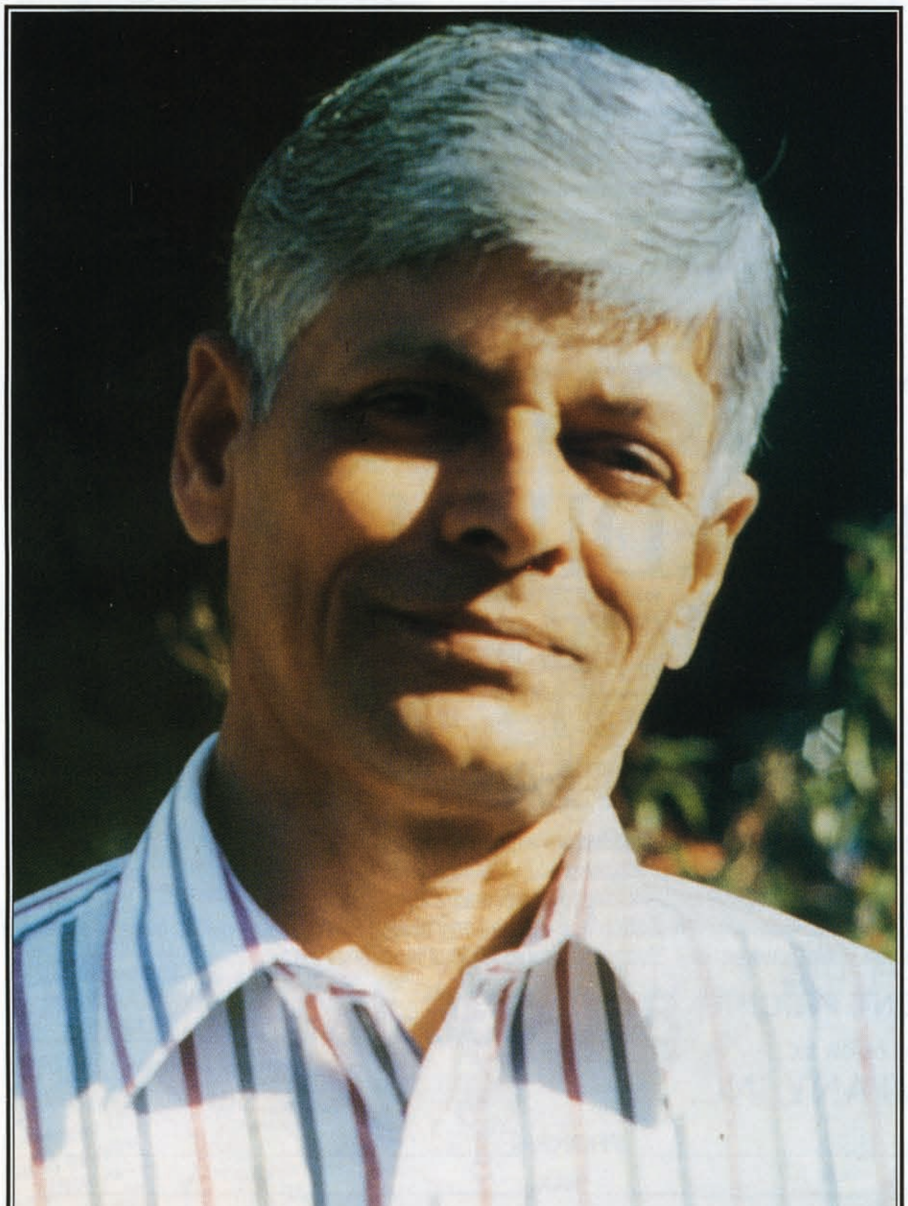
'The cold fusion phenomenon

An interview with Dr. Mahadeva Srinivasan

Dr. Mahadeva Srinivasan is the head of the Neutron Physics Division and an Associate Director of BARC (Bhabha Atomic Research Center) in Bombay, India. This interview was held on March 1, 1994 at SRI International in Menlo Park, California, where Dr. Srinivasan was a visiting scientist, working in the laboratories of the Energy Research Center.



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) 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?

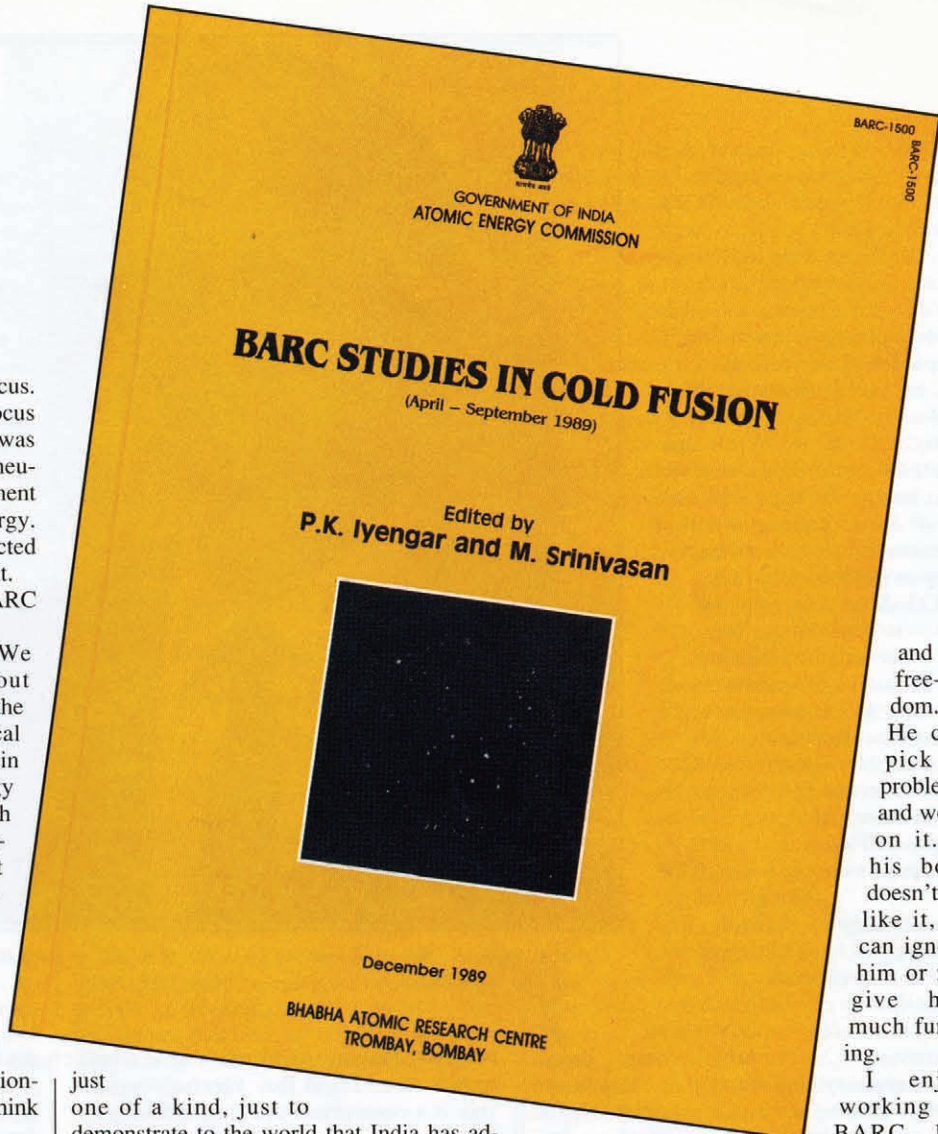
MS: Yes, we have a plasma group in the Neutron Physics Division, which I head.

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

is real.'

By Russ George



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,

'I am optimistic that the first commercial product will be a home heater.'

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!

RG: So what did you see?

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

'The phenomenon is real. There is no doubt that Fleischmann and Pons have unraveled something very fundamental in physics.'

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. 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 headstart 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." 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.

'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.'

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 windowless 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.

Autoradiograph of BARC deuterated titanium disc.

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.

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.

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 auto-radiographs 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.

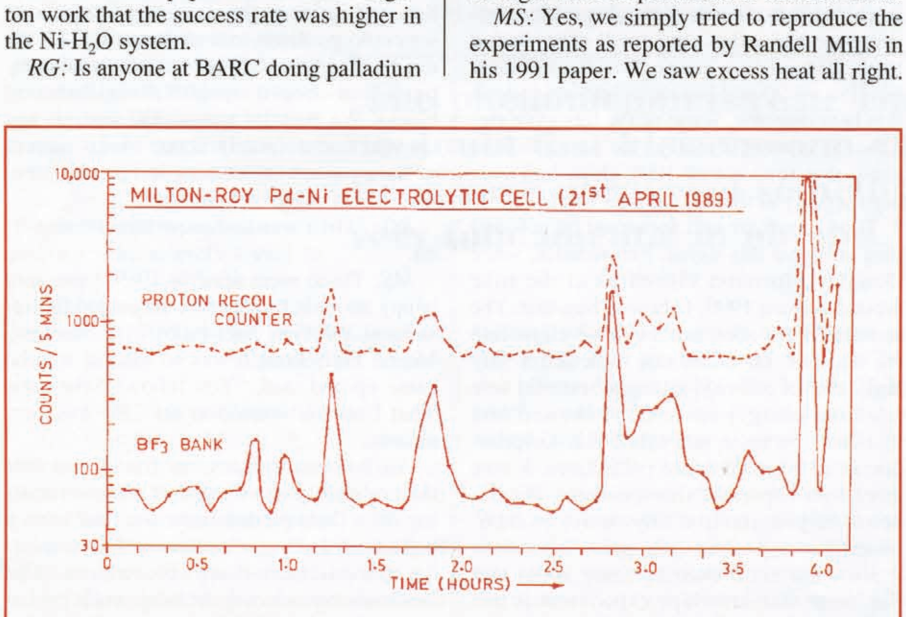
So we do have a historical interest in the plasma discharge approach. We still have a group pursuing this approach. When I go back I hope to strengthen the effort in the glow discharge type of experiments.

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?



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

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.

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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?

'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.'

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. 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 dollars. 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!

Russ George is a well-known figure in the international cold fusion community. He was a visiting researcher at Los Alamos National Laboratory and SRI International. He is currently a researcher and vice-president at E-Quest Sciences in Palo Alto, California. At E-Quest, he is in charge of efforts that led to the recently-announced availability of the first commercial cold fusion device in the U.S., the E-Quest Mark II research device. The MK2 makes use of E-Quest's proprietary ultrasound-induced cavitation method to provide reproducible micro-fusion reactions.

[Ed. Note: *"Cold Fusion"* Magazine has decided to donate several dozen subscriptions to science institutions throughout India.]

The yuga of electricities

A recollection of Vedic mythology

By Wm. A. Boas, Jr.

India, a land of eternal mythologies, is active in cold fusion research. Besides the revelation of March 1989, scientists there can draw inspiration from recorded Sanskrit accounts of advanced technologies prevalent thousands of years ago during their golden ages of cyclic yugas.

Manu, one of their ancient sages, records the yugas as four distinct periods which in Sanskrit are called Satya, Treta, Dwapara and Kali. It's said that they follow each other as long as Brahma lives. Kali is the shortest. Dwapara follows, being twice as long. Treta is next, twice as long as Dwapara, followed by Satya, twice the length of Treta.

Each age has a particular influence on earthly affairs, thought and knowledge. Satya is the golden age attributed to complete understanding of certain and all scientific principles. According to Vedic tradition, as the periods shorten, understanding diminishes such that the age of Kali is completely materialistic—and one understands only what one can be feel and see at hand.

Like orthodox science of the West, the orthodox contemporary Hindu calendar accepts that we are 5,094 years into the "dark" age of Kali which is thought to have a length of 432,000 years—a dismal prospect. However, like cold fusion cognoscenti, there exist other streams of vedic wisdom in India that say the current Hindu calendar is wrong because a mistake was made in calculations about 700 BC.

One critic of the presumed error is Swami Sri Yukteswar (1855–1936), the teacher of Paramahansa Yogananda who subsequently came to America and founded the Self-Realization Fellowship that still exists in California. Sri Yukteswar published a little volume called "The Holy Science," that's gone through several editions under Self-Realization Fellowship auspices.

In his short tract, Swami Yukteswar explains that one of the problems of orthodox Hindu calendar makers was assuming the classical yugas had a length 360 times longer than they actually had. After explaining why and expounding some elaborate astronomy which can be followed with a calculator and ephemeris, the Swami concluded that about 300 years ago, the world finished a 1,200 year cycle of Kali yuga and entered a 2,400-year age of Dwapara yuga, which has as one of its qualities the understanding of the principles of "electricities" and concomitant sciences.

He cites the work of William Gilbert, Kepler, Galileo, Newton, Thomas Savery, and Stephen Gray as people who started to bring the natural sciences out of the dark ages. The electrical research of Abbe Nolle, B. Franklin, and subsequent others of that yuga-transitional period can be added to the list for incidental support to his thesis.

Yukteswar believed that the four Yugas evolve within a 2,400-year cycle during which our sun revolves around some other body and causes the precession of the equinoxes.

He further reports that our sun and its dual has another motion about another intense energy center, and when the Earth and Sun's orbit is closest to this center is the age of supreme enlightenment, Satya Yuga. As the Earth and Sun recede from this energy center in their orbit, the subsequent ages of decreasing awareness set in. The cycle therefore has an apogee and perigee phase that is related to overall earthly intuition, knowledge, and understanding.

Thus we are now, according to the calculations of Swami Yukestwar's disciplic tradition, back on an ascending phase of knowledge, on the way to perigee with the grand center of "Brahma" which will occur in about 10,000 years. Thus, once again, we're said to be about 300 years into Dwapara yuga and capable of understanding the subtleties of electrical and magnetic sciences.

In sum, the mechanistic and dogmatic principles of scientific materialism and other fossil institutions, still partly rooted in Kali yuga, are giving way to unfolding phenomena like cold fusion. Why not Dwapara yuga and the revelation of the "new electricities"? The late mythologist Joseph Campbell would have loved this new mind-boggling aspect of the proverbial power of myth.