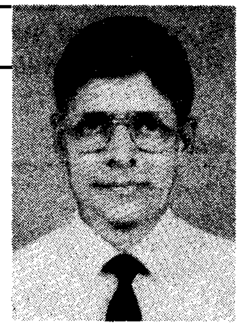
Interview: M. Srinivasan



Cold fusion has opened a new chapter in physics

Dr. M. Srinivasan is head of the Neutron Physics Division at the Bhabha Atomic Research Center (BARC) in Bombay, India. Well known in the international fusion community, Srinivasan has presented BARC's results in cold fusion at several international conferences.

He was interviewed April 3, 1990 by Marjorie Hecht, just after he attended the first international conference on cold fusion in Salt Lake City.

EIR: What is the scope of the Indian cold fusion program?

Srinivasan: There have been about eight different groups from different divisions in BARC who more or less jumped into this field as soon as we first heard about this Fleischmann-Pons effect back in April 1989. So this involves approximately 50 scientists in eight different groups in 14 different divisions. There are about 20 papers included in the BARC 1500 report [published in December 1989]. In fact, they are all going to be published in the July 1990 issue of *Fusion Technology,* so the American audience will have a chance to see them.

On the whole, I think we have gotten some very positive results. We are very happy with it, and we were very happy to learn that in the Utah meeting, for example, we got a very good response. . . . In fact, we were told we were probably the leading laboratory in the world in terms of getting very positive results, in terms of the number and variety of results.

EIR: Could you summarize the variety of results BARC has achieved?

Srinivasan: BARC's experiments have primarily been concentrating on the detection of nuclear particles, primarily neutrons and tritium. We figured that any measurements of excess heat might get embroiled in controversy because of the question of chemical reactions, and so on. So, we thought that this might be a better idea, to demonstrate to ourselves primarily, and to others, that nuclear particles are being produced.

As I said, about eight, quite independent groups—almost competing with each other—jumped into the game. They come from very diverse backgrounds. Most were chemists, some were electrochemists, some had been working in the area of hydrogen storage, some in water chemistry, some in enrichment processes in heavy water. And there were people like me who have been in neutron physics for many years. Also, some of us have been associated with plasma fusion experiments. We have a variety of people with diverse backgrounds who have devised and set up very independent cells. . . .

Almost all of these cells have produced neutrons *and* tritium. The most characteristic feature of the BARC experiments is that we find that neutrons are produced in bursts, one or more bursts lasting from somewhere between 20 seconds up to almost 1 hour. Some are 15 minutes, some are 20 minutes, and so on. The first point is, neutrons appeared in bursts, and when we talk of bursts, the neutron output is not just a few standard deviations above background, but 10, 20, even 100 times the background level. This is absolutely unquestionable. The BARC 1500 report has detailed graphs, figures, everything.

The other important thing is the tritium. Really, the first burst of neutrons and tritium was observed as early as April 21, 1989, almost within four weeks of the announcement from Utah, and the most important observation to come out of that first, early result was that the neutron to tritium yield-ratio was as small as 10^{-8}. We were perhaps the first group in the world to publish this figure. It was published in the July meeting at Karlsruhe [Germany] last year, and since then we have been, of course, attacked by a number of groups, and a number of others have verified it. . . . So this is one of the puzzles in cold fusion: Why is the branching ratio as small as this? [The branching ratio refers to the frequency of occurrence of particular nuclear outcomes, or branches, of a nuclear reaction: In deuterium-deuterium fusion, equal yields of neutrons and tritium would be expected—Ed.]

EIR: How long does it take on average between the startup of the cell and the occurrence of the bursts?

Srinivasan: This is what we call the switching-on time. . . . BARC results have very, very surprisingly been that most of the cells have produced neutrons on the first day of switching on the cell. This is something that we were very surprised to learn, that we were probably the only group [where this

Summary of successful electrolytic cell experiments conducted at Bhabha Atomic Research Center

BARC division*	HWD/ NtPD	DD/HWD/ NtPD	DD/HWD/ NtPD	ACD	ROMG	WCD
Paper no.	A1	A2	A3	A5	A8	A7
Cathode:						
Material	Pd-Ag	Pd-Ag	Ti	Pd	Pd	Pd
Geometry	16 tubes	Circular sheets	Rod	Hollow cylinder	Cube	Ring
Surface area (cm^2)	300	78	104	5.9	6	18
Pretreatment	Activated	—	—	—	Vacuum heated	Vacuum annealed
Anode material	Ni	Porous Ni	SS	Pt gauze	Pt mesh	Pt mesh
Electrolyte:						
Type	5M NaOD	5M NaOD	5M LiOD	0.1M LiOD	0.1M LiOD	0.1M LiOD
Volume (ml)	250	1,000	135	45	150	250
Cell current (A)	60	62	40	1 to 2	0.6 to 0.8	1
Current density (mA/cm^2)	200	~800	<600	~200	~170	60
Total Amp-hrs (no. up to first neutron emission)	180	250	120	17.5	14.7	620
$Amp\text{-}hrs/cm^2$ (no. up to first neutron emission)	0.6	3.2	1.2	3.0	2.5	34
Integrated neutron-yield	4×10^7	4×10^6	3×10^7	3×10^6	1.4×10^6	1.8×10^8
Tritium yield:						
Atoms	8×10^{15}	3.6×10^{15}	1.4×10^{14}	7.2×10^{13}	6.7×10^{11}	1.8×10^{11}
Picocuries	375	190	7.0	3.8	0.035	0.009
Neutron/tritium yield ratio	0.5×10^{-8}	1.1×10^{-9}	$\sim2\times10^{-7}$	4×10^{-8}	1.7×10^{-8}	10^{-3}
$Neutrons/cm^2$ (10^5 $neutrons/cm^2$)	1.7	0.1**	2.9	1.7	2.3	~100
$Tritium/cm^2$ ($picocuries/cm^2$)	1.3	0.5**	0.07	0.6	~0.01	5×10^{-4}

*The six BARC divisions that performed the experiments were: Heavy Water Division (HWD); Neutron Physics Division (NtPD); Desalination Division (DD); Analytical Chemistry Division (ACD); Reactor Operations and Maintenance Group (ROMG); Water Chemistry Division (WCD).
**Five surfaces considered.

Source: BARC Studies in Cold Fusion, April-September 1989.

The experimental results summarized were achieved at BARC from April to September 1989, using a variety of cathode-anode configurations. As one of the top nuclear laboratories in the world, BARC had the expertise to measure both neutron and tritium levels very precisely. The most important result of this initial set of experiments was the surprisingly low overall neutron to tritium yield ratio. The BARC researchers concluded from these larger-than-expected amounts of tritium that cold fusion is essentially "aneutronic." This is considered beneficial, in that the more neutrons that are produced, the more radioactivity is involved in the process.

happened], and not just in one cell, but several cells. There is a table given in the BARC 1500 report summary that shows this (see above).

I talked to all the various BARC groups. I was co-editor of this report along with Dr. P.K. Iyengar, who is now the chairman of the Indian Atomic Energy Commission. He has been giving us a lot of support; he's a great believer in cold fusion.

Now, we find that we have to charge it by a few ampere-hours per cm^2 of cathode surface area. We find that the tritium production is more or less a surface effect. It is the cathode surface area that mattered, so we normalize all of our results from the different cells accordingly. Some have 300 cm^2, some have only 1 cm^2; some have 100 amperes of current, some have only 1 ampere. So we normalized the results and looked at the ampere-hours per cm^2 at the total charged part up to the first neutron burst.

This, surprisingly, came out to be a few ampere-hours

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per cm^2, somewhere between two and three for most of the cells. Later, in fact, only in the last few days, I realized, carefully observing the results, it is only those cells that have used sodium deuteroxide [NaOD] as the electrolyte, that have produced the very early bursts. And, in fact, we find that we are probably the only group in the world who have been using this as an electrolyte, rather than lithium. So by comparing notes with other people in Salt Lake City, maybe this is the secret of our very good results. This is totally against the findings of many others. Even Fleischmann was very surprised when we mentioned this to him. So that is one of the interesting findings.

EIR: Others have taken days to get their cells to produce a burst—or anything.
Srinivasan: Yes. Even in BARC the groups that have used LiOD [lithium deuteroxide], have taken many days. So this is a very interesting observation. . . . Somehow, from the results of Fleischmann and Pons, and also one of the groups in Texas, they found that when you replace the LiOD with NaOD, the excess heat drops immediately. So they have been put off in the last several months, generally feeling that NaOD was a useless material, as excess heat is concerned. We are now beginning to believe that the mechanism which produces the excess heat may be quite different from the mechanism which produces tritium. So when I say it is good, it is probably so for the tritium and neutron production.

EIR: How would you explain that some cells in different experiments have gotten all three products?
Srinivasan: Very few cells have gotten that. In fact, there is only one cell—C.D. Scott from Oak Ridge [National Laboratory in Tennessee] as far as we know. But for most of the results, let me put it like this: The neutron yield seems to be the least in terms of quantity. The next most important is tritium, which is about 10^8 times, and then comes heat. If you look at the heat output, which is as high as a few watts, many groups have reported 10, 20, 30% excess heat. Now, that kind of excess heat you are unable to explain even from the large tritium that we measure.

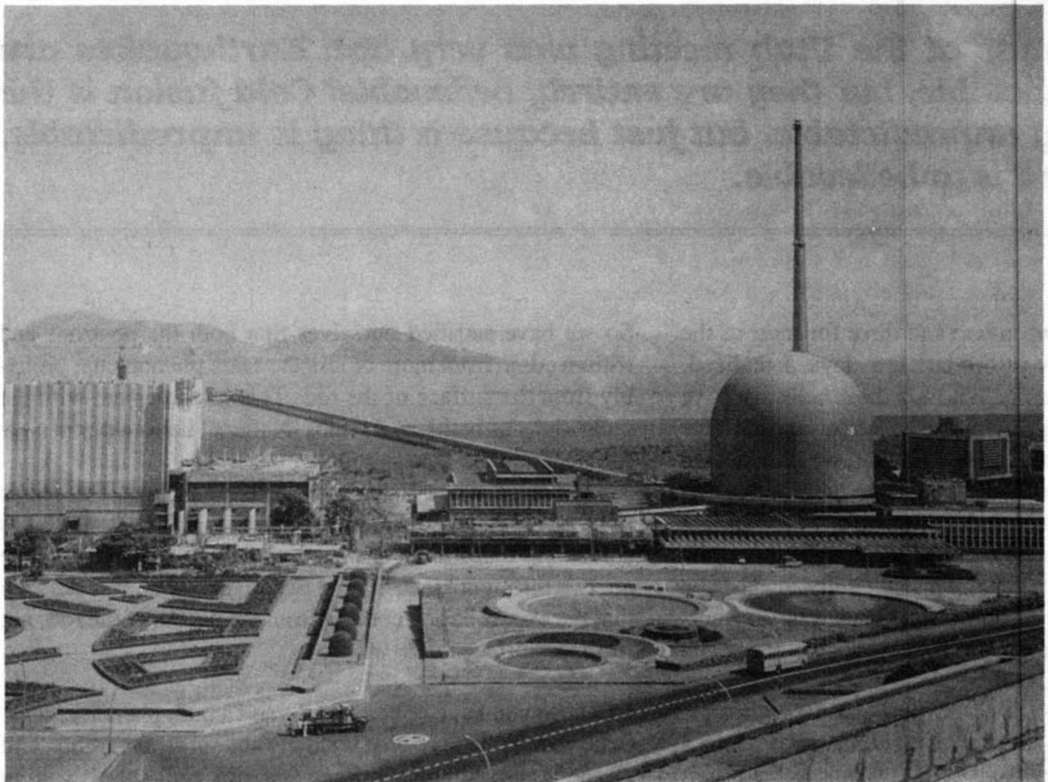
BARC results have shown, by and large, that both the neutron bursts *and* the tritium bursts occurred more or less simultaneously. We have seen this in several results—several cells—and nowadays we have an online tritium monitor. So we have satisfied ourselves that both the neutrons and tritium come from more or less the same phenomena, probably from the surface of the rod. This in agreement with the philosophy of both Los Alamos [National Laboratory in New Mexico] and the Texas group.

However, when you now come to excess heat, that seems to be a completely different aspect of cold fusion. So this is something that I think slowly seems to be emerging. There are two different kinds of cold fusion reactions. It is possible that the excess heat is coming from the depths of the rod, not from the surface.

EIR: So you have both a surface and a bulk phenomenon.
Srinivasan: That is what is slowly and surely emerging, and all the argument about surface and bulk probably is getting thrashed out. It appears that tritium and neutron production is coming from the surface, and the heat from the depths of the rod. However, I must admit that the kind of neutron bursts or the low-level neutrons, which is now being called the Jones-type of neutron emission, is [something else]—I don't know. We have always found neutrons coming out in characteristic bursts.

So these are some of the main findings. Let me also mention one more, very interesting result that we have. We have been looking at the statistics of neutron emission. For example, we wanted to find out whether the cells are throwing out neutrons 1 at a time, or in big bursts of 100, 200, 300. This is very crucial to answering the mechanism of neutron production. People have been talking of muon-catalyzed fusion, of fractal fusion, of coherent processes, and so on. So we did a whole lot of studies, and this is given in one of the papers in the BARC report, and we do find that about 80% of the time, the neutron emission is primarily Poisson-distributed in nature. However, 15-20% of the time, we still get bursts of neutrons in which the cell seems to throw out about 1,000 neutrons at a time, so we were very happy to learn that [Howard] Menlove of Los Alamos has also found a similar phenomenon.

These are some of our results. I think the most impressive feature from what I could hear from the reaction of other people is the large number of cells which we have been continuing to get even after the publication of the BARC 1500 report. And in the last two or three months, we have had about six more new cells that are not covered in the report. On the day I left, there was a very beautiful, big burst



Bhabha Atomic Research Center in India. Unaffected by U.S. press lies about the "failure" of cold fusion, Indian scientists were able to repeatedly produce excess heat and tritium in just six months after the Fleischmann-Pons announcement.

in one of our cells. So things are looking up. Of course we don't completely understand what is going on.

EIR: So you have actually been able to reproduce the initial experiment, and continue to get the same kind of results.
Srinivasan: Let us not use the word "reproduce," because we have used a variety of cell configurations. Some of them titanium, some are tubes, some are cubes, and some are rods, and all kinds of things. What the BARC results have shown is that there is nothing unique about any basic cell configuration. There is something more fundamental. Even titanium—we are slowly switching over to titanium. For one thing, titanium is made in India. It is also a thousand times cheaper, per kilogram. I personally feel that if cold fusion is going to have some future, it may be that titanium is the correct metal to switch to. Palladium is *very* expensive, at least for us in India.

Let me come to the second part of the BARC results, and this is the question of gas-phase loading. We have done a number of measurements, and in the most recent measurements we have used the plasma focus device. You remember this from the work of Winston Bostick of the Stevens Institute whose work you have reported in *21st Century* [see *EIR*, Feb. 8, 15, and 22, "The pinch effect revisited," by Dr. Winston Bostick]. Bostick visited us in India many years ago, and I have always admired the consistency with which he has worked. . . .

In the last month we have found some very interesting autoradiographs using the plasma focus device with titanium as the anode. We got some very *beautiful* autoradiographs! . . . These are very new results. We haven't published them anywhere. I just flashed a few slides at the Utah meeting. . . . I would like to go back and reproduce those results. I have a sneaky feeling that maybe the plasma focus will make it almost reproducible. . . .

EIR: How do you use the plasma focus device with the cell?
Srinivasan: We just replace the central anode with a titanium one [and use the plasma focus to charge it with deuterium]. After about 70 or 80 shots in a small 3 kilojoule plasma focus, we take the anode out, after a few weeks. We have developed the technique of autoradiography as a method of registering the presence of tritium. We get beautiful, spatially resolved pictures.

So after almost a month of charging the anode with deuterium in a plasma focus, we took an autoradiograph, and we got such a beautiful picture that we could even count the k-alpha x-rays coming from it. In fact, we stuck it in an ion chamber and we could measure the current being emitted—pico-amperes of current. I believe we have produced about 10^{16} atoms of tritium, which is "impossible." The 3 kJ plasma focus would produce about 10^{7} neutrons per shot, through a hot fusion mechanism, and correspondingly about 10^{7} tritium atoms. So, we cannot explain this 10^{16} atoms of tritium on the surface of the anode by any mechanism other than cold fusion. . . . This is a million times more than what we should expect.

In a sense, we are getting back to the same neutron to tritium ratio again of 10^{-7}, the same as we have seen in the electrolytic cell. But I must admit, these are *very* preliminary results, which I just got the day I left. So I would like to go back and reproduce them and check them once again, as far as the plasma focus is concerned. . . .

EIR: Most people in this country would be amazed to learn of BARC's very positive results and those of other researchers, because the press coverage implies that cold fusion is a mirage.

Srinivasan: I must say how appalled I have been at the way the press has been behaving, at the way people are writing books against cold fusion, so mean. . . . It is an education for me. I am amazed and appalled, I cannot explain it, why people go to this extent. . .

EIR: It has been amazing to me, even though with fusion over the past 15 years we have seen similar outbursts. . . . Still, it is hard to explain the animus in the press.

Srinivasan: I just have no words for it. I had read a little in India, but I never realized the intensity of it. It goes to the extent of accusing some people of spiking results. . . . It's more than a coincidence, an accident. Somebody up there is trying to make it die.

EIR: Somebody is trying to stop people from doing this kind of research. What was most exciting to me about the work in India is the feeling there that if this worked, it would be wonderful, because we could relatively inexpensively harness this kind of reaction.

Srinivasan: Right now we can only say that the physics seems to have violated a lot of our known view of physics, and to that extent it has opened up a new chapter. So, if not anything else, it is going to widen our perspective on physics. So out of that, I am sure some good will come.

EIR: It is hard to imagine that anyone, before this new chapter is opened, would rip up the book! . . . The dishonesty involved is very upsetting.

Srinivasan: Somebody is moving very fast. It is absolutely amazing. . . .

EIR: Are there any people at BARC working on applications and scale-up?

Srinivasan: No. It is premature. I don't think we should make that mistake. We must understand the physics first, then automatically you will get a number of ideas. The goal at BARC is, let's first understand what is going on.

EIR: In India is there any reaction against cold fusion?

Srinivasan: Even in our own center, where we have some very distinguished physicists, it usually turns out that the more distinguished the physicist, the more difficult he finds it to accept. I would say, except our chairman, Dr. Iyengar, who has from the beginning stuck his neck out, and taken a gamble even against his reputation to go ahead and firmly support it.

EIR: Well, you are also distinguished, and you have an open mind.

Srinivasan: This is true, I think, I probably have. And I believe it because we have gotten the results personally. I have no way to explain the variety of experiments in which we have been getting results—gas-phase loading, palladium, titanium, plasma, different electrolytic cells—so I had to be convinced of it. There was no other way, because I have personally participated in the measurements. . . .

EIR: The people who are doing this research have the kind of spirit that's necessary in science. It gives a very bad impression to young people in particular, of what science is, when they see scientists saying that something breaks the rules, and therefore it could not be possible. Because that is not the way invention has taken place. I hope that by publishing some of this material that we can begin to turn the situation around here in the United States. . . .

Srinivasan: That's right. When new phenomena are discovered that we cannot understand, we have to accept them and find an explanation.

One remark made at the Utah meeting was very apt: Earthquakes are entirely unpredictable, but they are entirely believable! I thought that was very apt. Cold fusion is the same. It is *very* unpredictable, but just because a thing is unpredictable, you cannot say it is unbelievable.

Today, our scientific understanding is more or less at that point. We cannot reproduce it as of today, but it surely has happened. I think there is a list of 20 countries today which have got good results. You just cannot turn your back on it anymore.

EIR: And yet, I have in front of me the *New York Times* article from today. . . . The lead of the article is, "For believers, the central mystery surrounding cold fusion is what is happening in experiments that have been frustratingly erratic. For skeptics, it is why the field does not die." Then it says, "In the last year, hundreds of scientists have failed to duplicate. . . ."

Srinivasan: It is not the hundreds of scientists who have failed, but the 21 groups that have succeeded that matters. . . . In India at least we are happily shielded from all this, although there are some people who will quote *Nature*. *Nature* publishes the negative results, and we have our own people who will put it up on the notice board, and say "What does Srinivasan mean, when *Nature* says it is negative?" That kind of thing. It is interesting in it's own way!