ICCF16 in India: A Historic Perspective

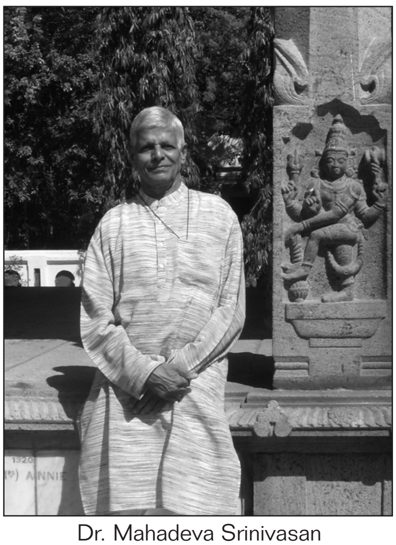
Marianne Macy

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.”

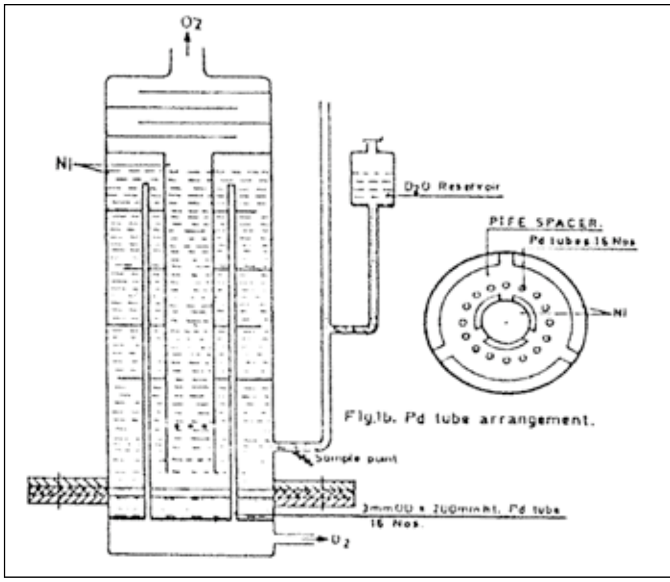
A key figure in BARC getting involved in cold fusion research was its director, Dr. P.K. Iyengar. Srinivasan notes, “He is one of those people with an open mind, sort of an adventurous kind of person who was willing to look at any new idea and explore it. He also saw the same news item and he called 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.”

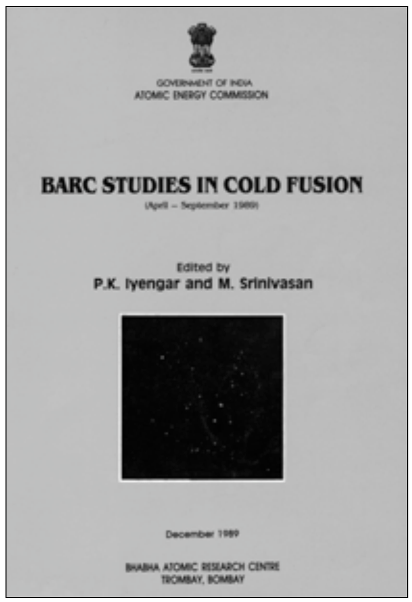
Instead of purchasing deuterium gas, it had occurred to Srinivasan, “Why not produce our own deuterium oxide?” At BARC, the Indian nuclear program was using CANDU reactors, which provide very efficient power and use heavy water as the moderator. India had gigantic plants producing heavy water. “We had 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.


GOVERNMENT OF INDIA
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BARC STUDIES IN COLD FUSION
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BHABHA ATOMIC RESEARCH CENTRE
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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 postdoc 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.]