has authored a new and highly critical book on cold fusion. But most of these writers do not refer very conveniently to the large number of confirmatory results which have poured in from different parts of the world in recent months as can be seen from my review article. Frank Close's book for example deals entirely with the events immediately following Fleischmann and Pons original paper, how the gamma ray spectrum presented therein was wrong etc (See Nature of 7th March 1991 for a review of his book). It is perhaps more than a coincidence that Close's book is being published in the USA by the Princeton University which hosts TFTR, the biggest hot fusion Tokamak in the USA! The other sources of virulent attack on cold fusion are MIT and LLNL, also recipients of massive Magnetic Confinement Fusion (MCF) funding.

Many of you must be aware of my efforts, particularly during the last one year to try and communicate the excitement of the cold fusion phenomenon to audiences both at BARC and elsewhere in India. I have also had occasion to discuss the subject with several distinguished scientists both Indian and foreign. I have noted during these dialogues that the reaction of scientists, particularly physicists, to the phenomenon of cold fusion can be classified under one of five categories or "levels of response" as one may label it, as follows:

Level 0: *Those who "believe" or have "become convinced" that cold fusion does not exist, that it is a myth and an illusion, that all the "claimed" experimental results (heat, neutrons, tritium etc) are all explainable in terms of experimental artificates.*

Level 1: *Those who "believe" in the neutron measurements such as that of Jones, Menlove etc but are "convinced" that these can readily be explained and understood in terms of the phenomenon of "fracto-fusion", namely (d-d) reactions caused by deuterons accelerated in the high electric fields generated across cracks and fissures in palladium or titanium deuteride (See Sec.18 of the review paper). But this they say is good old nuclear physics and there is nothing new in it!*

This category of people also dismiss away the excess heat of Fleischmann and Pons as of no "nuclear relevance".

Level 2: *This group of people believe in the low steady rates of neutron production such as that of Jones; They are willing to concede that electron or other screening effects in a solid environment may contribute to a lowering of the coulomb barrier resulting in occurrence of (d-d) fusion reactions at a low rate of "academic interest" only. But apart from this "concession" they are not willing to concede that any other new physics is involved. For example they insist that the neutron-to-tritium yield ratio cannot be anything other than unity. "Any good physicist knows this"; if experiments indicate that tritium yield is high and the (t/n) ratio is 10^8, then either the measurement is wrong (must be "chemiluminiscence") or it must be due to prior contamination of Pd with tritium (sec.4 of my paper discusses the Wolf episode!)*

This group also dismisses the excess heat observations as being due to some manifestation of stored chemical energy; nothing nuclear about it they would say.

Level 3: *This group goes one step further and is willing to accept the branching ratio anamoly, i.e. the tritium yield as being several orders of magnitude higher than that of neutrons. They explain the preferential "neutron transfer mode" of the (d-d) reaction on the basis of a manifestation of the Philips-Oppenheimer process, or as resulting from polarisation effects of the loosely bound deuteron etc.*

But this group too does not believe in "this excess heat business".

Level 4: This group swears by Fleischmann and Pons and fully "believes" that the excess heat phenomenon is of nuclear origin (especially now that He-4 has been detected both in the gas stream as well as in Pd; See Sec.2 of my paper). They proclaim that it is the excess heat that is the "essence of cold fusion". The other virtue of excess heat acclaimed by them is that it is virtually "clean".

Although the differences between Levels 1,2 and 3 would appear to be minor on the face of it, from the physics point of view each level invokes some new physics principle. However the jump from Level 3 to Level 4 is a big one since the physicist has to swallow quite a few "bitter pills" to accept Level 4. Many hard core physicists find crossing this bridge to be the most difficult.

Note that in the above discussion I have used words such as "believe" and "have become convinced" etc. How can different people come to different conclusions looking at the same set of experimental data? This is at the root of the controversy surrounding cold fusion.

What does the presently available experimental evidence really "prove"? For this you are the best judge! I offer the enclosed review article merely as a guide to the literature. I urge you and entreat you to go to the bottom of it for yourself. Nothing like reading the original papers and references. But for heavens sake don't "accept" the opinions, pronouncement interpretations and declarations of any intermediary as final, not even this reviewer. To me it has been a very painful experience to see responsible people pass judgements and opinions without reading any of the original new experimental papers. It is with a view to help you in this effort that I offer this review paper containing 173 references. My review is bound to be somewhat "biased", but you are welcome to go to the original sources of information, and indeed you should. However, irrespective of your present level of response, I feel confident that you will move atleast one rung up the ladder after reading my review article and possibly one more if only you would take the trouble of reading at least 10 good experimental papers giving positive results!

Mercifully, to settle the question as to whether the cold fusion phenomenon is authentic, whether the accumulated experimental evidence is adequate to give a clear cut digital answer to the question: "Do nuclear reactions occur in a deuterated solid such as Pd or Ti?" it requires only one week's time and patient effort, with an open mind of course!

I have meanwhile tried to pin point for myself what could be the causes for the widely prevalent skepticism towards cold fusion. I have indeed dealt with some of these issues in the paper, but there are some things that one can't quite include in a scientific paper.

(i) The most important reason for the skepticism is the non-reproducibility. Admittedly anything that is not reproducible or repeatable is against the spirit of science. But is it right to settle the question by putting it to vote? Invariably whenever I discuss cold fusion with my skeptic friends the response to my statement that "I can show you 10 papers where they have seen neutrons", they would quip "I can show you 100 who have seen nothing!" In my assessment even if <u>one</u> good carefully conducted experiment shows that neutrons are being emitted, that is enough to clinch the issue. If there are 10 good reliable papers it is even better! But to say 100 people tried and failed and only 10 succeeded and therefore the phenomenon does not exist is complete nonsense. As I have said in the review article it is obvious that there are some as yet unknown parameters/factors which seem to be determining whether or not an experiment is successful. Bush has for example (Ref /137/ of my article) brought out succintly some of these factors. The success rate however has been steadily improving in the recent experiments.

(ii) A second possible reason for the skepticism is the poor quality of the early experiments. The sloppy early work has no doubt given a bad name to cold fusion. This is understandable because in the flush of enthusiasm many groups simply used whatever equipment was readily available/accessible to them. (This is true of some of the early BARC work also). But one can't go on harping on the poor quality of the 1989 work! I urge the critics to study the newer experiments; the more recent results. As I have said repeatedly many people are simply not aware of the new good work and results. The purpose of the enclosed review article is precisely to help you in this.

(iii) Lastly much of the criticism can be attributed to the number of "miracles" involved. There are many preconceived notions and concepts that one has to give up before accepting the cold fusion phenomenon as part of physics. I have summarised these in Section 22 of my article under the heading "Puzzles of the Cold Fusion Phenomenon". It is the new physics that the phenomenon has unlocked that makes it really exciting.

I sincerely hope that this will initiate a fresh debate in India so that we can all put our heads together and get to the bottom of it all. If you do get converted into agreeing that the phenomenon is as fantastic as the Level 4 believers proclaim, then it is obvious that it is too important to the future of nuclear energy technology to be ignored and worse still ridiculed!

Happy reading!

Yours sincerely,

M. Srinivasan

(M. Srinivasan)

To

All Members of TC/TSC

A6. "Paradigm Shifts Which Can Drastically Affect Our Extrapolations/Projections" – Prepared by Dr. Srinivasan for BARC Senior Management, 1995

30th May, 1995 .1100 hrs

SEMINAR ON VISION 2020
(July 19th-21st 1995)

PARADIGM SHIFTS WHICH CAN DRASTICALLY AFFECT OUR EXTRAPOLATIONS/ PROJECTIONS.

M. Srinivasan
Associated Director
Physics Group, BARC

The Department of Atomic Energy has been charged with the responsibility for developing the technology of commercial Nuclear Power and carry out R & D in associated areas encompassing the entire nuclear fuel cycle. The promotion of various applications of radio isotopes in industry and agriculture is the second major responsibility of DAE. In the nuclear power sector the guiding philosophy has been the "Three Stage Nuclear Power Programme" originally formulated by Dr. Bhabha in the early 50s. Thus the epoch making discovery of nuclear fission which occurred over half a century ago is the original embryo from which the entire nuclear industry has grown the world over.

In formulating a plan and vision for the next quarter century, it seems to me, that it is automatically assumed and taken for granted, that this half a century old discovery, namely fission and only fission can and is going to provide a means of tapping the energy locked within the nucleus or indeed of any other source of energy. Has the scientific creativity of man come to a standstill that we take for granted that no new scientific breakthroughs are going to occur or possibly has even occurred already under our very eyes? A deliberation such as Vision 2020 is obviously the apt forum to examine such questions. In such a discussion involving long term projections it would not be wise on our part to keep a blind eye to the possibility that some new discovery/invention may take place or has already taken place which can completely and totally upset all our thinking and calculations!

Most of you might have already guessed that what I am driving towards is the new phenomenon of "Cold Fusion". I am fully aware of the highly skeptic reaction of most senior scientists to the phenomenon of "Cold Fusion". I however consider myself lucky to have had the opportunity to interact with almost every single important cold fusion researcher in the world and to have listened to their talks and also to have discussed with them in detail their experimental results. I have attended four out of the five international conferences on Cold Fusion held in the six years since the announcement of the discovery of the phenomenon. Besides, a number of experiments carried out by myself and my colleagues at BARC have fully convinced us that cold fusion is for real. Hence when I "claim" that a new method of releasing what appears to be nuclear energy, in small table top systems has been discovered, I say that with all the seriousness and responsibility that such a statement deserves. At ICCF-5 held at Monte Carlo during April 1995, throughout the four day meeting a light water cold fusion cell which has been granted the first patent for a cold fusion device in the USA in december 1994 and known as the Patterson Power Cell generating excess power of upto 600% was kept operational in the lobby. We were informed that this cell has been taken up for possible commercialization by the Bechtel Corporation of USA. The international cold fusion community has a strength of almost 1000 researchers today. I can of course go on to give a long lecture on what has been observed and achieved so far the world over and also why the scientific community by and large has found it difficult to accept the experimental findings etc. But this is not the occasion to go into details of that nature.

Let us for the moment leave the question of the reliability and validity of the experimental findings apart, and examine briefly only the question of what would be the impact on our entire nuclear power programme if such a discovery has indeed taken place. Let us assume that by the year 2000 the cold fusion community comes up with a device in the market place producing power in modules of 10 to 20 KW, using only ordinary water or at best heavy water, as "fuel". Is it not going to have a deep and direct impact on the decision making authorities responsible for allotting funds for various R&D programmes related to future nuclear power generation technologies?

One of the most important features of Cold Fusion type devices is their potentially small capacity, namely 10 to 20 KW range, ideally suited to serve the needs of a single home/family. It is therefore of appropriate size for mass production by private industry. Patents of the original inventors / enterpreneurs would be bought over by some of the large industrial houses such as Toyota, Mitsubishi, Fiat corporation and so on who would then help launch cold fusion devices in world markets. This is the vision of the unfolding scenerio, regarding emerging cold fusion technology.

In this context it is worth remembering that an automobile is essentially a type of "power plant" converting chemical energy of fossil fuels to mechanical energy and to some extent even electrical energy (the dynamo in the car). Taking the case of the Indian automobile industry as an example, I understand that the annual production of four wheelers in India is about 400 thousand units. If we take a conservative figure of 12 KW for the power capacity of an automible engine, we arrive at a figure of ~5000 MW of new power generation capacity being added every year by the indian automobile industry alone! Remember that in an automobile hardly 20 per cent of the total investment goes towards its power plant. Automobile manufacturing factories have been set up in as short a time as 18 months by some of the major industrial houses of Japan, USA and Germany for example! The essential point I am driving at is that the small size of cold fusion based power generators makes it ideally suited for exploitation by private industry. Going by what we have witnessed in the electronics industry, particularly the personal computer revolution, it is envisaged that intense competition is going to propel invention and innovation leading to rapid technological advance in the market place. Cold Fusion therefore need not wait for government operated R&D programmes which we all know take their own time to bear fruit! The first commercial cold fusion systems are expected to be simple heaters for hot water and home heating during cold winters and may even be employed for air conditioning units in tropical countries.

It is therefore obvious, that if what I claim is indeed true, namely that a new and fundamental revolution has taken place in physics and that a new method of releasing nuclear energy inside hydrided/deuterated metallic lattices in a "clean way" has already occurred, then I am sure that most people will agree with my contention that this singular invention is going to upset all our calculations and projections in the time frame of the next 25 years!

The only question to be answered is whether the claims of the cold fusion community are in fact true. Recently at BARC a group of about 50 scientists engaged either full time or part time in cold fusion studies, got together and formed a "Cold Fusion Forum". A half a day informal Seminar on "Current Cold Fusion Research at BARC" is being organised on June 22nd, 1995. If there are still some doubts regarding the reality or importance of cold fusion, a study group of young scientists in the age group of 35 to 45, may be appointed to independently go though all the literature which has been generated in the past six years in the area of cold fusion in order to independently assess the status of the subject.

Apart from what I have said above, it is also my assessment that for various reasons, not all of which are scientific, the world is not going to permit India to develop the technology of fast breeder reactors! To say that we will go it alone and develop the capability to build and operate commercial fast breeder reactors independent of the developing nations is, in my opinion being somewhat unrealistic. There are many special components which we are obliged to import. In my opinion electricity generating fast breeder reactors are not going to become a reality in the Indian context in the next 25 years. It has something to do with the obsession of the advanced countries on the non proliferation issue, and with the fact that following collapse of the Soviet Union and dismantling of some nuclear weapons of both USA and Russia, there is now a surplus of plutonium in the world. There is therefore more concern about "burning" Pu than "breeding Pu". Of course questions have also been raised about the economics and safety of fast breeders, at least in some quarters, as compared to thermal reactors. But on the whole there is more politics than scientific issues involved, while discussing FBRs. But the net result is that fast reactors are most probably not going to become a reality in India in the time frame leading to 2020!

Thus in a way it is a fortutious coincidence that when one source of hope in the form of fast breeder reactors is withdrawn, a new source of hope in the form of "cold fusion" and other such possibilities has emerged / is emerging. I therefore strongly recommend that we should admit the possibility of occurrence of new breakthroughs in nuclear energy technology and in order for DAE not to be left behind in this fascinating new science, it should do everything possible to encourage young scientists to take up such studies. Luckily cold fusion research does not cost much; it is inexpensive research, and very interestingly BARC has the right background to emerge as the premier cold fusion research centre of the world! Besides, since the new field is just growing, there is also the prospects of staking many original patents in the field, leading to possible financial pay-offs for the institution in the future.

[A more comprehensive write up including a brief summary of the current status of cold fusion research based on the papers presented at the Vth International Conference on Cold Fusion held at Monte Carlo (Monaco) duruing April 9th - 13th, 1995, is under preparation.]

Appendix B. Dr. Srinivasan Interviews

As noted in Section 3, Dr. Srinivasan's LENR research career as set forth in his autobiographical sketch (Section 2) is amplified with additional detail in three interviews:

- 2019, with Dr. Grimshaw at ICCF-19 in Assisi, Italy (Appendix B1)
- 2011, with Marianne Macy for Infinite Energy magazine (Issue 95) in conjunction with Dr. Srinivasan's leadership for ICCF-16 (Appendix B2)
- 1994, with Russ George for Cold Fusion magazine, accomplished while Dr. Srinivasan was researching LENR with Dr. McKubre at SRI International (Appendex B3)

Transcripts of these interviews are included in Appendix B. Gratitude is expressed to Marianne Macy and Russ George for their permission to include the interview transcripts in this report.

B1. 2019 Interview at ICCF-22, Assisi, Italy

Dr. Grimshaw conducted a three-part interview with Dr. Srinivasan during ICCF-22 at Assisi, Italy in September 2019. The interview covered the first three of Srini's six research phases described in Section 2 of this report.

Srinivasan Interview, September 10, 2019
Part 1

Tom Grimshaw: Okay, this is Tom Grimshaw. I'm here with Mahadeva Srinivasan, who also goes by Chino Srinivasan. This is September 10th, I believe it's Tuesday September 10th, 2019. We are in Assisi, Italy at ICCF 22. We're both attending the conference and the purpose of this interview is to supplement previous work that Chino and I have done under the umbrella of the LENR research documentation initiative. We're doing a project under that initiative to document Chino's LENR or cold-fusion research. Going back to the very earliest days in March 1989. So Chino, with that introduction, my first question is, when the March 23rd announcement took place, what were you doing and how did you hear about the announcement and what was your initial reaction please?

Chino Srinivasan: Right. Obviously because of the almost twelve hour time gap between United States and India, especially Utah, where the press conference took place, we learnt about it from a short news item that appeared in the Times of India newspaper, on the morning March 24th. If I recall correctly it was a Monday.

At that point in time, our group was already engaged in hot fusion related experiments and I happened to be the head of the Neutron Physics Division at the Bhabha Atomic Research Center (BARC). So when we saw the announcement that somebody had conducted a fusion experiment in a test tube, which also indicated the emission of neutrons, it naturally got us very excited. What really excited us was the statement that neutron emission had been observed.

Now, let me give a little bit of background. At that point in time, India's nuclear power planning, was based on what was called a “three stage nuclear power program”. In the first stage, the Indian government Department of Atomic Energy planned to build a series of Pressurized Heavy Water Reactors using natural uranium and heavy water, the kind of nuclear reactor developed in Canada and India had already jumped into that.

The second stage was to use the plutonium coming out of the first stage PHWR type Nuclear Power Stations and mix it up with natural uranium and deploy this type of fuel rods in Plutonium-Uranium Fast Breeder reactors (FBRs). These sodium cooled Fast Breeders Reactors (FBRs) would “breed” additional plutonium over and above what is burnt in situ to generate electrical power as well as surplus Pu.

In the Third stage the surplus Plutonium was to be mixed with Thorium and used to fuel converter reactors which would burn the Pu fuel but in the process generate U-233 from the Thorium. In the long term India’s Energy requirements would be met by the so called Thorium- U-233 fuel cycle.

The second stage fast breeder reactors using uranium and plutonium then would multiply the stocks of plutonium which would then be used in conjunction with Thorium in the third stage. So we will have thorium as the fertile material and plutonium as the fissile material. And such reactors would be producing a large amount of excess uranium-233 which will help India launch the Thorium U-233 cycle. Why the thorium U233 cycle? Because India has one of the largest reserves of thorium in the mineral rich beach sands in the southern state of Kerala. In contrast India's reserves of Uranium are very limited.

We thus recognized that neutrons are the key to converting thorium to fissionable U-233. We were already exploring whether neutrons can be generated externally in a non- fission process and for converting thorium to U-233. For example one of the strategies that was being discussed at that point in time was to use accelerator produced neutrons to convert thorium to U-233 and then deploy the same directly in "Thermal neutron based breeders"; the thorium U-233 fission reactor cycle has the potential of having a breeding ratio of almost unity. So once you start a nuclear reactor the thorium and U-233 can be recycled and you don't need to feed in any more fissile materials. Whatever U-233 is generated can be recycled and that was the strategy formulated by the first founder - chairman of the Indian Atomic Energy Commission, Dr. Homi Bhabha. We were thus already discussing prospects of deploying externally generated neutrons in this process. So when we heard here is a possibility of a cold fusion reaction which can produce very useful free neutrons, it was that aspect that attracted some of us. So at that point in time, since there was in place a sort of long term fuel cycle strategy, we never considered the "cold fusion reaction" itself as a source of energy.

Sorry for the long digression on the importance of neutrons to the Indian Nuclear program!

Let me come back to what happened on March 24^{th,} 1989. The first thing in the morning, the Director of BARC Dr.P.K.Iyengar called us for a meeting. He too had seen the news item in the Times of India that morning and was very excited that nuclear fusion reaction can be made to happen in a little test tube. As already mentioned there were two aspects : one was occurrence of the fusion reaction itself and the second is the production of neutrons which could also be used.

But I think what is historically important was, that the person who was heading BARC at that point in time, was a kind of visionary with a very open mind. So immediately he called a meeting of about a dozen people selected from various divisions of BARC and said, hey guys, I'm sure you must of seen this news item, I think we should try to quickly replicate it. It sounds very exciting. And he encouraged the groups to think about it and come up with ideas; pick up any piece of Palladium you have in your lab and set up an experiment; I will leave it to you, to strategize how you will do it, but get cracking, get some results quickly!

So there was a challenge thrown to us. Immediately, people went back to their labs to see what can be done. Many of them were chemists, who could quickly set up electrolysis experiments.

It turned out that in our group, we already had an electrolytic cell having tubular Pd-Ag cathodes to produce pure hydrogen gas. This was a commercially procured electrolyzer which we had purchased for our hot fusion studies. We were planning to modify it to generate pure D_2 gas for our ongoing hot fusion experimental program.

Tom Grimshaw: Okay, I'm going to jump in at this point Chino, and end this first session because we agreed we'd have a test session. So this is Tom Grimshaw here with Chino Srinivasan. It's September 10th, we're in Assisi, Italy in a side conversation at ICCF 22 and so Chino, if you agree we'll start a second session here in just a few minutes. Thank you.

Srinivasan Interview, September 10, 2019
Part 2

Tom Grimshaw: Tom Grimshaw here again, this is our second session. I'm here with Chino Srinivasan and it's September 10th , 10:15 AM. We're in Assisi, Italy for ICCF 22 and we're having an interview regarding Chino's cold fusion research. Going back to the very beginning in our first session, Chino, you said that you were already doing investigations kind of very closely related to what was announced on March 23rd, 1989 by Martin Fleischmann and Stanley Pons. So why don't you pick up the story then, maybe go back to the Head of Bhabha Atomic Research Centre, BARC. You said that he was very open minded, and when the news came out on the 24th of March, in the Times of India Newspaper, he gathered a dozen of you together and asked you to go off and set up experiments to follow up on the announcement. Why don't you start by saying what you know about how many groups were there at BARC that pursued this and then continue the narrative about your own group and what it was doing.

Chino Srinivasan: Thank you, Tom. Yes, the Bhabha Atomic Research Centre got into the business of cold fusion almost on day one. Although the date was 24th of March in India, it was probably still midnight of 23rd in the U.S at that point in time. As I said earlier, the Director of BARC Dr.P.K. Iyengar called selected members from the Chemistry Division, Analytical Chemistry Division, the Heavy Water Division and of course we were from the Neutron Physics Division and so on. The researchers invited were all from diverse backgrounds and the Director knew personally which people were doing what kind of work, and so he picked up people who he thought could quickly respond to the challenge.

In our group in the Neutron Physics Division, we were in a very fortuitous situation. By a remarkable coincidence, sitting right there on the table in our lab was the worlds biggest “cold fusion electrolytic cell” in which 16 nos of palladium (actually Pd-Ag alloy) in the form of thin walled tubes located along a circle served as cathodes and a pair of nickel cylinders located inside and outside the ring of cathode tubes, serving as Anodes. Sodium hydroxide solution in ordinary water was the electrolyte.

This commercially procured electrolytic cell (from Milton Roy company of Ireland) was sold in the market as a generator of very pure hydrogen gas for various industrial and research applications. When the electrolyzer is switched on, hydrogen gas released on the surface of the cathode tubes, diffuses through the Pd tube walls, separated from oxygen, and pure hydrogen gas accumulated inside the tubes comes out from the bottom gas plenum to which all the tubes are connected and is drawn off for use. The individual cathode tubes are sealed at the top.

We had procured this hydrogen generator to convert it for generating pure deuterium gas for our on going hot fusion research program. For this all we had to do was to replace the NaOH electrolytic solution by NaOD solution in Heavy water. When the Utah announcement came we were already in the process of converting this electrolyzer to produce deuterium gas. The chemists of the Heavy Water Division were carrying out this conversion. So when the news clip appeared on 24th March, it immediately dawned on us, that we already had a working Cold Fusion cell! Besides it was also the world's biggest cold fusion cell because it had a total Pd cathode surface area of 300 cm^2, which even Fleischmann and Pons did not have! To get cracking on cold fusion research all we had to do was to move in neutron detectors on either side the electrolyzer and switch on the cell current! Thus we were in business with a head start! It took us maybe a week or so to get everything working and move in the neutron detectors and check them out. Being in the Neutron Physics division we had two different types of neutron detectors readily available. Both a bank of BF_3 detectors embedded in paraffin moderator assembly and a proton recoil fast neutron detector (NE 102A) were installed to look for neutron emission. We also set up large NaI gamma detector. The Milton Roy cell with 5M NaOD electrolyte loaded was switched on 21st April 1989 and the cell current was slowly and cautiously ramped up from a few Amps reaching almost 100 amps at the end of 4 hours. Within the first hour we started observing episodes of neutron emission lasting tens of minutes each simultaneously in both the neutron detection channels. The neutron counting rate, averaged over a 5 minute interval, was a couple of orders of magnitude larger than that of background count rates.

A dozen or so large magnitude events were recorded before the cell automatically tripped by the "high temperature" safety circuit. At the instant when the cell current was automatically cut off both the neutron detectors also recorded a gigantic burst of neutron emission, suggesting that a sort of "nuclear excursion" event might have taken place !

After the cell had cooled down we opened it up to take out samples of the electrolytic solution for measurement of tritium content if any had been produced. The samples were sent out to the Isotope Division of BARC who had the expertise and equipment in the form of liquid scintillation counters to measure tritium levels.

When the cell was opened we observed that one of the Pd tubes had developed a big crack and was damaged. When the tritium level assay results came back the next day we were very pleasantly surprised to learn that the tritium level in the electrolytic solution had increased by a factor of over 20,000 times the initial stock solution value of 2.6 Bq/ml. This amounted to an integrated total production of 10^{16} atoms of tritium during that pre-shut down runaway incident. Likewise knowing the geometrical efficiency of neutron detection we could also estimate that the total integrated number of neutrons released in that event was more than 10^{7} neutrons. So we came to the surprising conclusion that the tritium to neutron ratio, which was supposed to be unity for DD fusion, was way over a million.

When all the other groups in BARC, heard about the generation of massive amounts of tritium in our Cold Fusion cell, they too made it a point to look out for tritium production in their cells. By July 1989 the cells set up by the Analytical Chemistry Division had also generated neutrons and tritium indicating similar Tritium to Neutron yield ratio anomaly. These results were presented by us at the Fifth International Conference on Emerging Nuclear Energy Systems at Karlsruhe, in Germany in July 1989. BARC was the first to publish this Anomalous feature of cold fusion devices within four months of the Utah announcement in an International Conference. But soon this anomaly was confirmed by groups from Los Alamos, Texas A & M University and Taiwan.

None of our groups at that point in time, had attempted to measure the excess heat because we did not have the expertise in calorimetry those days. We wanted to concentrate our efforts to firstly confirm that the whole phenomenon was nuclear in origin. That was not yet established clearly at the time of the announcement.

On March 23rd, 1990, coinciding with the first Anniversary of the Utah announcement, an International Conference on Cold Fusion was convened at Salt Lake city. By that time, the overall effort at BARC had considerably expanded with about a dozen groups from different Divisions of BARC involved; The total number of researchers inclusive of technical assistants engaged in cold fusion research had reached 50, making it the worlds biggest Cold Fusion effort ! BARC submitted three papers to this conference summarizing the results of all the 12 groups.

One of the papers which I presented was on the statistical properties of the neutron emission from LENR devices which even to this day nobody else in the world has attempted to replicate. I remember Martin Fleischmann personally complementing me on this work stating "you guys have done what I have always wanted to do"! Interestingly, in the afternoon session on the day when the BARC paper had been presented, Martin was session Chairman and since one of the scheduled speakers had not turned up he declared that he is going "to put Srinivasan on the dock to answer questions on the BARC work as there was not adequate time in the morning session to take up all the questions! I would therefore like to take a few minutes to elaborate on this statistical Analysis experiment and its significance! In those early days having obtained solid proof that neutrons are indeed being emitted by these cold fusion electrolytic cells, I asked the question whether these neutrons are being emitted one at time following Poisson statistical distribution or are they emitted in bunches of 3, 10 or a 100 for example! The answer to this question can give much insight into the mechanisms responsible for causing the neutron producing nuclear reactions! I was uniquely qualified and conversant with the related physics background to think along such lines because of my prior research background in the field of experimental study of fission chain reactions. My Masters Thesis was on the topic of "Neutron Fluctuation Studies in the Zero Energy Reactor Zerlina". There was a sub specialized topic in the branch of study referred to as "Nuclear Reactor Physics" called "Reactor Noise Analysis". I was quite familiar with the fact that because of the chain related nature of neutrons generated inside a nuclear reactor, study of the statistical properties of the neutrons detected by a neutron counter placed right in the middle of an experimental nuclear reactor yields very interesting properties of the fission chain process. A simpler analogy is perhaps the difference between Pu-Be neutron source and a Cf^{252} spontaneous fission neutron source. A Pu-Be neutron source emits one neutron at a time whereas spontaneous fission neutron source emits neutrons in bunches of 2 or more upto almost 10 at a time!

So we set up experiments in which could measure the statistical nature of the neutron emission and that was one of the papers I presented at the First International conf. on Cold Fusion held at Salt Lake City in March 1990. And I do fondly recall Martin Fleischmann congratulating me saying, Srini, that's a nice piece of work!. You have done what I've always wanted to do.

Tom Grimshaw: Oh.

Chino Srinivasan: But BARC groups did not perform any calorimetry in those early days. Within a year, we realized and also the world wide CMNS community realized, that the production of neutrons and tritium is not the main reaction in Cold Fusion, but it was d-d fusing to helium that is the source of heat.

As for the origin of neutrons and tritium It is known that there are two branches of the common d-d fusion reactions – one of these releases neutrons plus He-3 while the other branch releases tritium and a proton. But the problem is these two branches have equal probability and so there should have been as many neutrons as tritium atoms. But our measurements showed that the tritium yield is a million times more. The importance and implications of this observation can be discussed later.

Tom Grimshaw: Right. So this might be a good time to take a break. I think the coffee break is about to happen, but let me ask a couple of follow up questions if I may and then we'll end this session and perhaps have a third session after coffee.

So you heard about the March 23rd announcement, you were called in and had a meeting, several groups did various things. You were in the neutron physics division. You were lucky in that you already had on hand a commercially procured ready made electrolytic cell having Pd cathodes with a very large surface area. So you were perfectly set up. The cathodes were in the form of thin walled hollow tubes. Right. Okay. Good. Cylindrical tubes. Also BARC had heavy water readily available to prepare the NaOD electrolytic solution. Besides, being in the Neutron Physics Division you had the diagnostics readily available. So you were perfectly set up to do a follow up. Yeah, that's amazing. And then , it took a week to get going and in about three weeks time after, on April 21st you had this large burst of neutrons and you indicated that the cells stopped operating at that time. Was there a cause effect, do you think the neutron burst that caused the cells to stop operating or was that coincidental?

Chino Srinivasan: This neutron burst event obviously caused local heating to cause one Pd tube to melt and crack. Additionally there might have been other nuclear reactions wherein heat and Helium might have been generated. Whether the heat burst was due to the neutron producing DD reaction only or whether it was accompanied additionally by a helium producing reaction we don't know. As just mentioned only one out of the 16 cathode cubes had gotten damaged and so we had to remove it and seal that outlet, so that we could continue operating the electrolyzer with the remaining 15 cathode tubes.

Tom Grimshaw: Okay. So the big neutron burst came from one of the cathode tubes?

Chino Srinivasan: Yes, probably only one of the Pd tubes out of the 16 cathodes.

Tom Grimshaw: Right.

Chino Srinivasan: Only one of them had got badly damaged. Only one out of the 16. Again showing the non uniform nature of the cold fusion phenomenon. We now know in hind sight, about the concept of “Nuclear Active Environment (NAE)” phenomenon propounded by Ed Storms!

Tom Grimshaw: Right. This is so typical of cold fusion. Of course you go along and nothing happens for a very long time and then you'll be at sudden burst and we've seen that time and time, by investigator after investigator in this field, it's almost always the some conditions are just right. It takes off and then it stops or it just self-destructs, destroys itself and, and then you're left wondering what were the exact conditions that caused the event. So okay. And then the conference that was held in July 1989 where the early BARC findings were presented. Can you tell me which conference that was in July of 1989 ?

Chino Srinivasan: It was the Fifth International Conference on Emerging Nuclear Energy Systems (ICENES-V). Interestingly, I had been a part of that conference series having attended every one of the previous conferences of the series since the early 80s. We knew the people who were in the organizing committee of ICENES series very well. So we got an opportunity to tell the world about reality of the Cold Fusion phenomenon.

Tom Grimshaw: Good. And not to get stuck on details too much. Where was the conference held? In India?

Chino Srinivasan: No, no, no. It was in Karlsruhe, Germany. It was an exciting time for us at BARC. The next major conference came up in nine months, namely in March of 1990 at Salt Lake City. The BARC teams had submitted three papers for this meeting. The main paper was an overview of the BARC work. That paper was eventually published in Fusion Technology with 50 authors. The conference organizers in due course had their own conference proceedings, but they had restricted the page length. The Fusion Technology paper with 50 co-authors was more comprehensive.

Tom Grimshaw: I remember that from my previous work. 50 authors? Yes. Yes. And it was George Miley, the editor.

Chino Srinivasan: Exactly. George Miley also attended ICCF-1 conference and was kind enough to invite us to submit a paper on the BARC work to “Fusion Technology” of which he was the Editor. That paper was quickly peer reviewed and accepted for publication. It appeared in the Aug 1990 issue of Fusion technology!

Tom Grimshaw: Right, exactly right. Okay. Well this is a, a good time for you to take another break for half an hour or so and we can have a third session after we've had coffee and a restroom break. Yes. Very good. This is Tom Grimshaw with Chino Srinivasan. It's September 10 and we're doing this interview on his Cold Fusion journey starting in the very earliest days. Within a day or so after the March 23rd, 1989 announcement. So thank you Chino. And perhaps we can have a third session shortly here.

Chino Srinivasan: Thank you, Tom.

Tom Grimshaw: Okay.

Srinivasan Interview, September 10, 2019
Part 3

Tom Grimshaw: Tom Grimshaw here again, this is session number three with Chino Srinivasan. We're doing this set of three interviews regarding Chino's involvement in cold fusion. Going back to the very earliest time after the March 23rd 1989 announcement. And Chino, you in our previous two interviews, you indicated that you started within a day or two and you had probably the foremost, if not certainly one of the foremost cold fusion researchers in those early days. Very exciting days. And you mentioned the July conference and then came the ICCF-1 conference at Salt Lake City.

I remember from our previous conversation, Chino, that Professor Iyengar had kind of nominated you, or designated you as the coordinator for cold fusion research efforts at the Bhabha Atomic Research Center (BARC). Tell us a little bit about that role, if you would, back in those earliest days and what other things were going on at BARC in addition to your own division, please.

Chino Srinivasan: Yes, so as I said, I sort of became the natural coordinator of cold fusion for three reasons. One is, we were the only group in BARC at that point in time looking into fusion and we were already conducting what's called plasma Z-

Pinch experiments, using capacitor banks and so on. And I also happened to be, at that point, the head of the Neutron Physics Division so we had all the capabilities to measure neutrons.

And besides that, I had a very good personal rapport with Dr. Iyengar and so, naturally, I ended up coordinating the cold fusion effort in BARC although it was understood that he was the leader. All the other groups submitted their results to me and I stitched it all together into one comprehensive overview paper titled "BARC Studies in Cold Fusion".

As cold fusion progressed into the second and third years, it became obvious that neutron emission and tritium production were only a side show in the cold fusion mystery! Some renowned CMNS researchers still don't believe that the neutrons we measured are released in a d-d cold fusion reaction, but rather It may be a secondary reaction caused by an energetic triton released in a d-d- strip - fusion reaction which is also referred to as the Opponheimer-Phillips process. If the neutron came directly from a normal d-d fusion reaction it should have 2.5 Mev energy whereas if it came from a secondary d-t reaction it should have 14 Mev. But nobody has confirmed the exact energy of the neutron released to settle the issue. So I don't want to get into that argument for now; it doesn't really matter. But at least we had contributed to unraveling one mystery, namely the neutron to tritium ratio anomaly as being a very important signature of the cold fusion process.

Anyway coming back to what happened at BARC, a few months after the ICCF-1 conference held in Salt Lake city in March 1990, Dr. Iyengar got promoted as the Chairman of the Indian Atomic Energy Commission, moving up the ladder. The next person who took over as Director of BARC, Dr. Chidambaram, happened to be at the point in time, my immediate superior in the Physics group. By that time it was however well known in BARC circles that Chidambaram did not believe in Cold Fusion. He was inclined to support the skeptical point of view propagated by the American Physical Society. Also by that time the negative conclusions of the ERAB committee appointed by the US Dept. of Energy to assess the reality of Cold Fusion had been released.

Tom Grimshaw: Yes. Mainstream rejection of Cold Fusion was pretty much what happened worldwide at that time.

Chino Srinivasan : Interestingly, on the day that Dr. Chidambaram took over as Director BARC within hours he walked into my office and fired me left and right! He made it abundantly clear that BARC will not give institutional support for Cold fusion research from then on. He told me "you are senior enough, I cannot dictate what you should or should not do. If your younger colleagues wish to work with you and help you that is their choice; you can conduct experiments and publish papers etc. but you will not receive any financial support from BARC.

Tom Grimshaw: So I can see that It was a seismic shift at BARC; in other words, from open-mindedness and strong support under Dr. Iyengar to close mindedness under Dr. Chidambaram!

Chino Srinivasan: It appears that Chidambaram was contacted by his friends in the U.S. who told him, "*you are taking over the directorship of a very important Scientific Centre and it is our duty to tell you that the American physical Society has "rubbished" cold fusion. Most of the top Nuclear Physicists in the world have concluded that Cold Fusion is not real. So I would advise you not to spoil the good name of BARC by continuing to support cold fusion"*. In any case he had independently already made up his mind - the foreign advice was additional!

Tom Grimshaw: Extra impetus !

Chino Srinivasan: Yes. And so BARC did not officially support cold fusion from mid 1990 onwards. I was due to retire in 1997 on attaining age 60. So for the remaining six or seven years of my career at BARC life was difficult for me ! Not as comfortable as it was under Iyengar!

But that didn't stop me from continuing to carry on experiments in our Division. My entire group continued with me. But in many other Divisions of BARC Cold Fusion research slowly dried up as their bosses did not have the courage to stand up. I must however confess that those of us who defied authority had to pay a price. We all lost out in terms of being denied promotions and financial support to attend foreign International conferences etc.

Tom Grimshaw : Oh yes. Often very subtle and in an unstated way. Yes.

Chino Srinivasan : Exactly! As an Associate Director of the Physics Group I was automatically a member of the top most governing council of BARC, along with all Group Directors. Every time any discussion pertaining to Cold Fusion came up, I would politely beg to differ with Chidambaram. The rest of the council members would watch this stand off between myself and Chidambaram with amusement!

Tom Grimshaw : Yeah.

Chino Srinivasan : I eventually retired at the end of February of 1997 soon after crossing age 60 which was the prescribed retirement age for government servants. However after retirement also, I continued attending conferences on Cold Fusion at my own cost and benefitted by keeping in touch with my overseas cold fusion buddies. Obviously I had no opportunity to conduct any experiments as no private University or Institute in India had any funding for Cold Fusion related research. I merely followed the progress of the field for the next 10 years. I was sometimes invited to present a review paper on my previous work at these ICCF conferences.

Tom Grimshaw: Okay. So we'll call that the gap or the window, but I have a few questions about before you retired.

Chino Srinivasan: Okay.

Tom Grimshaw: Okay. Let's talk about the methods that you used. You started within a day or two by using electrolytic cells following the approach of Martin Fleischmann and Stanley Ponds, okay. Tell us a little about the trajectory of the other cold fusion work done by you.

Chino Srinivasan : Obviously, I, being a physicist, was not personally very conversant with electrolysis and things like that which required a chemistry background. Our group therefore collaborated with some friends in the Heavy water Division who were trained chemists. Our Division's contribution was primarily in neutron yield measurements. We have however already covered this part of my research career in our earlier discussions.

Our group pioneered the use of a very simple but clever technique to detect the presence of and also image the spatial distribution of tritium produced on the surface layers of metallic samples of Titanium exposed to deuterium gas. After loading deuterium gas, the metallic samples were placed overnight in a dark room on top of a medical X-ray film. Next morning the film was taken out and developed using standard chemical procedures. If there was any tritium on the metallic sample the low energy beta particles and also the very soft (4.5 Kev) X-rays induced in the Titanium would give rise to a very impressive image indicating the intensity and spatial distribution of the tritium on the metallic surface. This technique is what is often referred to as Autoradiography!

One of the things that got me interested in those early days was study of deuterium gas loaded Titanium targets. It is well known that Ti metal has an affinity for hydrogenous isotopes (p, d & t). Titanium hydride and Titanium deuteride are known to be extremely stable. Deuterium and tritium loaded thin film Ti (on Cu backing) targets have been used by the Nuclear Physics community years before the Cold Fusion era as a source of neutrons by bombarding such "deuterated targets" by deuterons accelerated to a few hundred kilo Volts in University laboratory based Cockroft Walton accelerators. The hydrogenous isotopes implanted on Ti surfaces stay put there for decades without getting oxidized.

I remember one of the early experiments we did was to pull out a conical (pointed) Ti electrode and a coin sized Ti disk electrode which had been exposed to deuterium gas during simple discharge experiments conducted in deuterium atmosphere, performed many years before the Fleischmann Pons announcement. We were studying the systematics of neutron emission in low voltage gas discharge devices those days. A couple of weeks after the announcement we pulled out such used Ti electrodes from a cupboard and exposed them overnight to a medical X-ray film in a dark room just out of curiousity, to check if there was any evidence of tritium. Next day after developing the film we were very pleasantly surprised to see a characteristic image close to the sharp tip of the cone. And the disk sample showed a number of spots indicating presence of tritium in highly localized spots. We soon confirmed that the images were indeed caused by the presence of tritium. But we had no clue as to when or where the tritium came from. Had it been generated during the gas discharge phase of the experiments or subsequently during storage as a result of spontaneous cold fusion type nuclear reactions.

As a follow up to these observations we went round BARC and collected from various Divisions, a dozen or so thin film TiD targets (on Cu backing) imported from Amersham Labs of UK for possible use for D-D neutron generation by bombardment with 10s to 100s of kev deuterons. The suppliers of these targets had provided certificates indicating the purity of the deuterium implanted in them, especially the upper limit of tritium contamination if any. But autoradiography of these TiD targets by us indicated that all of them (12 out of 12 targets) had a large amount of Tritium on them. All of them gave intense circular images! It does appear that during storage, over the years and decades, tritium had been generated following spontaneous cold fusion type reactions! Interestingly when we confronted Amersham Labs with these results suggesting perhaps there might have been cross contamination of tritium during their manufacturing process, they vehemently denied any such possibility!

These and other results obtained by us with Deuterated Ti targets are summarized in a paper titled "Observation of Tritium in Gas/Plasma Loaded Titanium samples" presented at an American Inst. of Physics Conference on the Topic "Anomalous Nuclear Effects in Deuterium/Solid Systems", held at Brigham Young University, Provo, Utah in Sept 1990.

In this Provo conference there was a paper from the Frascati Nuclear Centre in Italy authored by Scaramuzzi et al. They had reported observing emission of neutron bursts when an SS tube containing D_2 gas loaded Ti chips was suddenly chilled using liquid nitrogen. The speculation was that when such loaded metallic chips are subjected to a thermal shock, the deuterons presumably rapidly diffuse within the metal on account of the thermal gradients established and in this process collide with each other and cause d-d fusion reactions emitting neutrons.

Tom Grimshaw: Yeah. I do know that there was that kind of work also done at Los Alamos.

Chino Srinivasan: Yes, exactly. Tom Claytor... yes.

Tom Grimshaw: Tome Claytor and Malcolm Fowler in particular, were using titanium chips. But my question is this, why the focus on titanium as opposed to palladium ? What made you decide to use titanium in the gas loading?

Chino Srinivasan: As elaborated on earlier we already had got interesting results with deuterated Titanium samples. Also Scaramuzzi from Frascati had just reported observing neutrons after subjecting the samples to thermal shock. That experiment sounded simple enough. Although we had the capability to detect neutron emission we were curious to find out if tritium is also produced in such chips studies. Remember that neutron generation is immediate and often times measurement of a sharp burst of neutrons is tricky. Skeptics will say that the signal you get could be spurious electrical noise! But once generated, tritium stays put. You can measure it again and again to confirm! So unlike the Los Alamos work or that of Scaramuzzi we focused on tritium measurements!

Let me describe the procedure we adopted. The Ti shavings were produced from a Ti rod using a lathe. Shavings were acid cleaned and loaded with deuterium gas by heating them under vacuum in an induction furnace to over 650 C and allowing them to cool in presence of deuterium gas.

Several such cycles of heating under vacuum and loading while cooling were adopted to get a good loading. We then dropped such loaded chips into a vessel containing liquid nitrogen to create thermal shock. We did look for neutron emission spikes, but it was inconclusive. Later the chips were retrieved and subjected to autoradiography as described earlier in lots of 20 chips at a time. We were very surprised to find a few chips showing very impressive spotty images. Further analysis of these chips using a beta particle counter and an X-ray detector confirmed the presence of a very high level of tritium activity but only in a few chips, and that too in highly selected localized spots. Once again indicating the unique feature of the LENR phenomenon that it occurs only highly localized region. This phenomenon is now recognized to be due the Nuclear Active Environment concept propounded by Ed Storms !

Tom Grimshaw: You mentioned Ed storms and so perhaps a little clarification here, I think what you created was the nuclear active environment, NAE by creating cracks as per his concept.

Chino Srinivasan: Yes. You are right. However at the time we conducted these experiments in 1990 Ed Storms had not yet come up with his crack model of NAE.

Tom Grimshaw: Right.

Chino Srinivasan:

Plasma Focus Experiments

One of the “hot fusion” related experiments that the Neutron Physics Division had been engaged in prior to the advent of Cold Fusion in 1989, was the study of the so called Z-pinch plasma focus device which had been independently developed both at the Los Alamos National Lab in the US and the Kurchatov Institute in Moscow during the Cold War days of the 60s. In a plasma focus device, a high current rapid discharge is initiated by triggering a high voltage Capacitor bank between an outer cylindrical electrode and a central rod Anode mounted inside a metallic chamber filled with low pressure deuterium gas. This results in the formation of a transient dense “Z-pinch” in the deuterium gas in the form of an “elongated focal region” just above the top surface of the central electrode. The pinch itself lasts hardly a microsecond but during this short time (d,d) fusion reactions take place. The Physics of the “Dense Plasma focus” device and the phenomena that occur in the short “burst time” is rich in Physics and used to form the subject matter of specialist Z-Pinch conferences held almost annually in the western world. Physicists had fun carrying out diagnostic experiments to understand the formation of the pinch and the mechanism of neutron production. In 1989 the Neutron Physics division already had been experimenting with a small 20Kj Plasma Focus device.

But we had just got sanctioned a million dollar funding to build a powerful 100 Kj Capacitor bank Facility to be erected in a specially constructed new building in order to give BARC an opportunity to get involved “Fusion

Physics". The "Plasma Focus" approach provided a developing country such as India an opportunity to enter the "Thermo-nuclear Fusion club" dominated by the super powers with their gigantic Tokamaks, Stellarators and laser or inertial fusion machines!

So what is the relevance of the Dense Plasma Focus device to cold fusion? It turns out that the central anode of the small Plasma Focus device we were playing around with was made of titanium and during the capacitor bank discharges a fusion grade deuterium plasma "focus" was generated just a few millimetres above the top of the Ti Anode rod, injecting and implanting accelerated deuterons into the top surface of the rod. The discharge also cleaned the top surface layers of the rod prior to loading deuterons into the Ti lattice, while simultaneously generating intense electrical and magnetic fields. We now know that all this is very conducive to the occurrence cold fusion type nuclear reactions in the near surface regions of the rod, besides the conventional d,d fusion reactions which Plasma Focus researchers had been studying for decades!

We ofcourse also measured the intensity of the neutron bursts during each discharge. That part was routine and neutron yields were as expected. But we also used the autoradiographic technique to look for presence of tritium on the top surface of the Ti Anode rod after about 50 discharge shots and to our pleasant surprise we obtained a brilliant and very characteristic radiographic image (which later went viral in cold fusion circles!) What was heartening is that the total number of tritium atoms lodged in the rod surface was more than 10^{16} atoms, which was almost 6 orders of magnitude more than the total neutron yield in all the 50 discharges put together, clearly obeying the neutron to tritium ratio anomaly characteristic of LENR physics! The autoradiographic image also confirmed that the tritium is lodged in grain boundaries, confirming the localized nature of the cold fusion phenomenon. These results were presented initially at the ICCF-1 held in March 1990 at Salt Lake city, and later in greater detail at the Provo meeting in Sept 1990.

Tom Grimshaw: And the way you did that is, as I understand, you were using titanium rods exposed to hot deuterium plasma, I guess under pressure.

Chino Srinivasan: No, not really. The initial gas filling in the chamber was not at high pressure.

Tom Grimshaw: Not pressure.

Chino Srinivasan: It was actually, at much less than one atmosphere pressure. We evacuate the chamber and fill up deuterium gas to a pressure a few mbar or so. That is the standard procedure in the plasma focus experiment. But the transient Z-pinch itself must have been at high temperature and pressure.

Tom Grimshaw: Oftentimes the explanations are very straight forward.

Involvement with Ni-H20 Light Water Cells Following Randy Mills Paper

Chino Srinivasan: In 1991 there was a paper published in Fusion Technology by Randell Mills wherein he had reported that he was observing a lot of excess heat in a Nickel cathode light water electrolysis system but he affirmed that the excess heat had nothing to do with cold fusion but rather was due to a chemical reaction wherein a compact hydrogen atom (he called it a Hydrino) was formed. Mills' paper was followed by a couple of others. These results really excited our curiosity since in those days light water electrolysis was used as a sort of control by Fleischmann & Pons and others also. It was generally believed that fusion reactions are not possible in a light hydrogen configuration. Hence obtaining excess heat in a light water system would be considered a great breakthrough as compared to heavy water electrolysis.

So we quickly set up a series of open to atmosphere light water electrolytic cells similar to the cells reported in literature, in collaboration with the Chemical Engineering Division and also the Process Instrumentation and Systems Division. The Neutron Physics Division also set up a few cells. Some groups however had arrays of five cells each running simultaneously. On the whole it was a massive effort. The objective was to quickly confirm whether light water cells do indeed generate excess heat! None of us involved in this campaign bothered to look for neutrons, but samples of the electrolyte were periodically sent to the Isotope Division for Tritium analysis who indeed confirmed presence of tritium in many Cells.

The results of the multi divison work were presented by me at ICCF-3 meeting. held in Nagoya, Japan in early 1993. The title of the comprehensive paper was : "*Tritium and Excess Heat Generation during Electrolysis of Aqueous Solutions of Alkali Salts with Nickel Cathode".* This paper was published in the proceedings of the Nagoya Conference. Looking back and re-reading that paper I must say it was an exhaustive piece of research. To the best of my knowledge we were the first group in the world to report observing tritium production in light water electrolytic cells, confirming the occurrence of some sort of nuclear reaction in a light water system.

I do recall I had to obtain the permission of the BARC governing council to travel to Japan although all my travel expenses were borne by the local conference organizing committee. I had to submit the Abstract of the paper, its acceptance letter and confirmation of financial support. When the agenda item came up for granting permission to attend the conference, Dr. Chidambaram noting our claims of detecting Tritium in light water cells remarked "You are a loner I say!" implying that since no body else in the world had reported tritium in a light water cell, our results must be spurious !

However there was an interesting postscript to our claims of observing both excess heat and tritium in light water cells, since most cold fusion "stalwarts" who were staunch proponents of the Pd-D system were skeptic of our results, especially the excess heat part. At the Nagoya meeting many including my good friend McKubre suspected the excess heat must be due to recombination of the hydrogen and oxygen released on the Ni surface.

Later in 1993 I was invited by Michael McKubre to spend 6 months at SRI International in Menlo Park as a visiting Scientist. Mike made available to me a system comprising a pair of digital balances. An identical open electrolytic cell of the type we had used at BARC was placed on one of the balances. The gases generated during electrolysis were sent into a flask in which a recombination Pt catalyst was placed. The loss of weight of the working cell as well as mass of the water collected in the recombination flask was also recorded simultaneously.

Tom Grimshaw: Chino, I'm going to interrupt you here. Let's pick up the SRI connection in just a moment. I want to go back a little bit. You mentioned Randy Mills... Randall Mills, okay. That was Black Light Power. Not Brilliant Light Power. And so tell us about that connection. How did you get to know him and how did you become interested in the nickel hydrogen systems?

Chino Srinivasan: As I said it was Randy Mills' Fusion Technology paper published in 1991 that got me interested in light water systems. If excess heat can be generated in a simple light water electrolysis cell it would indeed be great !

Tom Grimshaw: Okay.

Chino Srinivasan: At that point in time, I was very excited because If excess heat can be produced in a light water system, you won't need expensive palladium or heavy water, you'll need only nickel and ordinary water and that gives us an excellent opportunity to get into the energy business through a cheaper route!

Tom Grimshaw: Okay.

Chino Srinivasan : So when I had the next opportunity to visit the US, which came in summer 1992, I made it a point to go and visit Randy.

Tom Grimshaw: In New Jersey?

Chino Srinivasan: No, it was in Lancaster, PA, a couple of hours drive, south of Washington D.C. His company was called "HydroCatalysis Power Corporation" at that time.

Tom Grimshaw: Oh this was even before New Jersey.

Chino Srinivasan : Yes. Even before New Jersey. He further changed the name to Blacklightpower and afterwards to Brilliantlight power. By that time he had a lot of funding. He was no more accessible. Today you can't even talk to anybody in his company. He doesn't entertain ordinary visitors. If you have a million dollars to invest you can meet him !

Tom Grimshaw: And he, of course, distanced himself perhaps wisely, from cold fusion .

Chino Srinivasan: As I said earlier my interest was triggered by his paper on the concept of the Hydrino. So I had a talk with him and very politely told him that, Randy, I don't think I beleive your Hydrino theory, but I fully trust your excess heat results, and so I'm going to try and replicate the same!

That is how we set it up many cells culminating initially in the ICCF-3 Nagoya Conference paper as mentioned earlier. Eventually we published a separate paper in Fusion Technology in 1996, that took us three years of repeatedly measuring tritium in a number of light water cells.

Coming back to the SRI replication experiments, it turned out that McKubre was indeed right. The excess heat we had reported at Nagoya was indeed due to recombination effects. The quantum of excess heat measured precisely corresponded to the shortfall in water regenerated in the recombination flask.

None of my colleagues, the co-authours of the Nagoya paper many of whom were Chemists, were aware that recombination can take place on the Ni cathode surface in an open cell. All of us were under the impression that only a Pt catalyst can cause recombination of the hydrogen and oxygen gases.

In electrolysis experiments in an open cell it is assumed that the joule heating component is (V-1.53) * I, where V is the voltage applied to the cell and I is the cell current, while in a closed cell the Joule heating Energy is taken as V * I.

Tom Grimshaw: Right. (1.53 * I) represents the energy component that is used up in breaking of the water molecule.

Chino Srinivasan: Yeah, breaking of the water molecule. But in an open cell, some of the hydrogen released re-combines on the Cathode surface internally. So the “Faraday efficiency” is not 100% . But when we do the excess heat calculations, we assume all of the electrolytic gases escape to the atmosphere. It was a silly mistake on our part to overlook the recombination possibility.

Tom Grimshaw: But you were measuring tritium though, which was your primary signature.

Chino Srinivasan: No, no. We also claimed we were measuring excess heat. Measurement of tritium was used to support the excess heat claim!

Anyway the SRI two balance experiment clearly demolished our excess heat claim. But I was disappointed that my SRI cells also did not produce any Tritium.

So on returning to Mumbai in Oct 1994 I proceeded to set up several Ni-H20 cells once again in the Chemical Engineering Division mainly to confirm tritium generation. This time we set up one cell in a funnel shaped flask where in electrolyte samples could be drawn off from the bottom at regular intervals without disturbing the cell operation. These carefully conducted replication experiments conducted during 1995 clearly confirmed Tritium production in several cells although at a much lower level than before. These results were published in Fusion Technology in 1996, just before I retired. In fact the funnel flask cell in which Tritium samples were drawn frequently showed that tritium was not only being produced but was also being “consumed’ at times! We speculated that the Tritium is perhaps absorbing a proton to become He^4!

Tom Grimshaw: Okay. So we'll go to the next 10 years, shortly. So in summary then, you were using electrolytic cells that were actually being operated by a couple of other different divisions as well!

Chino Srinivasan: Yes, yes.

Tom Grimshaw: And then you went to the Titanium work with gas loading using rods and chips.

Chino Srinivasan : Actually that was done before I got dragged into the Ni-H2O electrolysis work.

Tom Grimshaw: And so you did the nickel hydrogen measuring Tritium production and then some electrolytic cells at SRI. Does that pretty well encapsulate or summarize the work that you did at BARC then or were there other ?

Chino Srinivasan : At BARC yes, before my retirement. These are the areas in which we got good results. In many cases we were probably right in the forefront. We were among the first to report that tritium is produced in NiH systems. We were also the first to point out that these tritium producing fusion reactions occur non uniformly and only in selected spots, especially in deuterated Ti configurations using the autoradiographic technique which was pioneered at BARC. We can refer to this spotty feature as “localization in space”! The other notable observation emanating from BARC was the fact that when neutrons are produced it is usually in the form of sporadic bursts; and also that the neutrons seem to be emitted in bunches of several neutrons at a time lasting only a few microseconds. We refer to this feature as “localization in time”. By putting together the two independent observations of “localization in space” and “localization in time”, we postulated that neutron emission and tritium production are interrelated and results from the occurrence of some sort of chain reaction or micro-nuclear explosion! We published a paper on this titled “Neutron Emission in Bursts and Hot Spots: Signature of Micro-Nuclear Explosions?” in JCMNS in 2011. Clearly the sites where such micro explosions occur must be what Ed Storms would call an NAE!

Tom Grimshaw: Yes. A nuclear active environment.

Chino Srinivasan: Some kind of a Nuclear Active state!

Tom Grimshaw: Here's a question I always ask, which instances in your true trajectory of experiments, which instances were you most certain that you observed cold fusion occurring?

Chino Srinivasan: I think I would probably say we were amongst the first to measure huge amounts of tritium production in Cold Fusion electrolytic cells. I don't think anybody has beaten that, not even Bockris! In fact Bockris claimed he was the first to detect tritium in the Cold Fusion field. I sent him all our papers and finally he agreed. Okay. You may have published it, but we observed it before you did ! He was obsessed with the question who saw Tritium first! Anyway we became good friends and every time I came to the U.S. he

	would invite me to Texas A&M. I would give a talk and then have discussions and that's a different story.
Tom Grimshaw:	Yes. So obviously the April 21st event was probably the first and major event and also in the Titanium work.
	Okay. I'm probably going to wait until our next interview to talk about other developments. But before we leave BARC, did you actually conduct additional laboratory experiments since you retired from BARC?
Chino Srinivasan:	No.
Tom Grimshaw:	So mostly what you've been doing is working from home ?
Chino Srinivasan:	Yes.
	On the whole I had great fun at BARC during the 7 or 8 year period of my association with Cold Fusion ! I retired in Feb 1997. As soon as I retired Cold Fusion research was shut down at BARC. And given the anti cold Fusion sentiment among scientists worldwide I could do nothing to try and reignite interest in CF/LENR in India.
	For the next 10 years from 1997 to 2007 I did not publish any papers in this field although I continued to attend all the ICCF series conferences at my personal cost out of interest in the field. I also did not make any noises in India except for writing an article in 1999 in the Hindu Newspaper on the 10th Anniversary of the F & P announcement. I kept a low profile and was silent for the next 10 years until 2007 !
	But I will come to the next phase of my Cold Fusion story. That is I took it upon myself to reignite interest in cold fusion in India.
Tom Grimshaw:	Okay. So we're going to save that for the next session because we're now at 30 minutes and this is a good break point and so we'll stop here.
	This is Tom Grimshaw with Chino Srinivasan talking about his involvement in Cold Fusion. So far we've been talking about your work from the March 23rd, 1989 announcement until your retirement in February of 1997. And the chain of events that propelled you through the various research phases. I would say while you were still at BARC, and that's been the main emphasis up to now. In our next session, we'll talk about what you did after retirement. So I'll say again that it's September 10th, we're in Assisi, Italy at ICCF 22 and Chino, I'll look forward to our next session, which will be session number four. All right.

Post Retirement, Post ICCF 13, Post 2007 Attempts to Revive Cold Fusion Research In India

* 2008 NIAS Brain storming meet
* 2011 ICCF 16 Conference
* LENR Transmutation papers
* 2014 Modi/Piyush Goyal meeting – NIAS Discussions.
* 2016 Narayanaswamy Paper
* 2019-20 Elsevier Book Chapter

B2. Description of LENR Research in India in 2011

An article by Marianne Macy on LENR research in India appeared in early 2011, just prior to the 16th International Conference on Cold Fusion (ICCF-16) [23]. The conference was developed by an organizing committee chaired by Dr. Srinivasan. The article is based on excerpts from an interview of Dr. Srinivasan that was conducted as part of the New Energy Foundation Oral History Project.

Thanks go to Marianne for giving permission to include the article in this report.

[23] See #38 in the tabulation of Dr. Srinivasan's publications.

ICCF16 in India: A Historic Perspective

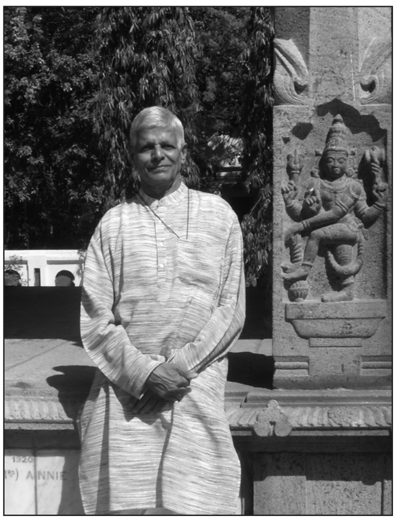
Marianne Macy

One top Indian executive told me, "We have a window of between five and seven years." India does not have the luxury of time. It must immediately address a host of problems in ways that are scalable and sustainable. It must leverage new technologies to minimize environmental damage while maximizing scarce resources to bring badly needed benefits to a huge population. India will have to use its proven talent for technological innovation to create the solution it needs.

India's new cars, power plants, and factories risk adding millions of pounds of carbon to an already overloaded atmosphere unless the country moves to power its growth with clean, alternative energies. India is the only country with a Ministry of Non-Conventional Energy. It has the potential to become a leader in new technologies for clean energy production. The Indian company Suzlon is an example of how India can scale up alternative energy, and take it global. In just a few years, Suzlon has become one of the world's leading wind power companies, selling power to utilities to the state of California and other foreign markets. As oil prices rise, alternative energies are becoming increasingly more attractive. India has the technological acumen and the pressing need to find clean energy solutions.

—Mira Kamdar, author of Planet India: How the Fastest Growing Economy is Transforming America and the World (2007)

In February 2011, the International Conference on Condensed Matter Nuclear Science (ICCF16) will convene in Chennai, India. This is the first time one of the ICCF series conferences is being held in India. But India has played an important role in the development of the field, with groundbreaking experimental results and support roles in work that commenced in other major scientific research establishments. Here is that story, told in parts by the chairman of the ICCF16 Organizing Committee, Mahadeva Srinivasan, in excerpts from his oral history done for the New Energy Foundation Cold Fusion Oral History Project (which will be housed at the University of Utah's Marriott Library), with additional details from his international colleagues. Srinivasan has emphasized that what follows is only his perspective; there are other stories too, especially ones involving chemists like the late Sinha Ray, who contributed significantly to the early BARC story.



Dr. Mahadeva Srinivasan

When Martin Fleischmann and Stanley Pons' March 23, 1989 announcement of the discovery of what was termed "cold fusion" became worldwide news, scientists rushed into laboratories to try to replicate the experiments and make their own discoveries. Some of the early work which led to the negative public perception of the field came from serious institutions such as MIT, Caltech and Harwell. Other experimentation that replicated and supported cold fusion was conducted in other fine laboratories around the world and continued after the court of public opinion had passed its verdict. Across the world from where Fleischmann and Pons were making their announcement in Salt Lake City, Utah, scientists at the Bhabha Atomic Research Centre (BARC) in India began their cold fusion work the very next afternoon, on March 24, 1989.

BARC, which began its history in 1954 as the Atomic Energy Establishment Trombay (AEET), was India's first and primary nuclear research center to develop nuclear technology. It was founded by Homi Bhabha, described by retired BARC scientist Mahadeva Srinivasan as "truly a brilliant scientist. We used to look upon him almost like the Leonardo da Vinci of India." Bhabha initially set up the Tata Institute of Fundamental Research in 1945; most of its nuclear scientists transferred to BARC when it opened. Bhabha, considered the father of India's nuclear program, worked closely with Prime Minister Jawaharlal Nehru to establish the Atomic Energy Commission of India in 1948. A few years later, AEET opened; it was renamed following Bhabha's death in an air crash in 1966. Srinivasan said, "He had vision and courage. . .Had he been alive, I think India's future might have been quite different."

Mahadeva Srinivasan grew up and did his schooling in Chennai. He joined BARC in 1957, at the age of 20, and remained there until his retirement as associate director of the Neutron Physics Division 40 years later. His division dealt with nuclear technology and the scientists were fascinated by the idea that you could have power from the atom. "We used to build little experimental nuclear reactors and play around with them and learn about fission reactors. We then got interested in thermonuclear fusion. It was a time when we were actually having a series of experimental projects on so-called hot fusion."

On March 24, 1989, BARC scientists saw a short newspaper item in the *Times* of India announcing that two scientists from Utah claimed they had been able to conduct nuclear fusion reactions on a tabletop device and that they had detected neutrons. Because of their decade-long involvement in a program exploring fusion, the item caught the attention of BARC researchers as well as the director. "We knew that beyond fission, the next stage in nuclear technology has to be fusion," Srinivasan relates. "We were aware of fusion reactions and what it's all about, and the different approaches to fusion, the so-called laser induced inertial fusion, magnetic confinement and there was another type of fusion in those days which was between the two, partly inertial, partly magnetic, called a plasma focus device." Srinivasan's group had experiments underway. They had already demonstrated the production of neutrons from a plasma focus fusion device. "So it was very interesting for us to know, since we had been actually working and reading all the papers about fusion, that here seems to be an alternate way of producing fusion reactions. We jumped on to that and tried to set up an experiment."

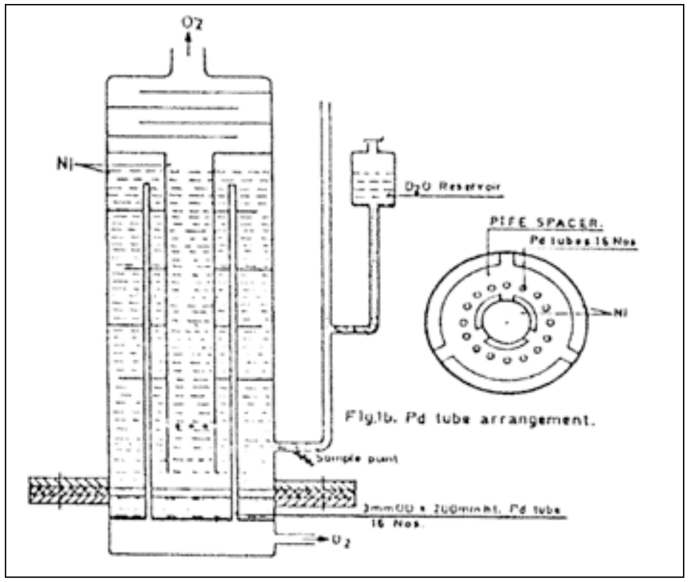
A key figure in BARC getting involved in cold fusion research was its director, Dr. P.K. Iyengar. Srinivasan notes, "He is one of those people with an open mind, sort of an adventurous kind of person who was willing to look at any new idea and explore it. He also saw the same news item and he called me up and we got together with many other people. He convened a little group of young people in whom he had faith. . .He really encouraged us and enthused us, not just one group but several groups."


Dr. P.K. Iyengar

BARC scientists enjoyed a certain amount of freedom in their work, and the director was personally interested in this particular problem, so there was no question as to whether or not they would work on cold fusion. Srinivasan was the head of the Neutron Physics Division, with 30 or 40 people working with him. "We had a number of groups, some working in plasma fusion, some working in fission and some working in theoretical analysis, and so on. We picked up those people that had the right equipment, the right background to set up these experiments. We were, in particular, trying to verify the claim of fusion reactions producing neutrons."

One of BARC's objectives at the time was to investigate the possibility of developing fusion into a neutron source in order to convert thorium into uranium-233 (U-233). The project sought to develop technology for using U-233 in power reactors. U-233, a man-made isotope, is produced from thorium; India has one of the largest sources of thorium in the world. With a goal to switch India's nuclear technology to U-233, a neutron source was needed. BARC entered fusion not for getting energy, but as a neutron source. Srinivasan came to believe that hot fusion reactors would never become feasible for energy production. But as a neutron source, to convert thorium to U-233 it would be useful. When the announcement came out of Utah, the report of neutrons being seen was what sparked BARC's interest.

In Srinivasan's group there was a fortuitous circumstance on the day the announcement appeared in the newspaper. On one of the workbenches was, of all handy things, what was essentially a cold fusion cell. Srinivasan recalls, "We had



Schematic of a Milton Roy Electrolytic Cell

purchased a device made by a company in the UK, called the Milton Roy electrolytic cell. The cell was basically a hydrogen generator. It was using sodium hydroxide as the electrolyte and palladium tubes as the cathode and I think a stainless steel body as the anode. The interesting feature was that the cathodes were in the form of 16 annular tubes. The way the commercial manufacturer had made it was that during electrolysis the hydrogen ions would diffuse through the tube wall into the tubes and come out from the inside of the tubes to produce pure hydrogen separated from oxygen. So here was a device, a commercial hydrogen generator, which was producing on the one hand pure hydrogen, and on the other, oxygen. We were not interested in the oxygen. Now, we had converted this device to produce—instead of hydrogen—deuterium, for our plasma focus experiments. So we were using this palladium cathode sodium deuteroxide electrolytic device, by applying a voltage of 30 or 40 V. You switched it on and it produced copious amounts of deuterium gas, which we were tapping off and using for our plasma device."

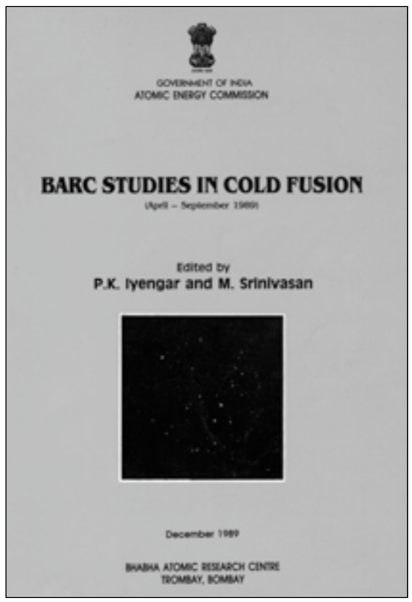
Instead of purchasing deuterium gas, it had occurred to Srinivasan, "Why not produce our own deuterium oxide?" At BARC, the Indian nuclear program was using CANDU reactors, which provide very efficient power and use heavy water as the moderator. India had gigantic plants producing heavy water. "We had our own heavy water. We did not have to import it. You just telephoned the guy in the Heavy Water Division and you could get liters and liters of heavy water. We needed a device to convert heavy water into deuterium. So a simple electrolytic cell sounded sensible. We had bought it, it was on the table, we had been using it. Looking back, it was a cold fusion cell. We were using a cold fusion cell to produce deuterium gas for months! So coming back to the news item, when we heard that a device which uses palladium as the cathode and NaOD as the electrolyte was used by Fleischmann and Pons, we said, 'That's fantastic! It's right here all set and ready to go!' So all we had to do was to move in the neutron detectors. As we were in the Neutron Physics Division, we had all the neutron detectors and related equipment. It didn't take us more than 24 hours to start looking for neutrons."

On the afternoon of March 24, the day after the Utah announcement, BARC scientists in India started their cold fusion work, looking for neutrons. This is probably one of the more amazingly efficient commencements of research.

The first neutron burst occurred on April 5, 1989. Srinivasan was in Washington, D.C. at a meeting organized by the National Academy of Sciences to commemorate the 50th anniversary of the discovery of fission. BARC colleagues were running experiments and immediately sent Srinivasan a message that they had seen neutrons. The second big burst occurred on April 21.

Srinivasan relates, "What is interesting is they sent the samples of the heavy water to the Tritium Department, which had all the equipment to analyze the samples for tritium content and we were amazed that we got fantastic microcurie levels of tritium. So we had detected, within three weeks of the newspaper item, both neutrons and tritium. And what's more, we were very positive at that point in time that the amount of tritium generated was orders of magnitude larger compared to the yield of neutrons; this was done by taking the factor of the efficiency of the detection and the total number of neutron counts recorded and all that. We immediately came to the conclusion that the neutron to tritium ratio was 10^{-7}."

That was from Srinivasan's group. Independently, meanwhile, in the next three weeks, ten other groups had set up electrolysis cells under the inspiration of Dr. Iyengar. Results were presented at ICCF1 in Salt Lake City, Utah (March 1990) and later written up in a paper published in *Fusion Technology* in August 2000. The ten groups' conclusions were that many of them detected neutrons, many of them detected tritium, and the neutron to tritium ratio was independently verified by half a dozen groups. "We first published that in July 1989 at a conference in Karlsruhe. At that point we were among the first groups in the world who found very positive results and we were really excited," Srinivasan noted.



December 1989 BARC Report

Srinivasan's group at BARC also came to experience non-reproducibility when they ordered two more Milton Roy cells, hoping to replicate the production of neutrons and tritium. But, the second and third Milton Roy generators did not produce the same results. This resulted in room for skeptics to come out, including some senior physicists at BARC not working in the area; around the same time the negative U.S. DOE report was published.

A titanium chip experiment done by Howard Menlove at Los Alamos, in which he took deuterium-loaded titanium chips in a cylindrical vessel and dipped the whole cylinder into liquid nitrogen, was thought to have given neutrons; later Menlove suspected that the neutron bursts in some cases were possibly due to water condensation in the high-voltage insulators. At BARC, rather than look for neutrons, the scientists took the deuterated titanium chips and dropped them into a can containing liquid nitrogen, then took out the pieces and monitored them individually for tritium. A thousand small chips weighing a total of five grams were divided into lots of 20 and put into a windowless beta detector. Some gave significant counts. Four out of 1,000 chips had very high tritium activity at the microcurie level.

"These chips are still preserved by us—and they still give this signal," Srinivasan told Russ George in an interview in *Cold Fusion* in 1994. "Douglas Morrison visited us at the time of August 1990 and I showed him [those high activity Ti chips]. The moment we loaded one of those chips into the detector, the count rate indicated a very high level of activity, giving a beautiful beta (electron energy) spectrum. . .I showed him this beta spectrum, and asked him to speculate as to where it could come from. I even gave him copies of the spectrum. He has never talked about it anywhere, or mentioned it in any of his writings."

BARC continued their productivity, publishing *BARC Studies in Cold Fusion* in early December 1989. Srinivasan was responsible for coordinating and compiling the data from the different groups, a progress report on six months of experimentation. The report covered the period ending September 30, 1989, and the first draft was out in early December, just a few weeks after the November 1989 DOE ERAB negative report.

At this time, Dr. Sivaraman Guruswamy, from the University of Utah's National Cold Fusion Institute, was visiting India and came to BARC. He got a copy of the draft version of this report, a 100-page report of preliminary, unpublished results with 50 authors from ten different groups. It was at that stage BARC's internal report.

Srinivasan reports, "Dr. Guruswamy made copies of this and sent it to many other groups. It was around that same time that the Department of Energy's preliminary report came out. So at the end of 1989 in the U.S., two reports were being circulated. One was the DOE report saying that cold fusion was all nonsense and there was nothing to it. Then there was the BARC report giving an exactly opposite conclusion, reporting very interesting results and showing that a number of groups were able to reproduce."

The BARC Director got a call from the Electric Power Research Institute (EPRI) in the U.S. "They had gotten hold of this copy and they were very interested," Srinivasan recalls. "They wanted to come to BARC and verify for themselves if all this was reliable. . . Two scientists from EPRI flew down to BARC during the Christmas to New Year break of 1989. One was George Stanford, and the other Joe Santucci. They met the director and then visited all the labs. When they saw the caliber of the scientists and the quality of the research being done there, they were totally convinced that the BARC results were no joke."

EPRI's Dr. Thomas Passell recalls, "I remember reading the report about BARC's tritium results, machines that gave big pulses—little spots of radioactivity due to tritium emissions, autoradiographic techniques in experiments involving gas-loaded titanium, for example. The BARC results were intriguing certainly, and helpful. . .We kept interested in what they were doing because of the possibilities of tritium showing up. . .It was impressive to someone who was not convinced what was going on was nuclear. We saw it as a good sign that it was going on."

Dr. Michael McKubre's team at Stanford Research Institute (SRI) received funding from EPRI, including for cold fusion research. McKubre reminisces on the cooperation and exchange between BARC and SRI: "Dr. Srinivasan and Dr. Iyengar visited SRI in 1990 and described a number of different experiments that had been performed at BARC to test the hypothesis proposed by Profs. Fleischmann and Pons. They brought with them a bound report that became one of our prized reference texts. Some results were extremely interesting, especially recognizing the caliber of the BARC team. Here was a group of world class experts in relevant fields who had combined in an extended effort, coordinated by Srinivasan and Iyengar, to evaluate the possibility of anomalous nuclear effects issuing from deuterium-loaded crystalline materials. Their results were impressive on a number of levels, both in the scope and intensity of observed effects."

McKubre notes that the BARC team of expert nuclear physicists, engineers and material scientists had "a precise and purposeful approach. . .An interesting historical irony is that the BARC report reflected exactly the type of coordinated, materials science activity that the DOE/ERAB panel members suggested as their preferred mode to evaluate the scientific questions posed by Fleischmann and Pons. That this was not done in the U.S., and was not continued in India, can be traced to the same root cause: politics."

In 1990 Iyengar was appointed chairman of the Atomic Energy Commission of India and retired from BARC. His BARC successor, Rajagopala Chidambaram, was also a nuclear scientist and metallurgist. "Unfortunately, from day one he didn't believe in cold fusion," laments Srinivasan. The new director responded to the advice of the larger international physics community. Srinivasan reflects upon what Chidambaram might have been thinking: "Here is an advanced country, the United States of America, whose wise people have conducted all the inquiries and come to the conclusion that cold fusion cannot work. Textbooks say it cannot work. I think this is all some artifact. I am therefore not going to provide Bhabha Atomic Research Centre's institutional support to cold fusion research." Srinivasan notes, "From then onwards there was no program in BARC under the heading cold fusion."

Slowly the number of groups working on cold fusion in other divisions dissipated once word spread that the new BARC Director would no longer institutionally support the field. Srinivasan did continue with cold fusion research until about 1995. Jed Rothwell, e-librarian for lenr-canr.org and cold fusion advocate, notes, "From 1989 through 1994, some of the best cold fusion research ever published was performed at BARC. . .Unfortunately, after Iyengar left BARC, and Srinivasan and others retired, conservative scientists who opposed cold fusion brought the research to an end."

In that time, Srinivasan's group moved on from electrolysis. BARC's emphasis was to establish the so-called nuclear origin of the phenomenon. They were not interested in excess heat or in the power producing capability. Srinivasan's focus was to see if anomalous nuclear reactions were occurring. They were pursuing the production of neutrons and the production of tritium. They switched from palladium-based electrolysis experiments to titanium-based gas-loading experiments.

Srinivasan's group read the reports of Scaramuzzi and others, using titanium and titanium chips. Srinivasan recalls, "We got some fantastic results using gas-loaded titanium chips. . .Although we didn't detect neutrons in most of them, some of those gas-loaded targets did give neutron bursts. But more importantly, there was tritium. At the end of the whole experiment, we could dissolve it, extract the tritium, measure it, and in many cases there were so many with microcurie levels of tritium. These are all published. We had a number of successes."

Because BARC was a large nuclear facility, criticism followed many of their results. Some thought that contamination could occur, since BARC houses the CIRUS Research Reactor, which produces neutrons, and also due to the levels of tritium in certain areas of the facility. Srinivasan wondered about the criticism, "Why didn't all bottles show tritium? Why was it only one or two bottles out of a hundred?"

"The main problem with cold fusion," Srinivasan says, "was the non-reproducibility. We also could not reproduce many of our results. So what is it that made some devices work sometimes, and not at other times? We are convinced that when it worked, it worked. I have no doubt we produced tritium. I have no doubts about the neutron bursts. . ."

Srinivasan reflects on some of the progress made: "In the beginning, we all thought—and I think partly it is Martin Fleischmann who gave the impression—that it was normal D-D nuclear reactions. But in six months we realized that one particular branch is being preferred, the so-called branching ratio anomaly. So whatever it is, the D-D fusion preferentially is going to the tritium channel. We soon realized that these kinds of reactions seemed to be happening not only in the palladium deuterium system but also in the titanium deuterium system. A little bit later we jumped onto nickel hydrogen devices too. We did carry out a number of light water experiments as well."

After reading a paper by Randell Mills from BlackLight Power, Srinivasan made it a point to meet Mills and look at his devices during one of his trips to the U.S. He postulated, "If the word was coming out that the so-called fusion reactions are occurring not only in heavy water systems but also in light water with nickel, it was much simpler to set up a nickel light water system." So that is what Srinivasan's group did, finding tritium in a nickel light water system. Srinivasan notes, "Slowly over a period of time, with the generality of the phenomenon, it was becoming clear that it was much more complicated than what we were thinking."

Srinivasan notes, "One of my colleagues was doing a Ph.D. thesis on different materials of the electrode and how it affects the neutron producing plasma focus. He tried with nickel, stainless steel, titanium. One of the titanium electrodes gave us such fantastic tritium results. We autoradiographed it. Again we estimated that we had almost a microcurie level of tritium, fantastic amounts of tritium produced there. How do we know it is tritium? Look at the betas. Measure the beta spectrum. Everything fits in very well."

Srinivasan's interest in transmutation has grown over the years; during his time at BARC some experiments were performed. In retrospect, he wishes that his group had focused more effort on transmutation. He stated, "The idea that transmutation reactions probably are occurring and taking place in Nature had not taken root because of cold fusion, but hundreds of years earlier." In 1992 BARC found iron in a transmutation experiment inspired by a visit from Roberto Monti, who carried out a carbon arc experiment originally done by George Oshawa. At Texas A&M, where a BARC post-doc was working with John Bockris, they set up the experiment and also found iron. The BARC/Texas A&M carbon arc transmutation experiments were published in *Fusion Technology*; George Miley's companion editorial noted, "By all accounts, these results are bizarre. But, as an experimentalist since we have no explanation for it, I am publishing it."

Research around the world in the field continued, with hundreds of papers published in journals and results reproduced in all kinds of experiments. In 2008, it appeared that India would re-open their research into what was now referred to LENR, but a series of events—including the terrorist attack on the Taj Hotel in Mumbai—made it difficult for international government scientists to get clearance to go to a key meeting which was to be hosted by BARC in Mumbai in February 2009. This set back progress.

A 2008 *Nature India* article by K.S. Jayaraman reported on Pentagram Research Center, a private company in Hyderabad that had offered to back any Indian initiative on LENR. Srinivasan reports that one large private company will have its representatives present at ICCF16. In the *Nature* article, Iyengar stated, "We did great injustice to the country by stopping the research that was going on at the Bhabha Atomic Research Centre. . .It is not too late to revive it." Iyengar is the chairman of the National Steering Committee for ICCF16.

Srinivasan believes that the prognosis for the future of CMNS/LENR in India is positive. It is his target to try to have at least half a dozen LENR labs operating in various universities/institutions in India by the end of 2011. He already has two or three who have indicated interest and willingness and is confident that more will follow. Researchers, meanwhile, will converge on Chennai for ICCF16, with updates on important new work. Breakthroughs in experimentation and technological applications of LENR should focus worldwide attention on the conference.

"On the whole," concludes Srinivasan, contemplating his role of chairman, "I feel that ICCF16 will mark a turning point in the Indian story." And perhaps also on the role of LENR in history.

[***Editor's Note:*** Dr. Srinivasan provides a more detailed experimental account of the BARC results in his paper, "Neutron Emission in Bursts and Hot Spots: Signature of Micro-Nuclear Explosions?" in this issue.]

B3. Interview in 1994

Dr. Srinivasan was interviewed in 1994, as indicated in the preface to the interview (next page), while he was a visiting scientist at SRI International. The interview[24] was conducted by Russ George. Thanks to Mr. George for giving permission to include the interview transcript in this report.

[24] See #13 in the tabulation of Dr. Srinivasan's publications.

The cold fusion phenomenon is real

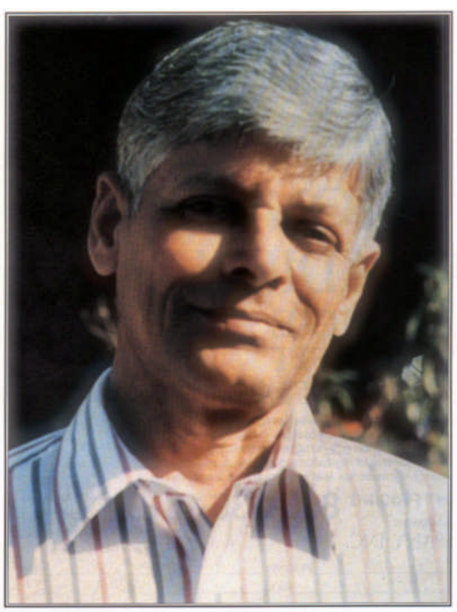
An interview with Dr. Mahadeva Srinivasan conducted by Russ George

Dr. Mahadeva Srinivasan was the head of the Neutron Physics Division and an Associate Director of the Physics Group of BARC (Bhabha Atomic Research Center) in Mumbai (Bombay), India when this interview was held on March 1, 1994 at SRI International in Menlo Park, California. At that time he was a visiting scientist there, participating in the Cold Fusion experiments underway at the laboratories of the Energy Research Center.

Since his retirement from BARC in 1997, at the end of a four-decade long research career, Dr.Srinivasan has been living in Chennai, India. He continues to take keen interest in developments in cold fusion which has now been re-christened as “Condensed Matter Nuclear Science” (CMNS).

His current interests encompass other futuristic energy generation schemes such “Zero Point Energy” or “Free Energy” as it is popularly called, as also a whole gamut of unexplained phenomena often characterized as “Anomalous Phenomena” which seem to be challenging many existing paradigms of “mainstream science”. The scientific investigation of paranormal phenomena is also an area of interest to Dr. Srinivasan.

He wishes to place on record his gratitude to the late Dr. Eugene Mallove for the many interesting discussions he has had with Gene on a variety of anomalies. In fact Gene was instrumental in kindling Dr. Srinivasan’s interest in many new topics that fall in the domain of “Anomalistics”.



Dr. Mahadeva Srinivasan of BARC

RG: Let me start by asking you a little about your background.

MS: I joined BARC in August 1957. BARC has a training school which recruits about 150-200 scientists every year. They call for applications from all over India. Usually 2000, 3000, or sometimes 5000 people apply, all first-class graduates. I belonged to the first batch, recruited way back in 1957. Following one year of training I was appointed as a scientific officer in 1958.

My specialization has been in the area of nuclear engineering and reactor physics, criticality experiments, nuclear instrumentation, and so on. So I am quite familiar with, and quite at home with, neutron measurements. I spent about two years at the Argonne National Lab (USA) in 1961-1962, and later a couple of years at the Chalk River Nuclear Laboratories in Canada (1968-1970). Since then, I've been in charge of fission criticality experiments and fusion-related research at BARC.

RG: How did BARC get involved in fusion science?

MS: Somewhere down the line, I think it was in the mid-'70s, we realized that we had to get into the fusion business in India. So we at BARC were looking for the right niche to enter. In the area of fusion we have two approaches, as you know: magnetic confinement fusion and inertial confinement fusion. We had in our center a group, a very good group, on lasers. They took up this question of laser fusion for study, knowing very well that they could never build these gigantic lasers, but they could certainly study the physics of laser plasma interaction. There was a policy decision that our center would not enter magnetic confinement fusion, which was allocated to a different institute in India. So we selected what is known as a plasma focus, which is a variety of Z-pinch device, for investigation, trying to understand the phenomenon — and through it the basic physics of fusioning plasmas.

RG: And you were involved in that research?

MS: Yes, we have a plasma group in the Neutron Physics Division, which I head.

RG: How many people are there in the Neutron Physics Division?

MS: The division has about 30 people at present. The plasma group consists of about 8-10 people, and we have funding to build a 300 kilojoule capacitor bank facility to dump the stored energy into a plasma focus. We started with a very small plasma focus experiment in 1976. I think our center was the first to generate and detect fusion neutrons from a small plasma focus experiment driven by just 100 joules of stored energy. Nobody else to my knowledge has detected neutrons in such a low-energy experiment.

RG: How many people are at the BARC institute?

MS: BARC is a gigantic institute. We have about 14,000 people now. About 3,500 are scientists and engineers, and the rest of the people make up the technical and administrative support. It is the main center for developing nuclear technology in the country, and is primarily a research center. The power reactors are constructed by the nuclear power corporation, but BARC gives the support for it with research in heavy water production, research in nuclear fuel — the whole fuel cycle. Our division initially was looking after criticality experiments, and then started taking interest in fusion.

RG: It sounds like BARC is roughly equivalent in size to the Los Alamos National Laboratory here in the U.S., where I think there are about 10,000 people working.

MS: BARC is a bit bigger. That is because in India we don't have the kind of industrial back-up support that you have in this country. So we have to do a lot of things in-house. Right from the start, Dr. Homi Bhabha, who started the center, placed a lot of emphasis on self-reliance. For instance, we have our own division for vacuum technology. We have our own fuel fabrication facility. So although the size is large, much of it is production and other technical level work.

RG: So BARC is a National Laboratory in India. Does it have a "black" budget with secret research like Los Alamos, or is it purely non-defense-related research?

MS: There is no weapons program in India. There was, of course, one experiment conducted way back in 1974, but that was just one of a kind, just to demonstrate to the world that India has advanced technology.

RG: So BARC is not a defense-oriented laboratory, and is purely a science facility?

MS: It is definitely not a defense facility. I should say the main goal is to provide the R&D backup for developing the nuclear fuel cycle. However, BARC also serves as the premier institution for basic nuclear research in India. In this center we have research reactors, we have condensed matter physics, laser physics, chemical sciences, as well as life sciences. It is a nice mixture of basic and applied research.

RG: Do scientists like yourself have a sort of tenure?

MS: The interesting thing about our center — and in fact any government job in India — is that nobody can be thrown out easily. Once you become a permanent government employee, it is next to impossible to throw you out. In that sense, everyone has a tenured position. This has its advantages and disadvantages. If people work, it is out of interest. If a man decides not to work, nothing can be done about it, you just don't throw him out. This gives one a lot of flexibility and freedom. He can pick a problem and work on it. If his boss doesn't like it, he can ignore him or not give him much funding.

I enjoy working at BARC because we have people there with expertise in almost any field. If I want to talk to a person with expertise in an area that I need advice on, I look him up in the phone directory, and call him up, or walk across the building to talk to him. It is a very nice atmosphere. There are always seminars going on, or lectures, or symposia. There is an active science atmosphere, and we are exposed to a wide variety of scientific topics. Sometimes it is a little too much, maybe.

RG: Did this level of academic freedom that you enjoy help with your interest in cold fusion?

MS: That is certainly one of the main advantages we have. Because of this freedom, we could easily enter this exciting new area. Interestingly enough, at the time when the phenomenon of cold fusion arrived, one of the problems we were looking into in our division was the possibility of developing fusion into a neutron source in order to convert thorium into uranium-233 [U233]. Incidentally, one of the key projects in BARC is developing the technology for using uranium-233 in power reactors. Right now, we are constructing a U233-fueled swimming pool type reactor. It is expected to become critical just within a few months. And when it becomes operational it will be the world's only U233-fueled reactor.

RG: What is the advantage of the U233 reactor?

MS: U233 is a man-made isotope, like plutonium-239. It is produced from thorium. India has plenty of thorium, probably the largest thorium resources in the world. So right from day one, when our center was started, it was generally assumed and understood that eventually our nuclear technology would switch over to U233. So this is what we are interested in doing. The only problem with the U233 fuel cycle is that U233 is a man-made isotope, and to produce it you need a neutron source. So we examined the energy cost of the production of neutrons by almost every imaginable technique — all kinds of devices, including fusion reactors. That was the reason we entered fusion. If only we could develop fusion, not for getting energy, but as a neutron source. I personally have concluded that [hot] fusion reactors will never become feasible for getting energy. Today, I am even willing to say that [hot] fusion energy will never become commercial. But as a neutron source, if we can use those neutrons to convert thorium to U233, that might be useful, in the Indian context.

RG: Do you need a high-energy neutron?

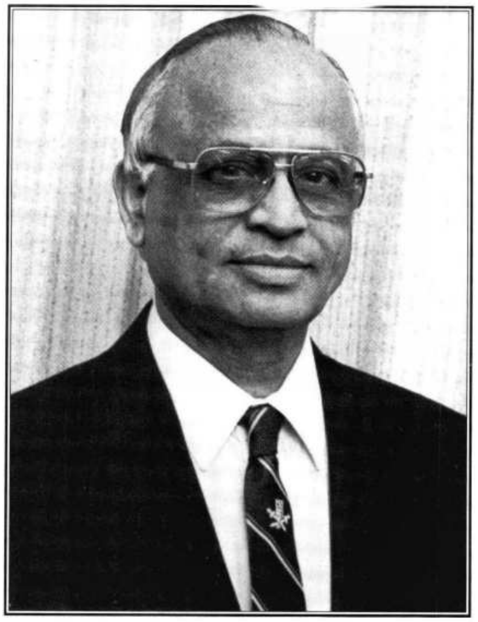
MS: No, any neutron will do. The important thing is that eventually, when this neutron is captured in thorium to give U233, one atom of U233 is going to give 200 MeV of energy. So the point to note is that if I spend more than 200 MeV of energy to produce a neutron, then the whole concept fails. We examined many schemes, and we were actively trying to develop the plasma focus as a viable source of neutrons. It very nicely fitted into our overall scheme of things. If only we could get a good neutron source!

RG: So this led you to cold fusion?

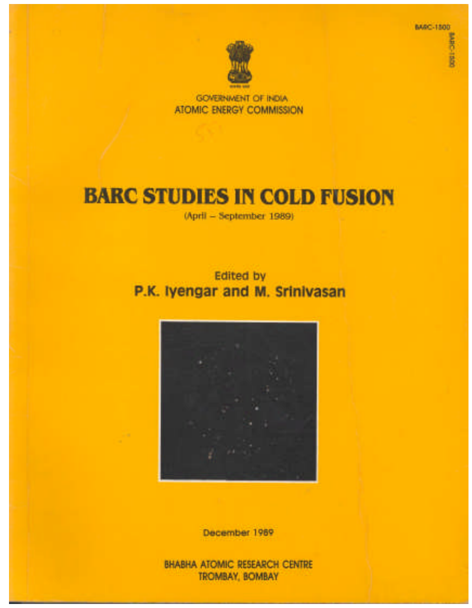
MS: Exactly. On March 25, 1989, we saw this little four-line news item in the *Times of India*, saying that neutrons were seen to be produced by a small battery and bottle experiment. I immediately got interested in it from the point of view of a neutron source.

RG: How quickly after you saw that news item did you begin work?

MS: That is a very interesting story. In 1988, we had imported from Ireland what is known as a Milton Roy Electrolytic Cell. This is a commercial cell for producing hydrogen gas. The important thing about the Milton Roy Cell is that it uses palladium tubes as cathodes and NaOH as electrolyte. During electrolysis, hydrogen goes through the walls of the tubes and comes out as pure hydrogen gas from the inside of the Pd [palladium] tubes. We were in the process of converting it to produce deuterium gas for our plasma experiments. On March 25th, purely accidentally, we had right there a palladium and heavy water electrolytic cell lying on our table — not because of cold fusion, but because we wanted deuterium gas. When we learned that this palladium and deuterium cell produces neutrons, all that we had to do was to switch it on and bring in the neutron detectors around it. So within 24 hours we had a “cold fusion” cell operating!



Dr. P.K. Iyengar, the open-minded Director of BARC at the time of the Utah announcement on March 23, 1989.



The cover of the BARC 1500 report, *BARC Studies in Cold Fusion.*

RG: So what did you see?

MS: Within three weeks, on April 21, 1989, we saw the first burst of neutrons. Not only did we see neutrons, we saw that there was plenty of tritium! That was the thing which really gave us confidence that cold fusion was for real.

The director of BARC at that time was Dr. Iyengar, who was a person who was always interested in exciting new things. So when this came up, he quickly convened a meeting of all the people who were likely to be interested. Different people got involved: the neutron physicists, chemists, chemical engineers, etc. So within a period of six weeks we had 12 groups in BARC, independently setting up cells and doing what they thought best to do. The result of that is the BARC 1500 report.[1] As you know, 10 of these groups got positive results. For some strange reason, however, we the neutron physics people were told that calorimetry is a very difficult thing to do. Somehow we were dissuaded from entering calorimetry. “You neutron physicists shouldn’t touch it.” A very strange attitude. So for two years we didn’t touch calorimetry.

When the announcement from Scaramuzzi in Italy came in May 1989 (neutrons from titanium in a gas loading experiment), we had a head start on that technique also! At that time, as part of our plasma physics activities, we had made titanium targets and electrodes for gas discharge experiments, to study neutrons in the context of plasma fusion devices. So it was as a result of this that I had in my drawer a piece of titanium that had been loaded with deuterium gas in May 1988 — well before the announcement of cold fusion. When I saw the Scaramuzzi announcement, I took out of the drawer a conical titanium electrode and I thought, “If something nuclear is going on, then if we keep this on an X-ray film something interesting should be seen.”

[1] Iyengar, P.K. and M. Srinivasan, eds. *BARC studies in cold fusion*. 1989, Government of India, Atomic Energy Commission: Bombay. Several papers from this book are available in the LENR-CANR.org Library.

So I gave this to my young colleague who does the X-ray film exposure for his plasma studies. He placed it overnight on X-ray film, and the next morning when he developed it, on the film where the tip of the electrode was there was a little spot. So the next day we repeated with a fresh film and again the spot appeared. Five nights consecutively, on five separate films, we saw that spot. We didn't know whether it was tritium or anything else, but we were sure something interesting was going on.

The question was: Was it X-rays coming from the titanium deuteride? This titanium electrode was earlier produced in a chamber using RF heating — all well-known technology. So we made fresh targets, and we looked at them on X-ray film — and we saw beautiful spots. And the same thing, placed in front of our germanium detector, gave us K-alpha 4.5 keV and K-beta 4.9 keV peaks, a clear signal of tritium in the material. This was in June 1989. The paper was presented at the Fifth International Conference on Emerging Nuclear Energy Systems in Karlsruhe in July 1989. We put one of those targets into our neutron detector, kept it there overnight and we got a beautiful big burst. We published this in the August 1990 *Fusion Technology*, a paper with 50 authors. At the time when the paper was published, we had probably the largest group of cold fusion researchers in the world. It was a very exciting time in those days. As I've said, we've seen tritium, we've seen neutrons, we've seen it in both palladium and titanium systems.

RG: Did you ever see sufficient neutrons to be of interest for your U233 program?

MS: No. We soon realized that the neutron-to-tritium ratio was very small. It was tritium that was the main product. Meanwhile we received confirmation of our results from Bockris of Texas A&M University, and Tom Claytor of Los Alamos, and others. We knew that even the tritium yield was very small compared to excess heat. So it was very clear to us that neutrons and tritium are only secondary phenomena. The main thing is clearly the excess heat.

RG: So you started out doing the classical Pons and Fleischmann heavy water experiment?

MS: Well, the Milton Roy Cell was readily available in our group and we didn't have to build a cell. It was fantastic, because we could drive that cell at a current of 100 amperes. And at that time in April 1989 I don't think there was any group in the world, not even Fleischmann's, who had a cell that could be driven at 100 amps! Since we saw neutrons and tritium with a Milton Roy Cell, we immediately ordered two more. Unfortunately, the second and third Milton Roy generators did not give us anything. So that was our first experience with non-reproducibility.

RG: Did you ever determine why that was?

MS: No, we didn't. We were just puzzled. Let me put it this way: We were probably drunk with success. Many of the BARC cells had given neutrons and tritium, so we thought cold fusion was simple! But when we started finding we could not reproduce it, the U.S. DOE report came out, the skeptics came out, and in our own center some of the senior physicists would not believe it. They challenged us to demonstrate the results again. But we could not again succeed with the Milton Roy Cells.

RG: But you were able to reproduce some of the time?

MS: Not with a Milton Roy Cell. But with other approaches, yes. For example, a very exciting experiment was the titanium chips experiment. I think the idea of treating them with liquid nitrogen was contributed by the Los Alamos group, by Howard Menlove's experiment. He took his deuterium-loaded titanium chips and dipped the whole cylinder into liquid nitrogen. It was supposed to have given neutrons. I am told that subsequently he has found that the apparent neutron bursts, at least in some cases, were possibly due to water condensation in the high-voltage insulators, and hence he has cooled down on that kind of experiment.

What we did was rather than look for neutrons, we took the deuterated titanium chips and dropped them directly into a can containing liquid nitrogen. Then we took out those pieces and monitored them individually for tritium. It was a tough problem because we had a thousand small chips with a total of about five grams. We divided them into lots of 20 and put them into a window-less beta detector. Some of the lots gave significant counts. Finally, we were able to show that four out of 1000 chips had very high activity at the microcuries level.

Those chips are still preserved by us — and they still give this signal. For instance, when Douglas Morrison visited us at the time around August 1990, I showed him that. The moment we loaded one of those chips into the detector, the count rate indicated a very high level of activity, giving a beautiful beta [electron energy] spectrum. I showed him this beta spectrum, and asked him to speculate as to where it could come from. I even gave him copies of the spectrum. He has never talked about it anywhere, or mentioned it in any of his writings.

Now the more exciting thing about that particular titanium chips experiment is that not only do *only* four out of the 1000 chips have that high activity, but even in those four chips there are very small hot spots — showing that what is happening is happening very selectively. There is clearly something very special about those sites. This is telling us something very important, because theoreticians immediately imagine a lattice which is fully loaded with deuterium, and that what is happening is happening everywhere in the whole lattice. I suspect that it is *not* occurring in the whole lattice.

RG: Do you mean that it isn't a uniform effect in the lattice?

MS: Definitely not. There is something unique in certain spots, and we haven't understood what it is. In the titanium chip experiments we came to the startling conclusion that each of those hot spots is the result of a micro-nuclear explosion. We gave that explanation in the paper we presented at the Provo meeting in October 1990. This, of course, is based on Menlove's observation of neutron bursts. If that is questioned, then the micro-explosion theory is not admissible.

There is, however, another interesting experiment we did by measuring the probability distribution of neutron counts. We did this to answer the question: In all these cold fusion experiments wherein we see neutrons, are these neutrons being emitted by the sample one at a time or in bursts of two, three, four or more at a time? In other words, was the neutron emission following Poisson statistics or was it non-Poisson? This is basic to the mechanism behind it, and so we devised an experiment to look for neutrons in 20 millisecond intervals because that is as far as we could go down to with our setup. All we did was feed the data out to a personal computer and chop it up into 20 millisecond blocks. We then did a statistical analysis and showed that definitely about 15-20 percent of the counts were coming in bursts of several tens of neutrons at a time.

RG: When was that experiment completed?

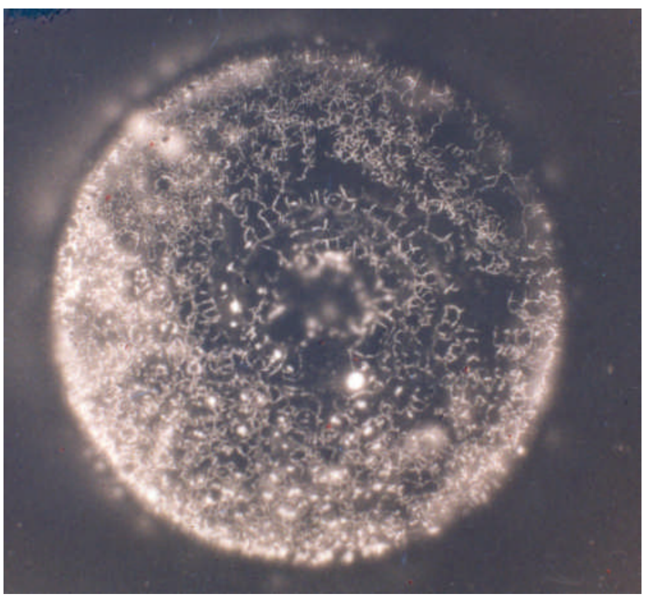
MS: Those were done in 1989. I am very happy about it because we presented the results at the first Salt Lake City meeting. Martin Fleischmann was so excited that he came up and said, "You fellows have done what I always wanted to do." He was very pleased.

An interesting thing we found was that the probability distribution of neutrons coming off a titanium deuteride disc, and from a palladium-D_2O electrolysis cell is similar. So there is something in common. The mechanism producing the neutrons is probably similar. I don't fully buy the theory that the d+d reactions are occurring continuously all the time. You cannot explain the multiplicity of neutrons this way. I know that there are some who now claim the multiple neutron measurements are erroneous.

But in our experiments we had two banks of neutron detectors looking at the source, and a background detector bank away from the experiment. Now, we believe our measurements, unless both these banks near the sample simultaneously decide to behave in a crazy manner or respond to cosmic ray-caused spallation neutrons, and not the background bank. In our experiments the background counter was absolutely stable. I am also aware of the argument that cosmic rays can cause spallation neutrons *only* in the Pd cathode and so do not give a signal in the background bank. But the size and duration of the detected neutron episode does not support the argument of the critics.

RG: So, what were the next experiments?

MS: Around October 1990, there were two considerations. First, we were having difficulty with reproducibility of the neutron and tritium measurements, and at the same time there was a feeling that we were missing out in terms of calorimetry. And that is when we decided to get into calorimetry. Just as we were building the initial calorimeter, this light water business came up. So instead of going into the calorimetry of palladium-and-heavy water systems, we started on the calorimetry of nickel H_2O systems, as we got the impression from Mills' paper and the Bush-Eagleton work that the success rate was higher in the Ni-H_2O system.



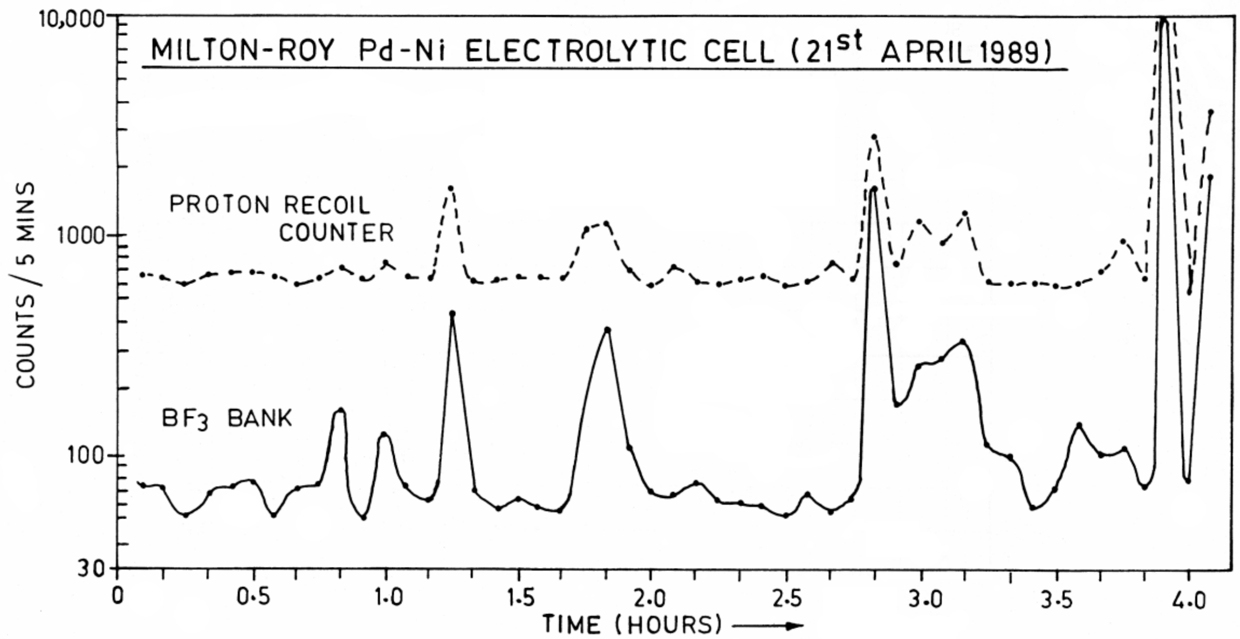
Autoradiograph of BARC deuterated titanium disc

RG: Is anyone at BARC doing palladium heavy water electrolysis experiments now?

MS: No, unfortunately it was also around that time, namely the middle of 1990, that there was a change in leadership at our center. Dr. Iyengar, who was the moving spirit behind the initial cold fusion program at BARC, moved on to become the chairman of the Indian Atomic Energy Commission. That has had an impact on the other groups involved in cold fusion experiments, though it didn't bother me. Many of the other groups did not want to risk their careers, and so they wound up their work. Many of the groups wound down their work. So in terms of numbers of people, we have come down from a level of 50 scientists actively engaged in cold fusion to about 15.

RG: So this is how you came to reproduce the light water experiments of Randell Mills.

MS: Yes, we simply tried to reproduce the experiments as reported by Randell Mills in his 1991 paper. We saw excess heat all right. Fifty percent of our cells showed it, although I now realize that we have perhaps not paid enough attention to ensuring that no recombination effects are occurring in our open cell experiments. My stay at SRI International during the last five or six months has opened up many issues which we are currently addressing.



Neutron counts variation during run No. 1 of Milton-Roy Cell (21 April 1989)

RG: Now that there are some new techniques emerging, are you going to look into any of them?

MS: Oh, yes. As I had mentioned earlier, we have conducted Z-pinch-type plasma focus experiments for the past 15 years. When we first heard of the Scaramuzzi neutron burst experiments from Italy, we ran a plasma focus experiment with a titanium electrode in place of the normal copper or brass central electrode. To our surprise, one of those electrodes gave us fantastic quantities of tritium. That was again presented in the first Salt Lake City conference. One particular electrode gave us 400 microcuries of tritium, but we could never reproduce it again. We still have that electrode. It gave us beautiful autoradiographs for a year and a half. Every month we'd take it out of the shelf and keep it on an X-ray film. The same pattern would reproduce itself, very beautifully.

RG: How many times did you try to reproduce the experiments?

MS: Many times. But without any success. Two other titanium electrodes did give tritium, but not high amounts. Only one electrode gave us such impressive results. You could see the tritium by sticking it in front of a germanium detector and the K-alpha peak built up rapidly. Stick it inside an ion chamber, and it gave measurable currents.

So we do have a historical interest in the plasma discharge approach. We still have a group pursuing this approach. When I go back I hope to strengthen the effort in the glow discharge type of experiments.

RG: What do you think about the Kucherov-type glow discharge work?

[Work reported in Physics Letters A. *November 1992. Ed.]*

MS: I think it is very interesting. As I said, we have the experience of the plasma focus experiments which gave us tritium. I hope to visit Los Alamos, before I return to India as this kind of experiment is being conducted by Tom Claytor and his colleagues. They seem to be having good success with this kind of experiment. We have some people trained in this area, and also have all the equipment. It is just a question of getting them interested in such experiments.

RG: So you can do the Kucherov version of this method with the palladium electrodes?

MS: Certainly, we will try that. I am also very interested in the ultrasound cavitation experiment of Roger Stringham and yourself. I also hope to try and set it up at BARC.

RG: Have you followed the Dufour experiment of Shell Oil in France?

MS: Yes, I first heard about it several months before the Maui meeting. The calorimetry in that appears tricky, but Dufour seems to have paid attention to most of the obvious questions. I am now convinced from the variety of phenomena we are seeing that we are on to something very exciting. Intuitively we feel there is a commonality to all of this.

RG: So the result of all our understanding about cold fusion is that the nucleus of the atom is not nearly so inviolate as we had thought. Nuclear reactions are much more accessible, and we can produce them with techniques more like chemistry than we would have thought possible a few years ago.

MS: Yes, certainly. I fully agree with you.

RG: What do you think is the future of cold fusion? Do you think that with Pons and Fleischmann having these boiling cells — and working on commercial versions of them — that commercial products will really emerge?

MS: Absolutely. I am optimistic that the first commercial product will be a home heater. Whether it will be hydrogen-based or deuterium-based is not clear at this point. I know that proponents of both are working towards that. I won't be surprised if the devices that eventually emerge have nothing to do with electrolysis and are completely different. In fact, I understand that Randell Mills has already shifted to gas-based systems which don't use electrolysis. The phenomenon is real. There is no doubt that Fleischmann and Pons have unraveled something very fundamental in physics. Unfortunately, the reproducibility of most of the systems is very poor. The cold fusion community very badly needs something that is reproducible which we can switch on, and show to the skeptics that here is proof of "new physics." The day you are able to do that there will be a quantum change — a big leap in terms of funding and changed attitudes. Even if it means diverting our attention from something else to this task, it must be done.

RG: How would you rank cold fusion today in terms of other forms of alternative energy?

MS: Well, I have gone through solar energy. I have personally built a solar collector. Of course, solar energy is going to play an increasing role, but it cannot provide all the energy needs of a developing country. What else is there? Everyone is running out of coal, oil, and all the rest. As for nuclear energy, especially fast [fission] reactors and hot fusion, I think they will die a natural death. They are uneconomic and impractical for various reasons. That leaves only thermal reactors which have a mixed reception in many countries. LWR (light water reactors) and some version of advanced thermal reactors will be around for another 20-30 years. But because of nuclear waste problems it will have to phase out too. There is the famous "NIMBY" philosophy: "Not In My Back Yard." Nobody wants nuclear waste to be stored for 300 years or 20,000 years in their territory. It is the fear of nuclear waste, even if that fear may be unscientific, which is going to kill nuclear fission energy sooner or later. So I definitely think cold fusion has potentially a very important role to play. But it implies new physics. The important thing is that the younger generation has to realize there is new physics involved. Unfortunately, there is a gigantic international conspiracy to prevent young people from even looking at new things.

RG: What do you think would happen if cold fusion were suddenly accepted as a real, viable energy source in the world?

MS: I always draw a comparison between cold fusion and the personal computer and the electronics industry. It is nice and small and ideally suited for mass manufacture. What I like about cold fusion is that for the first time here is an energy source that need not be promoted by the government. It is ideally suited for exploitation by private industry. Because of its small size there is a possibility for rapid technological innovation and improvement. You cannot do this with other forms of nuclear energy, because it takes 15 years and billions of dollars to do anything with a nuclear reactor. Then you operate it for 15 years to gain experience, and then build a new model. That is one of the reasons why neither fast breeder reactors nor hot fusion are likely to succeed in the marketplace.

The beautiful thing about cold fusion is that it is small. Small is beautiful.

The day we understand the physics, everything is going to rapidly change. There will be competition. You will have cavitation systems, gas-based systems, electrolytic systems, all kinds of things going on eventually. Technology will settle on two or three concepts. They will all be tried out in the marketplace. Fierce competition will drive innovation.

RG: Do you know of other Third World countries working in this field?

MS: No, I am not aware of any, except perhaps Taiwan. One of the most amazing things to me is that we have not seen anyone from Israel working on it. They have very capable people, and I am very surprised that they are not involved. I've not seen a single cold fusion paper come out of Israel.

Most third-world countries look up to the U.S. and the West. If the leading scientific bodies, bigwigs in the U.S., like the American Physical Society, say cold fusion is nonsense, immediately the message is passed on to top people in developing countries. This is happening in my own country. Some of the top people in the Indian national scene think that cold fusion is an illusion.

RG: So the negative opinion of cold fusion that is promoted by the American Physical Society or the British Physical Society is very influential?

MS: There is no doubt about it. Scientists all over the world tend to look upon these societies as the people who are spearheading the advancement of science and high technology. So their opinion is very important. If these bodies say cold fusion is bunkum, then they feel that most probably believe it is bunkum. That is the attitude that most top scientists in most developing countries seem to have taken. In this context, the open-minded approach of Japan, Russia, and Italy is to be lauded. I hope it will help to form a favorable climate for cold fusion in other countries too.

RG: So what's next for you? You're going back to India, to BARC?

MS: Yes. I hope to continue to push cold fusion. We need someone to give enthusiasm and encouragement to the various internal groups, and particularly to young people who don't want to risk their professional careers. To me it is a very sad thing. I always thought that you are adventurous when young. You are supposed to be willing to take risks. But oftentimes, career advancement comes ahead. People are willing to work in the area of high temperature (high T_c) superconductivity or fullerenes or whatever, which is a "hot" but "safe" topic.

RG: So that's part of your role. That's why you've been here at SRI as visiting scientist for the past six months, and you've been able to visit with other scientists in the United States.

MS: Yes, this way I get a global perspective of what is going on.

RG: A couple of years ago you made a tour of labs around the world, visiting the Japanese labs and labs here in the United States, and you wrote a report to the Indian government on the field. What was the effect of that report?

MS: First of all let me clarify, there was no formal report addressed to anybody in the Indian government. Yes, I keep writing informal notes and letters to convey to my colleagues what I have learned. I believe these efforts to bring to their notice recent progress in the field have helped create an improved climate. I hope that *"Cold Fusion"* magazine will play a useful role in this context.

RG: What do you think about the new Japanese program set up by their Ministry of International Trade and Industry?

MS: The way the Japanese are approaching the subject is very laudable. We have seen to it that this news has reached the decision-making people in India.

RG: What do you think about the American physics community and chemistry community who have taken such a hard line against cold fusion in this country?

MS: It is really very disappointing to see how these groups have networked and suppressed this field. But I am very encouraged that there is a large non-established scientific community that will discuss anything in science here, in spite of the stranglehold by the established scientific bodies. Only after coming to this country have I realized that there is a vibrant, off-the-beaten-track movement in science — and pseudoscience. They are in good contact through newsletters, e-mail, etc. Particularly in California, people are working in all kinds of scientific areas. I am happy there is this large underground community with a very open mind, where people conduct experiments in their own home laboratories. I am very happy to see this. I think that this is going to save the scientific community.

RG: Well, I think we've pretty much covered the field. Do you have anything more that you'd like to add?

MS: Yes, one thing. I would like to congratulate Wayne Green and Gene Mallove, and others, for starting this magazine. [2] I wish Gene good luck. The magazine is going to play a very important role. I have only one request for him. If this magazine is to reach every nook and corner of the world, he must come up with a dual price policy for the magazine so that the people in the developing world can afford to buy it. I believe an issue will cost $10. That is 300 rupees in India, and that is 10 percent of the monthly salary of a young scientist. Our center can afford to subscribe to it, but many universities cannot afford this.

In the interest of spreading the message, there must be a cheaper way to get the magazine to these people. It must reach the interior parts of Russia, China, India, and others in the developing world. It must reach the universities where the young people must get interested in this new field.

RG: It seems to me that the critical factor in this field is the shortage of people working in it. How would one encourage more bright young minds to get into this field?

MS: The best people are not entering this field as they are being diverted away. Once there is a general acceptance that cold fusion is real, automatically more people will enter the field. The day the American Physical Society accepts cold fusion, or the Department of Energy accepts cold fusion as a real scientific field, hundreds of people all over the world will jump into it within a fortnight. I can guarantee it!

[2] Note added in 2006: This magazine was succeeded by *Infinite Energy* in the next issue. Mallove was killed in 2004. Young scientists in India, and the world over, can now read about cold fusion at no cost on the Internet, at http://www.infinite-energy.com/, http://lenr-canr.org/ and other sites.