Transcript of Interview with Rusi Taleyarkhan, May 15, 2007

Steven B. Krivit: When and where were you born?

Rusi Taleyarkhan: In a small village called in Dohad in western India

SK: When did you move to the U.S.?

RT: That was in the Fall of 1977.

SK: Was that when you started your course of study at Rensselaer Polytechnic Institute?

RT: Yes, I was offered a scholarship and admission.

SK: Was that a full or partial scholarship?

RT: I think it was close to full. I did get some scholarship money from India also. I was what they call a Tata scholar, it is a nationwide competition for scholarships for students aspiring to go abroad. Definitely I could not have afforded to come to study in America on my own.

SK: What is Tata?

RT: It's the number one industrial house in India. The House of Tata is a large group of companies, as recognized in India as, let's say Bill Gates is in the U.S.

SK: When did you become an American citizen?

RT: In 1980 I got my Master's in Business Administration and in 1982 finished my Ph.D. Dick Lahey was my thesis mentor, he offered me a position as a research associate, or as we call, a post-doc, and RPI sponsored me for my green card. I got that in 1983 and in 1984 I was offered a position in Westinghouse Electric.

You have to wait until about five years after you get your green card, and in 1988 I was offered the chance to be a U.S. citizen and I grabbed it, and have been since.

SK: How did you feel about giving up your Indian citizenship?

RT: At first it seemed difficult, but ultimately -- ever since I was growing up I had respect for Americans, Abe Lincoln, George Washington, those kind of heroes, along with people like Mahatma Gandhi and [inaudible] of India also. I believed that if I was going to raise a family and earn a living here, then that was something I had to show as part of my allegiance to this country and I became a citizen and I've not regretted that one bit. I have a fond place in my heart for India as well, I will never forget that.

SK: When did you first get interested in science and physics?

RT: In high school I was quite interested in how things worked. I read about the giants of the field like Galileo and Albert Einstein. That fired me up so I decided to get into the field of science and engineering. I was fortunate enough to be admitted into the Indian Institute of Technology, that's like the Indian equivalent of M.I.T. and became captivated with nuclear science and engineering.

My senior project over there was to build a 1/8 scale facility for looking at the thermo hydraulic aspects of India's fast breeder reactor.

SK: I read something about you on the Internet regarding weapons research, that said came out of your work with bubble fusion, can you tell me about that?

RT: The bubble fusion reactor research came about significantly after the variable velocity bullet research.

The variable velocity bullet research was based on vapor explosion technology which has proven quite a problem for the worldwide metals casting industry as well as for nuclear "fission" reactors. The infamous Chernobyl reactor accident is an example; other similar events have happened in research/test nuclear reactors in the US also due to which the nuclear safety of any water-cooled reactor has to consider such events in terms of determining overall risk during beyond-design basis accidents. I had spent many years researching this topic for nuclear reactor safety since this sort of event can be devastating in terms of fission product release to the environment and can dramatically alter the risk profile. The fire-power of metal-water reactions can be very significantly greater than that from the best of high-explosives. Having done this sort of work resulted in understandings on how to intensify the explosive effects with "control" and also on how to prevent them - thereby, affecting both, the generation of a Star-Trek like weapon system ["Set phasers for stun]" for on-demand force projection, to aiding the metals industries where such explosions have happened quite routinely and can/do cause widespread facility damage along with injuries, etc. The variable velocity bullet research started out as a non-lethal weapon research program in the early 1990s but then later on has become an item I can not talk about further due to security considerations.

Furthermore, I can neither confirm nor deny the existence or absence of any present research nor application of the vapor-explosion based variable velocity bullet or other related extensions.

SK: When did your interest in acoustic inertial confinement fusion begin?

RT: It started off on a completely different project dealing with the nations' largest science project, the spallation neutron source project at Oak Ridge. It involves pumping high energy protons into a bucket of mercury. That system had a lot of cavitation problems and I was invited to help solve those problems and that involved forming bubbles from nuclear particles and when I saw the results of the implosions, how much energy could be focused into them, that's when I got interested into the acoustic aspects. That was about 1991.

Then came, around 1994, a conference in Saratoga Springs in nuclear reactor thermo hydraulics where two keynote lectures were given by my mentor and thesis adviser Dick Lahey and colleague Robert Nigmatulin. They were talking about the process of sonoluminescence and how one could use bubbles to create fusion in a room-temperature environment.

I then thought about how to combine the research being done for the spallation neutron source project, where we were searching for ways to get rid of cavitation damage, perhaps from nuclear fusion.

I got interested in sonoluminescence and wrote my proposals to ORNL and they funded one of the proposals and I got started, came up with some enhancements and then came an opportunity to make a proposal to DARPA. I wrote up an idea and the senior manager at ORNL came along with me and opened some doors.

Something must have clicked and he got interested in the general idea of sonofusion and decided to fund the work with myself, Lahey, Nigmatulin and Colin West on the original proposal. He also had Seth Putterman, Ken Suslick and Larry Crum at the University of Washington involved also, and Bob Apfel, good friend from Yale got involved too, he was my strongest supporter.

SK: About when was this?

RT: It started out, I believe in 1999. Then after about three years of hard work, we came up with our paper which got published in Science in 2002.

SK: Going back a bit, to the 1991-1994 time period, did you or your work have anything to do with Putterman and his work at the time?

RT: I had never heard about Seth Putterman until the DARPA work started in 1998-1999, that's when I first met him and found out what he was trying to do. He was fortunate to have gotten a patent in 1999. Gaitan was the first to come up with the SBSL that he did for his Ph.D. thesis in 1991 under the direction of Larry Crum.

SK: Was Gaitan the first to come up with the idea of sonofusion?

RT: Yes, for the SBSL, for being able to grab a single bubble in a sound field and make it oscillate, and each time it oscillates it implodes and creates flashes of light but the field of SL started out in the 1930s in Europe by Frenzel and Schultes. Colin West, one of my co-authors, was one of the first in the world to prove that a flash of light comes out when the bubble has imploded.

SK: How would you characterize Gaitan's contribution?

RT: Bubbles are extremely difficult to control, they break up, the coalesce, they have a mind of their own. Despite 1,000 years of working with bubbles in a two-phase flow - we have it all the time - any time you crack your knuckles you produce a bubble inside your blood that collapses and gives you the cracking sound. And if you keep doing that often enough you'll cause damage because of the handling load on your cartilage. He found a way to levitate a bubble and make it grow and collapse with some degree of control, that was the first time somebody learned how to control a bubble.

SK: And what aspect did you contribute to sonofusion?

RT: What we did was to amplify that approach and to find a way to control it, to whatever extent that we could. With SBSL it was like having one soldier walking on a bridge versus, ours, mutlibubble, an army walking across. So that's what we did, to find a way to control a group of bubbles imploding together in a coherent fashion that amplified the process much more than you could do with a single bubble. We've tried SBSL experiments and we know that that is not going to give you fusion conditions, at least what people think should be fusion conditions.

We've got mathematical models that we've developed - we had an idea before we even started this process of what needed to be done and the other thing that we did was to create very large bubbles. There is the bubble that you have in the Putterman/Gaitan type of approach, those bubbles go to about 100 microns starting with about 10 microns, a factor of 10 increase in size before collapse.

What we did was to go about increasing the size not by a factor of 10 but by 100,000. We start from the nanoscale and go up to the multi-millimeter scale -- you can actually see the bubbles grow up in front of your eyes, and then implode into nothing. That was an innovation that we brought.

SK: From what you know, is the approach that Putterman uses the SBSL method?

RT: Yes, that was in his proposal and what was in his patent. He visited Gaitan's lab from what he had told us and learned how to do the SBSL, which now a lot of people know how to do, but it's not that terribly difficult. But he learned that and instead of just using ordinary air that had been used in Gaitan's apparatus, he wrote in his patent using a bubble of deuterium gas. His theory was that with that approach, you could create high temperatures and pressures and then cause fusion conditions to take place. It was an extension of Gaitan's SBSL experimental approach.

SK: Do you know if his approach has yet to achieve the conditions required for fusion?

RT: No, everything that we have seen so far and even our simulations indicate that that approach will not give rise to the required conditions for fusion.

SK: How about Suslick, what do you know about the nature of his related research?

RT: Same as Putterman, those two have been working together on this approach for a long time.

SK: What approach does Ross Tessien work on, SBSL or MBSL?

RT: He works on a totally different approach. He takes a spherical resonator, a steel shell and he's got piezo-electric drivers symmetrically located around the outside surface and he bombards the liquid inside with sound waves. In a way the method is similar to the old days of the 1930s with the difference that instead of using 10 watts of power into the chamber that Frenzel and Shultes were doing, Tessien is using kilowatts of power.

SK: I want switch gears for a moment. Have Naranjo or Putterman responded to your rebuttal which shows that their speculation about Cf-252 spiking doesn't hold water?

RT: Not that I am aware of. I have told them that if they partake in these kinds of actions -- you know -- I'm sick and tired of that group, quite frankly for doing all the things that they have done. I will follow the time-honored tradition of communicating with and responding to editors of respected journals.

Nobody from PRL has contacted me with regards to a rebuttal to my rebuttal from UCLA. The smoking gun evidence that we have currently is the fact that we opened up our labs and the group from Texas came over and separately Bill Bugg from Stanford came over and insured that there was no Cf-252 or other contamination and they reproduced the results that we published. It puts Naranjo's claim completely out of the picture, it is just utter nonsense.

[Taleyarkhan recalls the day he learned of the spiking allegation during the March 1, 2006 DARPA review in his laboratory.]

The lab was swarming with people. There were all these guys from DARPA, ONR, UCLA, Ken Suslick and his students, my own students, postdoc Jaseon Cho. I mean, how could this thing ever be done in front of all of them? It's so ridiculous. Quite a bit of the time we were in a meeting room talking and talking, and then of course, there's Putterman bringing up his allegations of Cf contamination, and within two or three hours I get a call from Eugenie Reich. It all seems to have been pre-planned.

SK: Is there any simple way you could summarize your rebuttal to Naranjo?

RT: Okay here's what we did. If it was californium, you have to know that californium emits neutrons but also gamma. Bubble fusion does not generally emit gammas, it emits neutrons. Any gammas that are emitted are produced indirectly when the neutron hits atoms of iron and hydrogen, etc., and that's a very tiny fraction of the neutron population. If you have 100 neutrons, you'll likely get less than one gamma coming off from a bubble fusion experiment. If you have Cf on the other hand, for every neutron that comes off there's about 10 gammas coming off so it is an entirely opposite picture. So the data that we have published was showing that the gammas that you get are a tiny fraction of the total neutron population.

When we did the rebuttal, we actually conducted the experiment with a Cf source using the same detector, same geometry and everything and lo and behold, the gamma spectrum is huge compared to what we had in our published bubble fusion work. Even the shape of the gamma spectrum does not match what Naranjo and Putterman speculated. These guys keep on harping that the neutron spectrum has some similarity to bubble fusion and that may be very convenient because for a Cf source the average energy is 2MeV and for fusion it is 2.5MeV.

There is going to be some similarities because they have energies close to each other and that's not surprising, but the gamma spectrum is completely off. You're going to get almost no gammas in bubble fusion, with Cf you're going to get enormous quantities of gamma that you measure. So they started pointing fingers at us very conveniently forgetting, I would say naively forgetting, the relationship between neutrons and gamma. They completely ignored the gamma spectrum data and that was a critical shortcoming in their argument and the smoking gun evidence that this could not Cf.