The *New Energy Times* Special Report on Bubble Fusion/Sonofusion

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To discuss the bubble fusion controversy with those directly involved is to see into the messy way science sometimes functions, with personality clashes, disputes over funding and prestige, and journals struggling to satisfy their readers' desire for exciting new information and ideas.

- Journalist Erico Guizzo, *IEEE Spectrum*, May 6, 2006

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Preface

Science writer Kenneth Chang of *The New York Times* broke the story in the middle of the night as March 7 turned to March 8, 2006. The headline was alarming: "University to Investigate Fusion Study."[1]

Chang had received advance notice of a series of stories by science writer Eugenie Samuel Reich for *Nature,* but Chang had managed to draft a cursory sketch of the situation and be the first to break the news, beating *Nature* by a few hours.[2]

An hour after the *Nature* articles published that morning, the "investigation" story took on an ominous tone. Maggie Fox of Reuters was the first to label it an outright fraud investigation. Fox had trouble with the subject matter, evidenced by her lack of distinction among bubble/sonofusion, piezo-crystal fusion and cold fusion, and made several other minor errors in her story.[3]

Thus began the story of Indian-born nuclear engineer Rusi Taleyarkhan, a tenured professor at Purdue University who dreamed of a less-expensive way to make cleaner nuclear energy.

Researchers who are working on the frontiers of bubble/sonofusion -- if you accept it as real -- are struggling to understand an unknown phenomenon. Reporters have an even greater challenge, because they almost certainly learned nothing about bubble/sonofusion in school.

As Erico Guizzo noted in one of his investigations on this subject, "not surprisingly, those closest to the controversy have preferred not to go on record with details about its human dimensions."

I have had the same experience, because several key figures in this story have failed to respond to my requests for information.

New Energy Times has afforded Taleyarkhan an opportunity to air his views in this report. He has made repeated attempts to have his views considered and reported by the dominant media covering these events, *Nature* and *The New York Times*, as well as by a congressional committee. All three have significantly underreported or misreported facts and views that would represent Taleyarkhan's side fairly in the court of public opinion.

This report answers the following questions:

- What is bubble/sonofusion?
- What is sonoluminescence?
- What is the relationship between bubble/sonofusion and the global energy problem?
- What valid evidence, if any, exists to support the claims of bubble/sonofusion?
- Who is Rusi Taleyarkhan, and why has he been accused of science fraud?
- Is there any evidence of science fraud, and, if so, what are the details?
- What is the involvement and responsibility of the other key bubble/sonofusion researchers, Kenneth Suslick and Seth Putterman?
- What is the involvement and responsibility of journalists Eugenie Samuel Reich (*Nature*) and Kenneth Chang (*The New York Times*) in this story?
- What is the involvement and responsibility of the Purdue University administration?
- What is the involvement and responsibility of Purdue University professor Lefteri Tsoukalas?
- What is the involvement and responsibility of Defense Advanced Research Projects Agency program manager William Coblenz?
- Were errors of judgment made, and, if so, by whom, when and how?
- What journalism ethics and practices are illuminated as a result of these events?
- What are the significant differences between authoritative science media (peer-reviewed journals) and nonauthoritative media (science news), and why is the distinction important?
- How scientific is science, as seen by the facts revealed in this investigative report?

The report is intended to be a reference work for program managers, policymakers, scientists, scholars, historians and journalists. It is not intended for the lay audience; it contains the bare minimum of synthesis and interpretation, and is anything but brief and succinct.

Readers should be forewarned: This story is far from over, and the enclosed text provides no final outcome. The final chapter in this saga has yet to occur. A Nobel Prize may someday be awarded to a bubble/sonofusion researcher.

-- Steven B. Krivit
July 10, 2007

Introduction

Science, as this account reveals, can sometimes be anything but scientific. This story reveals the surprisingly difficult task faced by members of the public, the government and the science press when evaluating controversial, high-stakes science research.

Society's general understanding is that scientists are objective, dispassionate, dedicated to serving the greater good of society, and held to a high standard of ethics.

This story includes such understandings as assumptions rather than certainties. It is instructive; it teaches all who avail themselves of the lessons within how science works, how it doesn't and how it differs from scientific propaganda.

CHAPTER ONE

The Science and the History

Fraud, *noun*
- a) intentional perversion of truth in order to induce another to part with something of value or to surrender a legal right; b) an act of deceiving or misrepresenting
(Merriam-Webster)

By all accounts, science fraud is ugly business. When it occurs, the disconcerting repercussions are felt worldwide. Few institutions exist today that are as pervasive as that of science; it transcends nations, languages and cultures. Science has its own set of disciplines, ethics and rules of engagement that are, by and large, universally accepted.

Deserved or not, the most prominent scientists are often regarded with reverence and are assumed to act ethically and with integrity.

Disturbances to this fundamental aspect of modern civilization are not taken lightly. Society relies on scientists to interpret the world around us, to bring us new insights into nature and to guide us toward technologies that will advance society.

When the rules of science, written or unwritten, are broken, confidence in the scientific institution is threatened, and this invariably leads to unfortunate circumstances for all involved parties.

The latest alleged science fraud is the circumstances surrounding professor Rusi Taleyarkhan of Purdue University and his novel form of nuclear energy research.

Taleyarkhan was born in 1953 in Dohad, a small town in the Mumbai area of India. He was awarded a scholarship from the prestigious Tata organization in India. In 1977, he moved to the United States and obtained his master's degree in business and his doctorate in nuclear engineering from Rensselaer Polytechnic Institute. He worked at several commercial laboratories, then moved on to work at Oak Ridge National Laboratory before going to Purdue.

He has held several positions of leadership in the American Nuclear Society, has been a consultant to the International Atomic Energy Agency and the Japan Atomic Energy Research Institute, is a founding director and member of the national Acoustic Fusion Technology Consortium, and is a founding member and executive of Purdue University's Energy Center.

Taleyarkhan has a high-level security clearance from the Department of Energy and has worked with technologies for national security and defense, nuclear reactor thermal-hydraulics, and safety technology.

Taleyarkhan's two main colleagues in his bubble fusion research are Richard T. Lahey Jr., a professor at Rensselaer Polytechnical Institute and former dean of engineering and past chairman of the Department of Nuclear Engineering and Science at Rensselaer, and Robert I. Nigmatulin, a member of the Presidium of the Russian Academy of Sciences and past president of the Ufa-Bashkortostan branch of the Russian Academy of Sciences.

Surprisingly, only Taleyarkhan seems to have been the target of the fraud allegations and investigations; his colleagues haven't been mentioned in the press.

Taleyarkhan's plight and circumstances have come to the forefront because of his claim of a novel method of creating a nuclear reaction in a relatively simple table-top experiment. It is not simple from a technical perspective, but it is simple when compared to particle accelerators and magnetic confinement reactors far greater in size, cost and complexity that also are trying to harness practical fusion energy.

The popular term for his novel method is called bubble fusion or sonofusion. It is a table-top experiment that bombards a flask of deuterated acetone with sound waves, which in turn causes bubbles to rapidly expand and, on collapse, theoretically trigger energetic fusion reactions. The technical term for the research is acoustic inertial confined fusion.

Bubble Fusion - Technical Description

Bubble/sonofusion is a table-top experiment that exposes a flask of deuterated organic liquid mixture to intense sound waves that, in turn, explosively create a cloud of microscopic bubbles that are nucleated on the nanoscale when the liquid is in tension (that is, molecules are stretched such that the pressure field is under subvacuum conditions) by nuclear particles such as neutrons and/or alpha recoils; the acoustic field causes them to rapidly expand by factors of over 100,000 and, on collapse, trigger energetic fusion reactions.

Fusion and Other Anomalous Nuclear Reactions

Fusion is a nuclear process that powers the sun and the stars. In simple terms, when two atomic nuclei come close enough, overcoming their natural electromagnetic repulsion to each other, another force takes over and brings them very close together with a strong force. Once they come together, new elements are created, and energy is released.

Conventional thermonuclear fusion, using high-temperature magnetically confined chambers or, alternatively, arrays of the world's largest lasers, is well-understood scientifically. It has been used successfully in weapons, giving an uncontrolled rapid release of energy. It has been used in experimental reactors designed to provide controlled release. So far, however, these reactions consume more energy than they release and are therefore not an actual source of energy. Controlled thermonuclear fusion for commercial application remains an engineering mystery unsolved for 50 years. It is a wicked problem and is not expected to provide any useful energy for many decades.

Bubble/sonofusion is considered by Taleyarkhan and other scientists to be a possible future alternative to fossil fuels, but like conventional forms of fusion research, it is far from ready as a practical application. Bubble/sonofusion is considered a table-top version of thermonuclear fusion by its proponents, though not necessarily by other researchers in conventional fusion.

The field of low energy nuclear reactions, known historically as "cold fusion," is slowly gaining recognition after 18 years as another long-shot potential energy source, thanks to a committed worldwide group of researchers. Unlike thermonuclear fusion, low energy nuclear reactions faces an immediate bottleneck not because of engineering but because of poorly understood science.

The term "fusion," applied to both "cold fusion" and to bubble/sonofusion, may be unjustified, despite the evidence that nuclear reactions are taking place in these experiments.

Conventional fusion, the only kind that is well-understood and scientifically proven, produces two dominant byproducts, tritium and neutrons, in a specific ratio and at specific energies.

Any reaction that produces either tritium or neutrons at statistically significant levels is most likely a real nuclear reaction, but without the expected ratio and energies of conventional fusion reactions, a more cautious approach is to describe such claims as anomalous nuclear reactions.

Oak Ridge researchers Dan Shapira and Michael Saltmarsh noticed this anomaly when they attempted bubble/sonofusion: "Any neutron emission that might occur is at least four orders of magnitude smaller than that necessary to explain the tritium production reported in Taleyarkhan et al. as being due to deuterium-deuterium fusion."[1]

Background on Bubbles

The term sonofusion is derived from the contraction of sonoluminescence and fusion. The fusion aspect of sonofusion is highly debated, for the following reason. No bubble/sonofusion experiment has shown the expected match of the nuclear ash, the byproducts known to come from fusion reactions.

Yet the production of either tritium or neutrons at levels significantly above background is by itself unequivocal evidence of some, possibly previously unknown, type of nuclear reaction.

Ross Tessien, president of Impulse Devices Inc. in Grass Valley, California, a company working to take acoustic fusion to market, provided a detailed explanation: "Once you have confirmed that neutrons are screaming out of the device, it is proof positive that you are driving some sort of nuclear reaction. The first problem, however, is that there are myriad ways that a neutron detector will give false signals; in short, a neutron signal does not a neutron make. If, however, it has been confirmed that the signal is real and neutrons are in fact being produced, then who cares if it is a deuterium-deuterium fusion reaction or any other nuclear reaction – it is nuclear.

"Chances are, but not guaranteed, that if the neutrons came from deuterium in heavy water, the reaction path will be exothermic and produce energy at atomic scales. So, from the standpoint of proving that you are driving some sort of nuclear reactions in some device, any neutrons are good neutrons, because they don't exist in nature. If you are certain that you have them flying out of the machine, then you have proven that the machine works to excite matter to nuclear interaction intensities."

Supporters of bubble/sonofusion hope that it someday may generate energy at a fraction of the cost of the "big-iron" thermonuclear projects. For this reason, the prospect of bubble/sonofusion has been the subject of much interest and angst in recent years.

Sonoluminescence derives its name from the effect of transforming sound waves into light. Sonoluminescence is not fully understood, either. However, by itself, the phenomenon is not so controversial.

Drilling down further, the effect of sonoluminescence is created by a phenomenon known as cavitation, the rapid expansion and collapse of bubbles within a liquid.

Cavitation is generally, but not completely, understood. It was first observed nearly a century ago in 1917 by the British navy. The navy observed damage to a ship's propellers coincident with the occurrence of tiny bubbles in the water. Physicist Lord Raleigh was the first to begin investigations.

Encyclopedia Britannica defines cavitation as "the formation of vapor bubbles within a liquid at low-pressure regions that occur in places where the liquid has been accelerated to high velocities, as in the operation of centrifugal pumps, water turbines, and marine propellers. Cavitation is undesirable because it produces extensive erosion of the rotating blades."

In the 1930s, two Germans, H. Frenzel and H. Schultes, made the connection between cavitation and sonoluminescence, using acoustic waves to trigger the cavitation.

Taleyarkhan explains: "Frenzel and Schultes were conducting acoustic cavitation research, and they found that, even in a darkroom, their photographic plates were being fogged up. At first, they didn't quite understand until they started looking at the chamber and there were blue flashes of light coming out, and this was the light that was exposing the photographic plates, so they decided to call it sonoluminescence, sound to light." [2]

In the 1960s, Colin West, working as a postdoctoral student at the Atomic Energy Research Establishment in Harwell, U.K., was given the task of building an inexpensive neutron detector. He came up with a clever idea, using an organic fluid with a low vapor pressure which he put in an oscillating acoustic field.[3]

In 1982, Hugh Flynn was issued a U.S. patent (No. 4,333,796) for a "Method of Generating Energy by Acoustically Induced Cavitation Fusion and Reactor Therefor." Flynn was the first to suggest that this method might create the required conditions for fusion. Flynn did not create a proven fusion reaction at the time, nor has anyone. The use of the word "fusion" in relation to sonofusion has been symbolic of the intentions of the field, not the accomplishments.

In 1990, Felipe Gaitan reported in the published literature that a single gas bubble, acoustically levitated in a liquid, could produce sonoluminescence emissions in each acoustic cycle, a phenomenon which became known as single-bubble sonoluminescence.[4]

Gaitan's innovation improved on the earlier sonoluminescence work, which had been done with acoustic horns that produced a lot of random gassy bubbles without any control.

Now the chief scientist for Impulse Devices Inc., Gaitan developed this method as a graduate student at the University of Mississippi under the direction of Lawrence Crum, who is now at the University of Washington.

According to the Impulse Devices Inc. Web site,[5] Gaitan's "discovery and research in ... sonoluminescence has led a new generation of physicists to begin contemplating the potential for using acoustically driven cavitation to produce fusion reactions."

Crum mentions in a 2003 paper that single-bubble sonoluminescence "attracted the attention of many scientists"; however, research in the area has been limited to only a "handful of individuals." Crum writes that these researchers have "rapidly gained enough insight into the topic to have it declared 'understood'" and that they have found that "maximum temperatures were restricted to a few tens of thousands, if even that high." [6]

Such a ceiling on the temperature, if true, implies that the conditions for fusion would be impossible to achieve using this method. However, a handful of serious researchers, undiscouraged by the theoretical calculations of others, are pursuing the goal of a novel energy source through bubble/sonofusion. One of these researchers is Rusi Taleyarkhan.

Readers can learn more about sonoluminescence in the articles "Extracting Light From Water: Sonoluminescence," by Ryan Gutenkunst and "Sonoluminescence" by Lawrence A. Crum and Ronald A. Roy. [7]

"My discovery of something interesting in bubble fusion started," Taleyarkhan wrote, "when I decided to switch from working only with hydrogen-bearing liquids to deuterated forms. This occurred in November 2000. Per my recollection, the first signs of neutron activity correlated with sonoluminescence were observed in January 2001."

Bubble/sonofusion hit big in 2002 when Taleyarkhan, then at Oak Ridge National Laboratory, and co-authors Colin West, JaSeon Cho, Richard Lahey, Robert Nigmatulin and Robert Block published a paper in *Science* claiming to have achieved a successful fusion reaction, success which had previously eluded all others in the bubble/sonofusion field. [8]

As experimental evidence, they claimed neutron production as well as tritium evolution at levels significantly above background. They also claimed theoretical calculations suggesting that their method provided the required high temperatures needed for thermonuclear fusion reactions.

Taleyarkhan says that their work was innovative in two respects. First, they found "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." As a result, they were able "to grow the bubbles much larger than other researchers had previously done" in the sonofusion field.

"We increased the size not by a factor of 10 but by 100,000," Taleyarkhan said. "We start from the nanoscale and go up to the multimillimeter scale – you can actually see the bubbles grow up in front of your eyes and then implode into nothing."

CHAPTER TWO

Suslick and the Early Backlash

Lawrence Crum writes that the Oak Ridge group's claim was greeted with considerable skepticism by some people and great delight by others.[1]

"I thought, doggone!" Crum told Charles Seife of *Science*, "I'm depressed I hadn't done that experiment." [2]

Even before the paper published, an inferno had developed at Oak Ridge, according to Seife. Lee Riedinger, the lab's deputy director for science and technology, asked two other physicists at Oak Ridge, Dan Shapira and Michael Saltmarsh, to check the Taleyarkhan group's work with a more-sensitive neutron detector. Shapira and Saltmarsh decided that the Taleyarkhan group's results had been an "illusion." [3]

Seife wrote that "Don Kennedy, the editor-in-chief of *Science*, says that Oak Ridge officials tried to withdraw their permission to publish the [Taleyarkhan] paper after they had granted permission. 'There was certainly pressure from Oak Ridge to delay, if not to kill, the paper,' says Kennedy. 'I'm annoyed at the intervention, and I'm annoyed at the assumptions that nonauthors had the authority to tell us we couldn't publish the paper.'"

Kennedy said that late in February 2002, William Happer, a physicist with Princeton University, and Richard Garwin, with the IBM Thomas J. Watson laboratory, joined the naysayers and discouraged him from publishing the paper. Seife reports that Happer and Garwin were merely "encouraging *Science* to publish the Shapira and Saltmarsh data as well, or at least not to hype the paper."

Kennedy wrote more-interesting details on this ordeal in his March 8, 2002, editorial. [4]

After publication, Robert H. Austin, a professor at Princeton University, wrote Kennedy a strong letter, suggesting that the Taleyarkhan group's results were wrong – simply because the other group at Oak Ridge supposedly had replicated the experiment yet failed to replicate the results. And for this reason, Austin went so far as to accuse *Science* of "irresponsible journalism." [5]

And this was just the beginning of the bubble drama.

On July 25, 2002, Yuri Didenko and Kenneth S. Suslick, professors of chemistry at the University of Illinois at Urbana-Champaign, published a paper in *Nature* discussing their perceived limitations of Taleyarkhan's version of bubble/sonofusion.

Suslick performed an experiment and made calculations that strongly suggested that it was unlikely that Taleyarkhan et al. had achieved or ever would achieve "the extraordinary conditions necessary to initiate nuclear fusion."[6]

There are a few troubling things about the Suslick paper, but first, a little background.

Sonoluminescence work performed in the 1930s was called multibubble sonoluminescence. This was a different form of multibubble sonoluminescence than Taleyarkhan uses. Today, the term distinguishes between Taleyarkhan's method, multibubble sonoluminescence, and the work of Suslick and UCLA physicist Seth Putterman, which is based on the single-bubble sonoluminescence developed by Gaitan.

The work described in the Suslick paper is not Taleyarkhan's multibubble sonoluminescence work; it is Suslick's single-bubble sonoluminescence work, a different animal. Yet toward the end, the reader is presented with pessimistic comments that appear to discredit the Taleyarkhan work.

The paper fails to address directly any errors of protocol or analysis regarding the key claims of the Taleyarkhan paper, their neutron and tritium observations. The paper merely alludes to a theoretical argument with the Taleyarkhan work and does not even provide a rock-solid argument, because Suslick is comparing apples to oranges, even using a different liquid in the acoustic chamber.

In the introduction to his paper, Suslick concedes a significant deficiency in its utility for critiquing Taleyarkhan's work: "It is extremely difficult to perform a quantitative analysis of the chemistry associated with multibubble cavitation: unknown parameters include the number of active bubbles, the acoustic pressure acting on each bubble and the bubble size distribution."

The paper illustrates the theoretical limitations of the phenomena through calculations with single-bubble sonoluminescence despite the fact that Taleyarkhan's work uses multibubble sonoluminescence.

The paper concludes, "The present results show that endothermic sonochemical reactions within a collapsing bubble are a major limitation on the conditions produced during cavitation."

The casual reader would infer from this broad statement that the Suslick group's work applies to all sono-cavitation work and that the group has explicitly found a flaw in the Taleyarkhan group's work.

Taleyarkhan responded to the attempted comparison of his multibubble sonoluminescence to Suslick's single-bubble sonoluminescence in an e-mail: "This subject has been conclusively settled via several debates over many years and culminating with the 2005 publication of the theoretical foundation for multibubble versus single-bubble sonoluminescence, published in *Physics of Fluids*.[7]

"I played a minor supportive role in this theoretical effort which was led by Nigmatulin in close collaboration with simultaneous efforts by Lahey. All aspects of criticisms related to endothermic effects by Suslick et al. (2002) were included and accounted for. Our theoretical foundation well predicts overall fusion output for our group's experimental approach but importantly also sheds light on fundamental limitations of the single-bubble sonoluminescence approach. We now understand why the Suslick/Putterman single-bubble sonoluminescence approach encompasses physical limitations and will not provide measurable fusion signals."

The Suslick paper also reveals several interesting fact about the timing of the writing of this paper. Although the paper was published on July 25, it was, as is typical, in the pipeline much earlier. But how much earlier? The Suslick paper, a response to the Taleyarkhan paper, was submitted to *Nature* on March 1, one week *before* the Taleyarkhan paper published.

The Taleyarkhan paper is referenced in the Suslick paper, reference No. 23, including the precise page number from *Science*. Reference No. 24 in the Suslick paper refers to a news article from *Physics Today* dated April 2, one month *after* the Suslick paper was submitted.

Suslick had been one of the reviewers for the 2002 Taleyarkhan paper. Because Suslick has refused to speak with *New Energy Times*, we can only assume that he used his privilege of having advance access to Taleyarkhan's manuscript to prepare his own paper before Taleyarkhan's paper published and that Suslick edited the footnotes in his paper after he submitted his manuscript.

In the context of the timing of Suslick's submission relative to the as-yet-unpublished Taleyarkhan paper, readers may wonder whether Suslick attempted the Taleyarkhan experiment himself -- based on the Taleyarkhan paper -- before submitting his critical manuscript. This would seem the prudent and honorable thing to do before publishing a serious critique of a colleague's work.

Suslick is no amateur when it comes to sonoluminescence and chemistry. Sonochemistry, the use of ultrasound in chemistry, is his specialty.

In 1999, he was recognized by *Nature* for coming up with "an ingenious method to take the temperatures of cavitating bubbles," in a paper also published in *Nature*. [8]

[Do not confuse the title of Suslick's paper with the mult-bubble sonoluminescence of Taleyarkhan. The older form of acoustic cavitation was multibubble, but using an acoustic horn, which was a different pedigree from the multibubble sonoluminescence reported by Taleyarkhan in 2002.]

Suslick (who quotes P.T. Barnum on his Web site: "Publicity is good. Good publicity is better.") got the attention of at least a dozen major media outlets within days, as a result of his paper.

A common theme ran throughout. Here are some of the headlines and key excerpts:

> ***Nature*: Inside a Micro-Reactor**
> "So 'bubble fusion' — an energy-generating fusion reaction in the high-density, high-temperature interior of the collapsing bubble — is most unlikely."[9]
>
> ***Science:* Chemistry Casts Doubt on Bubble Reactions**
> "A controversial claim that scientists had detected signs of fusion in a rapidly collapsing bubble may have further imploded this week. A new experiment that measures the energy budget of a collapsing bubble for the first time indicates that so-called bubble fusion is highly unlikely to occur." [10]
>
> ***BBC News*: Fusion experiment disappoints**
> "Some researchers [Taleyarkhan et al.] have suggested that conditions within a cavitating bubble might be hot enough and have high enough pressure to generate nuclear fusion."
>
> "'We've shown that chemistry occurs within a collapsing bubble and that it limits the energy available during cavitation.'" [Suslick][11]
>
> ***New Scientist*: Bubble Bursts for Bench-Top Nuclear Fusion**
> "Suslick has already said he believes Taleyarkhan's lab was contaminated with tritium - the very thing used as evidence of fusion."[12]

The last quote is surprising and brings to light a common but unfortunate practice of including unscientific opinions in scientific discourse.

If Suslick truly thought that there was a problem with the tritium – a major component of the Taleyarkhan claim – he needed to report this to the scientific community through the formal scientific journals and peer-review process.

This is the beginning of what will be shown to be a collapse in the distinction between authoritative scientific publishing (journals) and nonauthoritative scientific publishing (news outlets).

If Suslick was following the formal rules of science, he would be expected to explain and publish in a journal exactly how the tritium might have been the result of contamination and why it failed to appear in controls. For a charge that implies such gross incompetence or fraud, he would have been expected to have very solid evidence before making such allegations.

Also, a judicious editor would apprise Taleyarkhan of the pending critique and offer him a chance to rebut, the fairest such opportunity being a side-by-side rebuttal in the same journal issue.

New Energy Times is unaware that Suslick pursued his suspicion abiding by these rules of science etiquette.

CHAPTER THREE

The BBC/UCLA Bubble Fusion Experiment

On March 22, 2004 a second paper from the Taleyarkhan group published, in yet another journal, *Physical Review E.*[1]

Tessien provided his perspective on the reaction to this second paper: "While the first paper was met with about the same welcome as a new cold fusion experiment, the second paper with dramatically improved data acquired by Taleyarkhan and his team, using improved instrumentation, turned the tide.

"While 90 percent of hot fusion scientists expected that bubble fusion was another cold fusion hoax after the 2002 paper, and 10 percent felt it should be looked into, after the 2004 paper the tides reversed, and 90 percent felt this should be looked into.

"All of the objections raised after the first paper published in 2002 in *Science* were answered in the March 2004 *Physical Review E* paper."

As Tessien mentions, not all scientists were pleased with the progress the Taleyarkhan group was making in the journals and in the science establishment.

Sometime before September 2004, Colin Murray, a producer for the BBC's *Horizon* television show, told Taleyarkhan that he was coming to the U.S. to film a documentary on Taleyarkhan's bubble/sonofusion.

Taleyarkhan had initially asked for a consultancy fee: "On many occasions such as this, and others where I give lectures and discussions on my research, an honorarium check is customary."

Murray wrote back, "The film is about your work on sonofusion and will bring it to a large audience, so it's not really a consultancy position I'm asking you to fulfill. Instead, it's the normal process of publicising your work as a scientist through interview and demonstration – and this isn't something the BBC would normally pay for ... I'm sure you understand that the BBC is a publicly funded organisation and our impartiality is the key to its reputation. Large payments for interviews could be seen to jeopardise that position of impartiality. But I do have a splendid bottle of single malt."

On this basis, less the bottle of single malt, Taleyarkhan agreed to spend time with Murray and his film crew. He thanked Murray for his reply and asked him to explain his position and expectations for the two-day visit.

Murray provided a detailed response of his stated objectives:

> 1. Film you with your pulse neutron generator.
> 2. Film some "set ups" – which means you doing some science for camera. This doesn't have to be real-time science – but can be. It just has to look like you're doing something.
> 3. Interview you in your office about your work: the whole history from how you got interested in sonoluminescence/fusion all the way through to the [March 2004] *Physical Review E* paper. I'm interested in some science detail but am also interested in the exciting story of the breakthrough, the reaction of the science world and so on.
> 4. Possibly a demo at the board about how bubbles act in sonofusion.
> 5. A demo of sonoluminescence, if this is easy for you to achieve.
> 6. On Saturday, I'd like to spend a couple of hours filming with you and talking with you away from the lab. This will allow me to cover anything I missed on Friday and also talk to you more as a person than as a professor.

Taleyarkhan recalls that everything went smoothly with Murray's visit on Sept. 17-18, 2004. Nothing seemed unusual or concerning, with one exception.

At the end of the second day, Taleyarkhan says that Murray asked whether he would be willing to work on a replication with Putterman and Suslick. Taleyarkhan declined.

"In good faith I had agreed to go ahead with the interview," Taleyarkhan wrote. "I found out their true intentions later when the BBC revealed that they intended to fund quick-turnaround work at UCLA. No funding was offered for Purdue even on telling the producer that we can't use university resources for technical work for free, but I did not pursue that further."

Taleyarkhan says he heard nothing further about a replication from Murray until he received the following e-mail on Jan. 13, 2005.

Subject: FW: report to referees
Date: Thu, 13 Jan 2005 19:51:54 -0000
From: "Colin Murray-GW" <colin.murray.01@bbc.co.uk>
To: "Rusi Taleyarkhan" <rusi@ecn.purdue.edu>

Hi Rusi,

I've enclosed the data that Seth Putterman produced during his attempt to replicate your results last year. You'll see that unfortunately he was unable to replicate your results. Recording simultaneously the sonoluminescence and the neutrons, with nanosecond resolution, Seth found no coincidences during two experimental runs, adding up to 4000 data points. The data has been reviewed in the U.K. by three experts on neutrons, instrumentation and sonoluminescence, respectively.

While Seth wasn't using your own cell, or a pulsed neutron source, he does seem to have reproduced your scientific parameters using your published recipe. I'm sure you'll want to digest the data before getting back to me, but I'd be grateful if you could give me a written response that I might be able to incorporate (at least in part, or in summary) in the film by next Friday (21 Jan.)

Also, I hope you can appreciate why I'm so keen to find out if other groups have replicated your work in order that I might incorporate this information in the film, as well. Please feel free to give me a call on cell phone (44) 7810 473 646 at any point or e-mail me a time when I can call you.

Yours sincerely,
colin
Colin Murray
Producer - Horizon
(44) 141 338 3087
(44) 7810 473 646

Taleyarkhan was traveling and did not return Murray's e-mail right away. Murray sent a second request on Jan. 18: "Please find enclosed a text document that explains the data that I sent you last week. I do urge you to come back to me with a statement, however brief, regarding your reaction to our attempts to replicate your results. I'd also urge you to let me know details of any positive replication attempts before its too late."

The next day, Taleyarkhan sent a formal response to Murray, dated Jan. 17, 2005.[2] Among other comments, Taleyarkhan wrote, "I have not been able to find time to study in depth the piece-meal information package received by me on Jan. 14., 2005, containing email and other such transmittals from UCLA to BBC and its consultants. However, as a courtesy to BBC's Colin Murray, I did take a brief look and offer the following interim feedback...

"The BBC-sponsored experiments at UCLA represents a scoping attempt and while, on the surface, may appear similar to the reported work of Taleyarkhan et al., it does NOT replicate the key aspects of experiments that have demonstrated acoustic inertial confinement fusion.

"The test cell, standing wave mode, rate, timing and evolution of clusters and transient drive, the energy spectrum of neutrons used to nucleate to name a few parameters all appear radically different from those used for the reported results by Taleyarkhan et al., in Science (2002) and in Phys. Rev. E (2004.)

"However, the UCLA experiments appear to represent a good demonstration for timing of sonoluminescence marker parameters and for pulse-shape discrimination -- something Prof. Putterman (UCLA) has proven adept at. The UCLA scoping experiments demonstrate yet another set of experimental parameters that may not lead to significant nuclear fusion during neutron-seeded acoustic cavitation and could be useful as a guide for other researchers in the field.

"For the record, I had cautioned BBC about the potential perils of sponsoring such an undertaking, however noble the motive.

"Finally, I neither confirm nor deny any data or results from related confirmatory work performed by various worldwide groups -- that have been shared with me in confidence, even if it means to take a loss of not taking advantage of positive publicity, etc. I firmly believe that scientific reviews (especially for unpublished information) should NOT rest in the domain of, nor be conducted by, the press. One needs to abide by the sacrosanct time-honored and tested traditions of anonymous peer review for publication before engaging in public debate."

Enclosed with the e-mail sent from Murray is what appears to be the complete data package sent by UCLA to the BBC referees. It reveals something interesting about the UCLA work. Every communication with regard to the lab work was prepared by Putterman's postdoctoral fellow, Brian Naranjo, not by Putterman.[3]

This is interesting for several reasons. One, all of the public credit seems to go to Putterman. Two, if a problem arises with this experiment, who will take the fall? Three, if Naranjo was the one doing all the hands-on work, it is unreasonable to expect that his experience in building and operating a bubble/sonofusion reactor was on par with that of Taleyarkhan.

The broadcast stated that Putterman claimed to replicate the Taleyarkhan experimental conditions – at least close enough.

The BBC show was called "Nuclear Fusion: An Experiment to Save the World," and it was broadcast on Feb. 16, 2005. In addition to the unusual title, the show had other oddities, for example, the manner in which the narrator introduced the challenge to replicate Taleyarkhan's method of bubble/sonofusion: "If the result is positive then this man will be on the way to a Nobel Prize, and a dream of a shortcut to a world with unlimited cheap energy could finally be within reach. But if it fails, one of the great dreams of science will surely die."

The show went through some of the history of "cold fusion" for perspective and conveyed how it had "turned into a scientific embarrassment of epic proportions."

"It seemed that the dream of a short cut to nuclear fusion was dead," the narrator said. "But then something happened to resurrect the dream. It began when physicist Seth Putterman heard about something that seemed more like magic than science. It was a way of turning sound into light. Seth Putterman was so intrigued by this idea that he set about trying to do it himself. It's a process called sonoluminescence."

The show introduced Taleyarkhan, mentioning his second published paper and some of the related controversy.

"But despite this second publication, many sceptics still weren't convinced," the narrator said. "They believed that there was one vital measurement that he still hadn't made. It was a measurement that could finally prove once and for all whether Rusi Taleyarkhan's neutrons really were from fusion. It was all to do with timing. If fusion was taking place, neutrons should be recorded at the very moment the flash of light was given off."

The dramatic ending of the show slowly panned the BBC "experts" one by one.

Ominous music played in the background.

Each of the "experts" read Putterman's "negative" results to the camera like a panel reading a death sentence verdict: Putterman had failed to replicate Taleyarkhan's results.

The narrator put the final nail in the coffin, concluding that Taleyarkhan's "dream of a shortcut to unlimited clean energy forever must remain just that, a dream."

A few interesting points to note:

- No mention is made in the BBC show that Taleyarkhan's primary claim was based on the evidence of neutrons and tritium, not timing coincidence. Precise timing is Putterman's specialty. Theoretically, the producers of the show could have looked for nuclear ash. However, for some reason they set as the goal not that but rather timing coincidences.

- Taleyarkhan had two published papers claiming positive evidence for sonofusion; Putterman had then, and as we go to press, none.

- In 1994, Putterman had applied for, and in 1999 was granted, U.S. Patent 5,659,173 for his particular method of bubble/sonofusion, though he hadn't achieved fusion conditions in his bubble/sonofusion method.

- The BBC paid $70,000 to Putterman to perform the replication. (This is the same organization which, producer Murray states, holds "impartiality" as "the key to its reputation" yet declined to compensate Taleyarkhan for his time, instead offering him only a bottle of liquor.)

- Putterman's response: "I'm desperate for money, and here's a chance to infuse my laboratory with overhead-free money. ... We had fun."

While it's not unusual for scientists to be desperate for money, it was unusual, to say the least, for Putterman to have "fun," while making the fundamental error of putting noncondensable gas into his experiment, publicly pronouncing his result negative, and claiming that he had properly replicated Taleyarkhan's experimental conditions.

As Lahey explains later in this report, the use of noncondensable gas would kill the experiment. He and Taleyarkhan documented this fact in their papers, as well, Lahey explained this in his lecture on bubble/sonofusion at UCLA. [4]

- On April 28, 2005, Putterman, Naranjo and UCLA chemistry professor Jim Gimzewski attained worldwide acclaim for a completely different variety of fusion, crystal piezo fusion. [5]

-- Mark Peplow of *Nature* wrote enthusiastically on April 28, 2005, about the new crystal piezo fusion paper published in *Nature* and how "Putterman helped to discredit [the cold fusion] claim, as well as more recent reports of bubble fusion."

Coincidentally, perhaps, just one week before the BBC show aired, on Feb. 7, 2005, two students of Taleyarkhan's, Yiban Xu and Adam Butt, had just learned that the paper reporting their replication of the Taleyarkhan work had been accepted in *Nuclear Engineering and Design*. [6]

Readers who care to go back to read Taleyarkhan's letter to Colin Murray will notice that Murray had been tipped off by someone to a forthcoming replication.

Eventually, Putterman and Naranjo did publish their BBC-sponsored replication attempt, two years later, on Feb. 9, 2007.

For comparison purposes, Martin Fleischmann and Stanley Pons, who were shamed for holding "science by press conference," at least had their paper submitted to and accepted by a peer-reviewed journal before they announced their "cold fusion" discovery on March 23, 1989.

Related text from the transcript of the BBC broadcast is relevant:

> NARRATOR: "We invited Rusi Taleyarkhan to come to the laboratory and check Seth Putterman's equipment," the narrator said, "but he declined our invitation on the basis that in the small and competitive world of fusion science he did not feel comfortable with Seth Putterman's group."
>
> DR. RUSI TALEYARKHAN: I would help out anybody who I feel, who I felt comfortable with. I would, I would, but I have to be comfortable with that particular group.
>
> INTERVIEWER: Why, why is that, because is it not just science?
>
> DR. RUSI TALEYARKHAN: I will not answer that question right now.
>
> NARRATOR: So without Rusi Taleyarkhan's input to the experiment, our team had to follow his recipe for fusion from the published papers.

Taleyarkhan takes exception to the manner in which this "invitation" was presented by the BBC.

This is the way Taleyarkhan recalls Murray's "invitation" at the end of the second day of filming, on Sept. 18, 2004: "All of a sudden," Taleyarkhan said, "Murray tells me that he'd like me to do a replication and asks if I would cooperate with Seth Putterman and Ken Suslick. And that's when I said I don't feel comfortable with that group.

"That was the extent of my discussion with him about that subject. I didn't want to get into any more about personal conflicts and all that."

Asked about the part where they invited him to check out Seth Putterman's equipment, Taleyarkhan replied, "That is the biggest lie. The Putterman experiment hadn't even begun yet, and I had not even imagined that Murray would then go forward and do such a thing."

CHAPTER FOUR

New Publications and New Objections

One month after the BBC *Horizon* show, Suslick achieved fame and praise for a significant advancement in his own pursuits.

Suslick, who pursues the single-bubble sonoluminescence method as opposed to Taleyarkhan's multibubble method, was able to get his bubble beaker hotter than ever before, hot enough to form a plasma. He made no claim for fusion, because a plasma is only a precursor to the conditions required to create fusion. Not surprisingly, he failed to find neutrons.[1]

Nature, the journal that published the Suslick paper, wrote, "Flannigan and Suslick's experiments are a milestone in single-bubble sonoluminescence, as they constitute the first direct measurement of the temperature and the state of matter in a single bubble at collapse."[2]

The excitement that Suslick got his bubbles hotter than ever before, but without any evidence of fusion, was sufficient for *Nature* to write that Suslick's research "raises hopes that the effect, called sonoluminescence, might one day be used as an almost limitless source of energy."[3]

Chemical & Engineering News quoted Suslick: "This is the first time that anyone has obtained strong experimental evidence for a plasma during acoustic cavitation." [4]

Nature did not hesitate to point out the "controversial" work of Taleyarkhan and wrote that "Suslick, along with almost every other researcher in the field, says that 'bubble fusion' has yet to be proved." *New Energy Times* is not aware of whether *Nature* performed a survey of the field or was expressing an opinion.[5]

Putterman Objects Again

On Jan. 10, 2006, to the surprise of many people, the Taleyarkhan group revealed that it had been working very quietly on a radical variation of its multi-bubble sonoluminescence experiment, and the group announced that a new paper of its had been accepted for publication in *Physical Review Letters.*

"Nuclear Emissions During Self-Nucleated Acoustic Cavitation" described the next evolution in Taleyarkhan's work. Even though their second (2004) paper answered many of the questions and concerns about their first (2002) paper, a nagging problem, according to Putterman, remained. He asserted that, in order for Taleyarkhan to prove his claim of fusion, he must show a precise correlation between the sonoluminescence flash and the emitted neutron in the experiments.

In Telepolis, Haiko Lietz wrote, "Putterman demands a timed coincidence between flashes of light due to sonoluminescence and neutron measurements in a time window of a billionth of a second."[6]

According to West and another physicist familiar with a variety of fusion research, the matter of timing coincidence had some importance before the 2006 Taleyarkhan paper.

The reason was that, in Taleyarkhan's earlier experiments, he used an external device known as a pulse neutron generator to seed, or initiate, the reactions. But Taleyarkhan also was claiming to detect neutrons emitting from his reaction. Thus, distinguishing between the two was important in order to eliminate one possible source of error.

Rather than pursue the path of trying to mark the time of the sonoluminescence flashes to a billionth of a second, Taleyarkhan redesigned his experiment to bypass that concern. His new experiment used dissolved uranium-238 as an alpha source to provide the nucleation site, eliminate the external source of neutrons and make the matter of timing moot.

West spoke with *New Energy Times* in detail about the timing argument: "It's certainly true that we needed to distinguish the neutrons emitted by the reaction from the neutrons that had been used to nucleate the bubble which later collapsed and led to the nuclear reaction.

"It may help if I provide some technical background. When the bubble collapses, it may or may not give off a sonoluminescence flash. It turns out for reasons that, as far as I know, no one understands, the sonoluminescence signals from acetone are pretty weak. For perspective, the kind of liquid that I used gave you

a sonoluminescence flash that you could see with the naked eye. However, the flash seems to be weak in acetone. We need to remember that, because this may mean that you may not see sonoluminescence signals from each bubble collapse because it's such a weak signal; you might miss it. If it only sends out a certain number of photons and they go out in random directions but none of them lands on your photomultiplier, you may never know it had happened.

"The second thing to understand is that, when they collapse, they also give off a shock wave, an acoustic shock wave in the liquid. Now the sonoluminescence flash of course travels at the speed of light, but the shock wave from the collapsing bubble travels roughly at the speed of sound.

"So there is time delay between when the bubble collapses and when the shock wave from it reaches the microphones that are fastened onto the outside of the chamber.

"So you would expect to detect a sonoluminescence flash, essentially, within a fraction of a nanosecond of the actual end of the collapse, if you are going to detect it at all.

"When I performed earlier experiments with a liquid that gave a good bright flash, I detected every flash. I know I did in two ways: I have two photomultipliers looking at it, and I always saw that signal in them both. And secondly, whenever I saw the flash, I later saw the shock wave for every flash, delayed, as you would expect relative to the speed of sound.

"My understanding is that, in our experiments using acetone, I know we did not detect sonoluminescence signals from every bubble. And I attribute that to the fact, although we never did an investigation to prove it, that signals were weaker in the acetone and we most likely missed some of them. And we always got the acoustic signal.

"If I run through the sequence briefly, you fire the pulse neutron generator to trigger the bubble, the bubble will grow and collapse on the order of 30-to-50 microseconds later. When it collapses, it emits a shock wave which you detect on your microphones another 30-to-40 microseconds later because that's the time it takes the acoustic signal to travel.

"So if you detect a neutron from the reaction, those are traveling very quickly, and they arrive at your neutron detector before the shock wave does.

"So if all goes well, you should see the first neutron signals from the pulse neutron generator - they travel faster, having an energy of 14MeV - and then you would see the reaction's neutron signals - they have an energy of about 2 MeV.

"I realize some people have said that the time it takes for the neutrons to reach the detectors is important, but it is not. The time is so short for these neutrons that the time of flight is almost instantaneous for both the pulse neutron generator and the reaction's neutrons. It's way outside of the timing capability of our apparatus, even if we knew when to start the clock. And the shock wave signal is no use for starting the clock to time those neutrons because it doesn't get to the detector until after the neutrons have gone by.

"I don't know how to put it more politely, but I think that the demand for timing coincidence is bullshit, and I think that for two reasons. The first reason is that you can't be sure that you will detect every sonoluminescence flash. And if you remember from the earlier papers, we believe there is not a single bubble but a cloud of bubbles. You have absolutely no possible way of knowing whether the same bubble that generated the neutron that you saw was the one that generated the flash that you saw.

"Now, it is worth attempting, and we did try to get the coincidences, but if you don't, there's all these other things, like the energies of the neutrons, the control experiments, and so on, that are much better. I wouldn't regard the matter of timing as any sort of proof or disproof, for the reasons just mentioned.

"Also, in the self-nucleated experiments, you are generating bubbles all over the place, all the time, and if you do in fact detect flashes, again you would have no way of saying that this flash and this neutron went together, because, in most cases, you wouldn't detect the neutron because it goes off in a different direction that doesn't hit your detector, and in some cases, you wouldn't detect the flash for the same reason, or it may be shielded by another bubble.

"I would like to add something. When Rodney Howlett and I did our experiment, published in *Nature* in 1967, there was a debate going on in the community about whether the sonoluminescence flash came when the bubble started appearing or when it collapsed. It was a reasonable question at the time. So what we did was to measure the time delay between the two, and, as it shows in the paper, we settled that question.

"Now, the difference is that, back then, we were using timing as a way of determining the correlation between that flash and the bubble behavior. That's already established; we don't need to go back and do that again. We know it comes when the bubble collapses. And there were logical reasons to ask this question at the time, but it's a completely different argument that is being presented by some of Taleyarkhan's critics."

CHAPTER FIVE

Battling Charges of Californium Contamination

The day after the self-nucleated paper was accepted for publication in *Physical Review Letters,* William Coblenz, a program manager with the defense sciences office of the Defense Advanced Research Projects Agency sent an e-mail to Taleyarkhan and asked him "to set up a time for a meeting at Purdue where I can see the reactor in operation. I then want to discuss the quickest way to duplicate these results in Seth's lab."[1]

A DARPA-sponsored self-nucleated replication at UCLA never happened. Six weeks later, all hell broke loose at Purdue.

DARPA Sonofusion/Bubble Fusion Review

On March 1, 2006, around 9:30 in the morning, a dozen people involved in the DARPA bubble/sonofusion program walked into Taleyarkhan's laboratory for an on-site review of his work.

Taleyarkhan recalls the following participants: William Coblenz (DARPA), Graham Hubler (Naval Research Laboratory/Office of Naval Research), Peter Schmidt (Office of Naval Research), Ross Tessien (Impulse Devices), Felipe Gaitan (Impulse Devices), Wylene Dunbar (Impulse Devices), Kenneth Suslick and two students (UIUC), Seth Putterman and one or two students (UCLA) and JaeSeon Cho (ORNL).

Taleyarkhan told *New Energy Times* that the stated purpose of the meeting was to "review the status of ongoing efforts at UCLA and Purdue related to the neutron-seeded cavitation fusion experiments," though he said that such reviews were unusual.

"In all the four years that we did bubble fusion work between 1999 to 2003 at Oak Ridge," Taleyarkhan said, "there was never a site visit for review."

As noted earlier, Coblenz requested the on-site visit a day after Taleyarkhan's 2006 paper was accepted.

One day earlier, on Jan. 10, *Nature,* reporting on the new Taleyarkhan paper, wrote, "Taleyarkhan says that Suslick and Putterman are welcome to visit his lab

to see the results for themselves. Both are eager to go as soon as possible. 'We look forward to seeing the experiment run,' says Putterman."[2]

"Over the course of several hours," Taleyarkhan wrote, "guests were shown details and a live demonstration of the neutron-seeded cavitation fusion experiment which had been sponsored by DARPA."

They also were shown a live demonstration of the newer alpha-mode experiment, which did not depend on an external neutron generator.

"Guests participated hands-on for an hour or so," Taleyarkhan said, "to set up the nuclear track detectors and then monitor the results at the end of the day."

Taleyarkhan performed these demonstrations using CR-39 nuclear track detectors, a special type of thin plastic that is ideal for registering charged particle emission and neutron emissions. A significant advantage of CR-39 is that it eliminates any uncertainties about interference that otherwise may occur using electronic detectors.

The detectors would be impervious to any possible alpha emission from the experiment because they were outside the flask, which is impenetrable by alpha particles. Such detectors are also unaffected by gamma radiation.

Taleyarkhan used the 80-micron polyethylene plastic covering that ships with such detectors for protection as a neutron radiator to boost the efficiency of the CR-39.

He explained to *New Energy Times* part of his protocol for calibration and exposure: "Two detectors are placed in diametric opposition immediately outside the experiment, and a third detector is placed about a meter away on a table with the same covering to monitor changes in the background."

Taleyarkhan explained some of the controls used: "Only those detectors mounted on reactors with deuterated benzene-C2Cl4-D-Acetone and only when cavitation is on provided us with 8 to 14 standard deviation increases in neutron counts. Experiments without cavitation but deuterated fluids gave null results. Detectors placed a meter away recorded no change. Detectors for all control experiments (that is, with hydrogen-bearing fluid) gave null results. Finally, experiments with heavy water (D2O,) even with cavitation also gave null results – something that was predicted by Nigmatulin/Lahey et al. theory."

Tessien had asked Taleyarkhan whether he could run the experiments the following day, as well, to see whether Taleyarkhan could repeat the work on demand. Taleyarkhan agreed on the condition that Tessien support him with a signed statement testifying to the results of the experiments that were shown to the group on March 1. [3]

CR-39 detectors begin to accumulate tracks, ever so slowly, from their date of manufacture. Tessien estimates that an unused detector has a monthly track accumulation rate of about five hits, or tracks, per month. In Taleyarkhan's experiment, which lasted for only a few hours, it is likely that one background neutron, give or take, will hit it, thus giving an uncertainty factor of one.

Based on the results, the tracks registered on these detectors can be considered to have about 15 hits from previous accumulation, plus one from the background during the experiment, ending up with at least a 10:1 signal-to-noise ratio, which isn't too bad.

The experiments on March 2 showed positive results, as well, and a similar signal-to-noise ratio.

If we turn the clock ahead for a moment, comments made by Putterman and Suslick in the press a year later appear inconsistent with these events.

"'If a scientist feels they've made a great discovery, it's their obligation and joy to convene other scientists from other schools to come to their lab to observe it,' Mr. Putterman says. 'As soon as that doesn't happen, that scientist has to be prepared for the backlash of the scientific community.'" – *Chronicle of Higher Education*, April 2, 2007[4]

"'It's a very sad and regrettable time when a scientist has to stand up against another scientist,' Suslick said. 'If Taleyarkhan wants to dispel all of this, he can do so very quickly. He can bring a bunch of people into his laboratory and reproduce his experiment right in front of their eyes.'" –*Journal and Courier*, May 11, 2007[5]

Putterman Drops the Bomb

More than just science happened on that day in March at Purdue. According to Taleyarkhan, as well as in e-mail comments from Coblenz, a conflict developed between Putterman and Taleyarkhan that afternoon.[6]

Taleyarkhan stated that, during this meeting, Putterman suggested that Taleyarkhan's results looked like contamination from an external source, rather than from his experiment, and that he insinuated fraud.

"Seth Putterman," Taleyarkhan said, "all of a sudden made his allegation saying, in front of the whole group, that our data that we published in *Physical Review Letters* in January 2006 seemed to be coming out of a californium source."

Putterman wrote that he does "not remember announcing at a DARPA site visit that Rusi's work is fraud."

According to Taleyarkhan, Putterman said that the data, the neutron spectrum, in Taleyarkhan's paper looked exactly like what you would see if the element californium-252 were present in the vicinity of Taleyarkhan's experiment. This speculation would provide a routine explanation for Taleyarkhan's experiment instead of supporting his claim of a major discovery.

The suspicion of contamination does not constitute by itself an accusation of fraud; however, the implication is unavoidable because such carelessness in the handling of californium would be unlikely from a senior researcher like Taleyarkhan. The hostile manner in which Putterman conveyed his "concern" also contributed to the ominous nature of his pronouncement. The californium would have to be removed from the test environment for the control experiments, so the allegation implies more than just carelessness, even outright deception.

"To my utter dismay," Taleyarkhan wrote, "while we were giving good-faith demonstrations of not one but two experiments that day, Putterman had the gall to embarrass me in front of the DARPA sponsors by insinuating fraud in regard to our group's January 2006 *Physics Review Letters* paper related to evidence of nuclear emissions during self-nucleated acoustic cavitation.

"He just showed the analysis, and mind you, this is the first time I heard about it, I said, 'Look, when an analysis is run along these lines, you need to write it up so that I can give it some thoughtful, careful assessment. You cannot just come out like this, all of a sudden, and tell me about this kind of analysis.'"

When asked whether Putterman presented Naranjo's analysis at the time, Taleyarkhan said, "The analysis was apparently done, and he showed me the curves and just made the allegation and asked for an immediate response, and I said, 'I cannot give you an immediate response. I need to see a paper written up on this, some draft report or something so that we can give a proper response.' And we left it at that."

Nature Reporter Gets Involved

It was around 2 p.m. when Putterman made his announcement to the group, Taleyarkhan explained. "And all of a sudden, within two hours, I get my e-mail – I checked between experiments and coffee and all those things – and right around 4:00 or so that afternoon, the reporter working for *Nature* writes to me [and left voice mail] demanding an explanation – demanding an explanation – for that allegation. That was Eugenie Reich."

The e-mail that Taleyarkhan received read as follows:

From: Eugenie Reich [mailto:eugenie.reich@gmail.com]
Sent: 01 March 2006 20:31
To: 'Rusi Taleyarkhan'
Subject: left message

Dear Dr. Taleyarkhan,

I have left a voicemail. Would you like to add any comment or explanation as to why the simulation by Brian Naranjo appears to be so much of a far better fit to the spectrum of the californium fission than to fusion?

Eugenie Samuel Reich
freelance journalist, science and technology
Cambridge, Massachusetts
phone: +1 617 354 0329
cell: +1 617 821 1538
nasw.org/users/essreich

Taleyarkhan recalled that Reich had contacted him several weeks earlier, sometime in February.

Taleyarkhan wrote that, at the time, "she said she was a freelancer and that she might submit it to *Nature* or some other publication. Her questioning started with relatively innocent queries but then quickly, within days, escalated to questioning technical details, with an accusatory/hostile tone. Furthermore, her questions sounded insulting, and it seemed like she already had made her mind up and was creating news rather than reporting it. At this point, I started having questions about her integrity.

"Upon discussion with my co-authors, it was getting clear that the writer was striving for a sensational story stoked with input from detractors and competitors. I advised her that the proper norm and time-honored tradition in the scientific community is to transmit technical questions or issues to the editors of the relevant journal [in this case, *Physics Review Letters*], who would then communicate with the authors for a thoughtful rebuttal/clarification to benefit their technical readership. If I had known she was with *Nature,* it would have changed the entire dynamic, and I would have contacted the editors to discuss it with them."

Taleyarkhan's surprise turned to shock when he saw, one week later on March 8, that Reich had published a stinging set of four articles in *Nature* based on the allegations of contamination from Naranjo.

"I did not know this story was coming in *Nature*," Taleyarkhan said, “until the day before, when another journalist from IEEE Spectrum called to ask."

Although Reich had first made contact with Taleyarkhan on Feb. 20 about what was to be the forthcoming article in *Nature*, the first time she mentioned anything to him about the allegation of contamination was on March 1, according to Taleyarkhan.

Taleyarkhan told *New Energy Times* that, had he known Reich was writing for *Nature*, he would have proactively contacted the editors of *Nature*.

"No one from *Nature* ever called or wrote to confirm, alert or fact-check for something as serious as this," Taleyarkhan wrote.

Reich disagrees that Taleyarkhan was not informed that the piece was for *Nature*.

"I informed Taleyarkhan 21 Feb. the article will likely be published in *Nature*,” she said, “and I updated him on 22 Feb. that *Nature* had accepted the story."

New Energy Times does not know whether Reich had advance information on Feb. 22 about the forthcoming allegation and has no information about how, when or why Reich might have learned about Putterman's allegation within two hours after he made it on March 1.

Kenneth Chang was the first to break the story, around midnight between March 7 and 8, and *Nature* followed several hours later. The story was *Nature's,* but it had given Chang an embargoed copy in advance, as often happens to help get the word out quickly. Chang found a way to get around the embargo and be the first to write about the situation, although his story was a bit out of context because it ran in advance of the *Nature* stories and he was not permitted by the embargo to mention the content in the *Nature* stories.

Nature ran four articles by Reich on that day, all of them quite nasty, both toward the idea of bubble/sonofusion in general and toward Taleyarkhan in particular. Reich did not use the word fraud to describe Taleyarkhan's work; however, the articles were crafted in such a way that it was the obvious interpretation, as evidenced by the headline given by Reuters an hour later. Being a wire service, Reuters succeeded in sending the words "science fraud" worldwide within minutes.

It is difficult to understand how Reich had the confidence to write such a destructive set of articles based on the limited evidence she had at the time.

However, she did have a tenuous ground for the story. Reich relied on an unpublishhed pre-print posted to a Web site the prior day. Given the timing, Reich, Putterman and Naranjo could have conspired in this matter.

The day before the story ran, Naranjo posted to a Web site an analysis showing how he was confident that the Taleyarkhan neutron data were not derived from the actual experiment but from a californium-252 source, suggesting that the experiment had been spiked.[7]

Debunking the Californium Claim

Taleyarkhan eventually debunked Naranjo's theory in the formal scientific literature, but it took some work and time; it published seven months later. Few, if any, in the news media reported it, allowing to go uncorrected a significant misunderstanding that they had helped to create back in March.

Reich used an un-peer-reviewed, unaccepted and unpublished manuscript from a research group that was a direct competitor to Taleyarkhan's as the basis of her articles. To make matters worse, she failed to so much as hint at either the immature status of the paper or the conflict of interest.

Nature accepted Reich's story idea on Feb. 22. Writing and producing the four articles took a significant amount of work, not likely something that could occur in the span of a few hours. The californium-252 allegation was made on March 1. Chang had Reich's story on the same day Naranjo signed and uploaded it.

Eventually, in October, Naranjo's manuscript did pass peer-review and get published.[8]

However, Taleyarkhan was given a chance to rebut concurrently. His reply revealed a major flaw in Naranjo's speculative allegation. [9]

"The Comment [by Naranjo] has overlooked important aspects which result in incorrect conclusions.

“Rather than argue about the merits or demerits of [Naranjo's] attempts at a computer code calculation for a ‘presumed experimental configuration and instrument settings-cum-performance,’ we directly obtained additional experimental data with our laboratory’s Cf-252 source. ... We then show by direct one-on-one comparison in Fig. 1 that the reported spectra in our Letter for neutron and gamma photons are significantly different from corresponding spectra derived from a Cf-252 source."

In other words, Naranjo had used a computer simulation to craft his speculation that the neutrons observed in the Taleyarkhan experiment had come from Cf-252.

Second, Taleyarkhan explicitly ran such an experiment and found that the complete nuclear signatures do not provide the perfect match, as Naranjo had speculated.

Not willing to give up so easily, Naranjo attempted to comment on Taleyarkhan's reply. However, the editor of *Physical Review Letters* said that it was not the policy of the journal to consider a comment on a reply.

Despite the fact that Naranjo's attempt was rejected by *Physical Review Letters*, Reich reported his attempt as if it were scientific fact*,* repeating the same error of protocol that started the entire problem, with her report of the californium speculation – failing to differentiate between authoritative science media (peer-reviewed journals) and nonauthoritative media (science press). She succumbed to the dubious but not infrequent practice of “doing science by press release.”

Taleyarkhan explained more about the faulty UCLA analysis: "The obvious has been overlooked in the feeding frenzy caused by the alleged computer code calculations for an imaginary experimental setup. The reason for Putterman and Naranjo claiming Cf rather than some other source like Pu or accelerator-based source was convenience; the average neutron energy from Cf comes close to the D-D fusion energy of 2.5 MeV.

"However, one crucial item that was overlooked by UCLA relates to gamma emissions. A Cf source emits about four to five times more gammas than neutrons; for our bubble fusion reaction on the other hand, it is exactly the opposite. One gets only about 1 gamma for every 10 neutrons. This fatal flaw and oversight was pointed out to the referees and published in our October 2006 rebuttal in *Physical Review Letters*. Fortunately, we don't have to argue about neutron-gamma emissions from Cf."

New Energy Times asked Taleyarkhan whether anyone had asked him how his March 1 and 2 experiments, performed in front of a dozen witnesses, could have been spiked with californium.

"Nobody has yet asked that question," Taleyarkhan replied. "The air on March 1 was clouded by the surprise announcement of code calculations which apparently Reich got to hear about right thereafter. The obvious results were overlooked even with a dozen-odd people acting as witnesses."

Reporter Erik Vance, writing for the *Chronicle of Higher Education* on Monday, April 2, 2007, missed a few important aspects of the larger story and the related science. However, he was accurate with this comment, which surprisingly was the first time, one year later, that any reporter noticed the illogic of the californium allegation.

"In order for this new claim to be true," Vance wrote, "Mr. Taleyarkhan would have to be going to considerable effort to hide a piece of the metal near or in his experiment, along with some kind of device that shielded it and unshielded it as adjustments were made with equipment. Without such a shielding mechanism, any californium-252 in the laboratory would have registered even when the experiment wasn't running." And they would have registered on the control experiments, too.[10]

Taleyarkhan wrote that the californium speculation was moot for yet more reasons.

He wrote that two other individuals came to Purdue on their own to his laboratory, one to observe and audit the experiment and another to perform his own experiment using Taleyarkhan's apparatus.

Edward Forringer, a physics professor at LeTourneau University in Texas, came to Taleyarkhan's lab in May 2006 with two students and performed his own experiments, found positive results and published the results in an American Nuclear Society conference proceeding.

William Bugg, who Taleyarkhan states is a "nuclear scientist with more than 50 years' experience, presently a research professor at Stanford University and until recently the head of the department of physics at the University of Tennessee, Knoxville," performed his audit of an experiment carried out by Taleyarkhan in June.

Taleyarkhan wrote that he had not worked with either person, that they approached him to find out for themselves whether the *Nature* allegations had any merit and that they came to Purdue on their own grant funding and resources. Taleyarkhan wrote that the Forringer work was funded by a grant from the Welch Foundation; he did not provide information about the funding for Bugg, who drove there from Tennessee.

"We successfully reproduced the spectra type we published in January 2006, got and read data with up to 9 standard deviation statistical significance with two independent detector systems," Taleyarkhan wrote. "They then all conducted their own analyses, wrote their own reports, had them peer-reviewed and accepted for presentation and publication at two International conferences in November 2006.

"I really feel for Brian Naranjo because he's a postdoc, and this is not right. He should be doing work that is useful and that he can get published. Instead of that, he is spending and wasting his time doing this kind of forensic work? For what?"

The conflict that developed at the Defense Advanced Research Projects Agency review meeting at Purdue on March 1 triggered several consequences.

CHAPTER SIX

DARPA UCLA Replication Failure

Turn the clock back now to the fall of 2004. Around the same time that Putterman and Naranjo were working for the BBC to attempt a replication of Taleyarkhan's work, the UCLA team had also submitted a proposal for a Defense Advanced Research Projects Agency-funded replication of the same work. According to Taleyarkhan, a subcontractor on the proposal, it was sent to the agency for approval in the October/November 2004 period. The principal investigator on the contract was Seth Putterman, and UCLA was the prime contractor; a few other subcontractors were involved, as well.

New Energy Times believes this proposal was approved sometime in February/March 2005, after the BBC broadcast Putterman's first failure to replicate the Taleyarkhan results, and the grant was funded in May.

Sixteen months later, on Sept. 21, 2006, Putterman submitted his manuscript to *Physical Review Letters*. The paper eventually published five months later, on Feb. 9, 2007. It states, "Observation of thermonuclear fusion generated by cavitation in deuterated acetone has been reported by Taleyarkhan and co-workers. ... Here we describe our unsuccessful attempts to reproduce the claimed effect. Shapira and Saltmarsh, Tsoukalas and Saglime have also reported null results."[1]

A common misperception is that a failure to replicate implies that the claimed phenomenon does not exist.

This logic fails because such a conclusion assumes that new science can be, and is, well understood at the beginning by its originators and can be confidently and effectively communicated to others who have no prior experience in the exact work. Both these assumptions are unlikely.

Putterman et al. chose to list the work of others who had failed. The significance of the inclusion of this text is unclear. Does it mean that the others' replication attempts were invalid? Probably not. They probably listed the work to influence the reader to think that the Taleyarkhan work is invalid.

Putterman worked with Suslick on this paper, and together they reported to the press, as stated in their paper, that they reproduced Taleyarkhan's experiment,

built it to his specifications, it didn't work, and they found no evidence for fusion.[2,3]

As expected, a finger-pointing episode followed, with the replicators stating that they had followed the directions of the originators.

Guizo of IEEE Spectrum reported that Lahey, on a visit to UCLA, said that "Putterman was using a design 'that was doomed to failure' and that he told him so when visiting his laboratory at UCLA last year."[4]

Putterman disputed this, saying his experiment was built to Purdue's specifications.

Regardless of who or what is to blame for the breakdown in the communication between the two groups, whatever Putterman did, it most certainly did not reproduce the Taleyarkhan experiment, according to Taleyarkhan.

"He simply did not replicate our experiment," Taleyarkhan said, "and that is manifested in the way his experiments were run and the way in which he described the functioning of his apparatus in his published paper."

Taleyarkhan denied that his group had failed to communicate the lab protocol sufficiently to Putterman's group.

"I think that's a crock," Taleyarkhan said. "Giving somebody guidance and them having the ability to follow the guidance to its fruition, that's a completely separate thing.

"I had given him CDs of key data on what to be looking for as a bare-bones minimum, number of bubble bursts per second, the shape of the bubbles and the general configuration of the system, what drive amplitudes, what kind of neutrons for nucleation, at what point in time, how you epoxy your transducers to the glass, what kind of glass blower you've got, and so forth. Based on what he reported, he apparently did things some other way."

New Energy Times asked Taleyarkhan whether there was any reason his group might have withheld design information.

"No," Taleyarkhan said. "I sent the UCLA group the technical information after Lahey's visit."

Taleyarkhan also wrote in an e-mail that this obligation was specified in the DARPA contract.

On the question of why the UCLA replication attempt was "doomed to failure," Taleyarkhan provided a short video clip[5] of the UCLA experiment that had been

given to him and explained what it showed: "The operations, including the drive train and other operational factors such as care for degassing and wetting of motes, would be the likely culprits. Additionally, the fabrication of the cells.

"The bottom line is that the test cell is producing long-lasting (hundreds of milliseconds' duration rather than our five milliseconds) distinct cometlike dissipative streamers, which is the first thing one gets in these experiments, and very easily. However, this effect kills hopes for spherical implosion-based high-enough compression.

"The other feature seen in the video is the rate of nucleation: one or two every five seconds or so in the UCLA apparatus versus 25 to 30+ every one second as we published in *Science* (2002), *Physical Review E* (2004) and *Nuclear Engineering and Design* (2005).

"The effect of buildup of resonance phenomena-induced growth and physics are completely neglected, as is the importance of deriving transient bubble evolution patterns where (as we have shown in our 2004 *Physical Review E* paper) the majority of sonoluminescence flashes and neutrons transpire after the first implosive collapse.

"As far as I can tell, all of this was just ignored. This week I've conveyed to DARPA's manager that the UCLA group cannot be helped. To have one of the world's leading scientific magazines like *Nature* draw conclusions relying on expertise from the UCLA group is to us a travesty of science. Having proven themselves incapable technically, they then resorted to allegations based on contrived modeling and simulations of imagined experiment geometries."

In a related conversation with Taleyarkhan, Lietz pointed out that Xu and Butt reported in *Nuclear Engineering and Design* that "cometlike structures can last for several tens of milliseconds and appear to play a critical role in terms of their ability to induce bubble nuclear fusion."

"That is right," Taleyarkhan clarified. "Xu showed categorically that cometlike structures did not allow bubble fusion in his papers."

Lietz inquired whether that implied that UCLA had gotten close to achieving the right conditions.

"Quite the contrary," Taleyarkhan replied. "Getting cometlike structures is easy and one of the first things one sees. One not only needs to get out of this mode but also to get the sonoluminescence time evolution history as shown in our *Physical Review E* (2004) paper."

And that brings this story to March 2006. Five days after the on-site DARPA review at Purdue, Coblenz informed several members of the sonofusion group

that, as a result of the UCLA failure to reproduce the results, DARPA would not continue funding the next phase of sonofusion work.

"From a scientific point of view," Coblenz wrote, "the hypothesis that the conditions for D-D fusion in acoustically cavitated deuterated acetone has been neither proved or disproved."

A Collaboration Doomed to Failure

[The following interview was conducted on May 25, 2007.]

Steven Krivit: Can you help me sort out what happened at UCLA when you went there, particularly with this matter regarding the design drawings?

Richard Lahey: Well, let me give you a little background first. I've been involved with Rusi ever since day one on this stuff, and the test section that he used at Oak Ridge and which he's been using ever since is a design which is a modification of one that Colin West used a long time ago for a completely different purpose. He used it to try to measure neutrons.

Colin worked at Oak Ridge, and he helped us out to make an acoustic chamber that allows you to compress bubbles in a spherical way, bubble clusters,
so that you can get to the kind of conditions that are necessary for sonofusion.

The problem is -- and unfortunately it's easy to look back and see it; at the time we weren't aware of it -- that each one of these test sections is hand-crafted. There isn't anything like machine drawings made of them that specifies the glass thickness and the tolerance and the gap thicknesses, etc.

The way Rusi and Colin worked is they had built different test sections, and so they got one that worked, but there were lots more, 30 or 40, that didn't work.

SK: What does the term test section mean?

RL: That's the acoustic chamber. That's where the experiment is run, the container for the deuterated acetone, a glass tube about 100mm tall and about 60mm in diameter.

The only thing that I've ever seen as far as drawings is a sketch that Rusi made of the conceptual design. When we got it here at RPI, we had to sign a nondisclosure agreement with Oak Ridge because Oak Ridge

views it as their proprietary information. It's my guess, but I'm not sure, that that is what Putterman got from Rusi.

[Taleyarkhan confirms this.]

It's my understanding that engineering drawings do not exist, that Rusi's design is a hand-drawn sketch.

[Taleyarkhan confirms this.]

To get it to work, it's not like you can just build the test section and it will work. There is a fair amount of art in there, and you have to get lucky to make it work. It's very frustrating for others because none of that was ever published. Even Oak Ridge wouldn't allow the details of it to be published, so people had to imagine what one of these test sections would look like. Unfortunately, it is very sensitive to the design because, when we got it, I had one of my Ph.D. students do a very detailed analysis of the test section, to figure out the optimum gaps, the effect of different glass thickness. It's very sensitive to that kind of stuff; you can get either a really good test section or a really bad test section, but if you look at them, they might appear the same.

So we said, well, we can't use the optimal design of Oak Ridge because we can't release that design to others, so let's come up with a different design that is optimized to work, and make machine drawings that people can take to a shop, build them and get the same results every time.

So we did that, came up with a design and made 3D computer drawings, and we wrote a paper on it.

Then I visited UCLA - they had invited me to give a talk on sonofusion - Putterman did not attend the lecture, but his students did.[6]

Before the lecture, I went over to visit Putterman's lab and talk with him and his students. When I looked at his test section, it was absolutely clear that it was not going to make it. It wouldn't work for sonofusion. It would work fine for sonoluminescence, where you don't have to get it to these high compressions. In that work, you drive it at one atmosphere, but in sonofusion, we're talking about 15 atmospheres.

I told him, “It's just not going to work,” and in fact, it didn't. As I understand it, he then got the sketches from Rusi and tried to go on, based on that design which he said didn't work.

[Taleyarkhan confirms this. Putterman declined to comment.]

I had offered at the time to give him the information. I did send him the paper, and it had all the detailed analysis in it, how to design the test section so it's going to work. I offered to send him the design drawings of the one we built at RPI, but he never responded to my offer.

He probably figured that, after he got the information from Rusi about the Oak Ridge design, he didn't need my drawings.

Other people have taken me up, though; I've given our drawings to a professor in the U.K. who is running experiments right now.

Honestly, I can understand how other people can become very frustrated and say that this is not real. Very few people have the tenacity or the funding even to build one of these things. It takes a long time: You make one and find out, crap, that didn't work; let's try it again.

SK: What do you mean by long time?

RL: Several months.

SK: And what about the glassblowing? Does that require someone with specialized skill?

RL: Yes, you have to have people who are very good at it, and at Oak Ridge, they certainly do. Still, no matter how careful the glassblower is, each test section will come out differently. It's very sensitive.

When we did it at Oak Ridge, we were just happy if we could get results that were repeatable for us and that agreed with our analysis. We weren't thinking about everybody and his brother wanting to reproduce it. And that's a problem, because now, when people want to reproduce it, it's a very touchy thing.

Even Rusi will tell you, when he builds a test section, he never knows if it's going to work. He may have to build one and then build another until he finally gets one working, and that's not the way these folks like to work, I can appreciate it.

We went through that here at RPI, and it was very frustrating. Frankly, that's why we decided we're not going to work with that design, anymore. We decided to design one that's optimized and robust. Then, if we get good results, we can publish the design drawings at the same time as the data, and anybody who wants to replicate it would be welcome to do so.

SK: I can see that this sort of thing requires a significant amount of teamwork and collaboration between originator and replicator.

RL: In the UCLA case, where they attempted a replication for DARPA, that was the case. We were claiming we had good solid evidence and lots of published papers showing that it works, and they were without the required knowledge of the test section, trying different things and not getting anything.

As I understand it, the agreement that they reached was that Rusi would supply the information on the test section to Putterman, and he would try to get it to work.

What I told Rusi to do - and I wished the hell he had listened to me - is don't do that. Just build one, keep building them until you get one that works, then hand-carry it out to him and show him how to set it up and use it, because, otherwise, chances are it may not work.

I wish we had done that, because the obvious temptation is for somebody to say, "I didn't get the results, so it can't be real." It's a tough situation. You know the saying, "Lots of people can build a violin, but only a few can build a Stradivarius?"

SK: Yes, I understand.

RL: So a lot of this has gotten very nasty for all the wrong reasons, in my view. Now Congress is involved. It's just a mess.

There's two real undercurrents. One is our competitors, the Putterman group. They really got cross-wired, in particular with Rusi. They just can't stand each other's company, and that's too bad That's not science.

The more fundamental thing: Right from the start, the fusion community just hates this technology because they got burned big time with cold fusion. They're viewing this as "Oh, my God, it's going to happen again, and Congress is going to transfer the funding into this stuff." So they did their best; they did a pretty good job at trashing it.

Back to UCLA: Honestly I don't think these folks knew what they were getting into. They're so used to sonoluminescence, where almost anything works. Even if you look at some of the stuff they did in their experiment, they were trying to enhance it to get brighter light flashes. They're doing all the wrong things, like putting noncondensable gas in there to do that, and that's exactly what kills sonofusion. Anytime you have that stuff in there, that's what gives you what Rusi calls the comets, and there's no way you're ever going to get sonofusion.

SK: Would they have known that?

RL: If they read our paper, they would have known it. That's something that we pointed out very clearly.

SK: About the gas?

RL: Yes! That was the key difference between sonoluminescence and sonofusion and how you run the experiments, and we documented that in a number of places. We wrote a book on the differences between sonoluminescence and sonofusion results and experiments and provided answers to many of the criticisms that have been raised concerning sonofusion.[7]

So it wasn't only the test section; it was also the test technique. Unfortunately, they apparently didn't learn by their mistakes, I guess they just decided it didn't work. A lot of money was thrown down the drain.

The sad thing is that they are viewed as the gurus, and if they can't make it work, then it must not be real. I can tell you that it is very easy to run a bad experiment but really hard to run a good one. No great trick is required to run a bad experiment.

SK: So what work are you doing with this now?

RL: We have a sonofusion experiment under way, and I'm working with the Germans. They're funding this stuff. The well is pretty much poisoned here in the U.S. in terms of funding, but it's not the case in Europe. They think this is interesting stuff, and they're moving ahead.

SK: You were Rusi's mentor if I recall, right?

RL: Yes, and I have a very high regard for him, but I feel very sorry for the treatment he's gotten. His career has been hurt bad. I sort of interested him in this technology a long time ago. I wish the heck I had not done that, because he's in the center of a storm right now, and I don't know how to extricate him from it. Even Congress is involved now. Hopefully, if they take his testimony, some rational picture will appear.

CHAPTER SEVEN

The Reich Articles

> "Few could have predicted that this collaboration would end in such disarray."
>
> -Eugenie Samuel Reich, *Nature*, July 20, 2006

On March 8, *Nature* published a set of articles written by Eugenie Samuel Reich.

Their titles are:

- Bubble Bursts for Table-Top Fusion: Data Analysis Calls Bubble Fusion Into Question
- Is Bubble Fusion Simply Hot Air? Concerns Gather Momentum Over Claims for Table-Top Energy Production
- A Sound Investment? Rejection Leaves Bubble-Fusion Patent High and Dry
- Bubble Fusion: Silencing the Hype: *Nature* Reveals Serious Doubts Over Claims for Fusion in Collapsing Bubbles

As the tone of the headlines indicates, the articles convey the idea that bubble/sonofusion is bogus, perhaps fraudulent. The title "Silencing the Hype" implies suppression.

The foundation of the Reich articles was the speculative and now-debunked accusation of californium-spiking by UCLA, as discussed earlier in this report.

Philip Campbell, editor-in-chief of *Nature*, and editor Jo Marchant will have to decide how to repair the damage. Cambridge theoretical physicist and Nobel prize winner Brian Josephson has made significant efforts in the year since publication of the articles to apprise both editors of the problems with the Reich articles and to suggest options.

In addition to the charges from UCLA, the Reich articles pursued two other angles that invalidated Taleyarkhan's work. The first showed that the patent office was rejecting sonofusion-based patent applications.

Reich states that patent examiner Ricardo Palabrica "notes in his rejection [of a related patent application] that the patent office is aware that the field of fusion research has been particularly prone to erroneous claims" as a significant reason for rejecting the application.

If Palabrica were challenged to back this comment with facts from the scientific literature and examined such literature carefully, he might find that the only erroneous claims are that the underlying mechanism responsible for the possible production of neutrons and tritium in bubble/sonofusion remains speculative, and that calling it "fusion" may be a semantic error.

The third negative angle of the Reich articles drew on a brewing animosity within the Purdue faculty. If Reich wanted dirt, she didn't have to look hard.

The Tsoukalas-Taleyarkhan Conflict

Several faculty members in the Purdue University School of Nuclear Engineering provided Reich with multiple slanderous and defamatory comments on Taleyarkhan's character, which she reproduced in her articles. These faculty members included Lefteri Tsoukalas, Tatjana Jevremovic and "several others who do not wish to be named." The introduction of unnamed accusers is unfortunate, because it gives the appearance of persecution.

Many parties to this conflict, including Taleyarkhan, Tsoukalas and the university, are now represented by legal counsel, and a significant amount of communication from these parties is therefore now controlled by the lawyers.

Tsoukalas has remained publicly active in the continuing saga since March 2006. Jevremovic dropped out of sight after the *Nature* articles and reportedly refused to answer questions from the press.

The Tsoukalas story started several years earlier.

Tsoukalas, then head of the School of Nuclear Engineering, had hired Taleyarkhan, with the expectation that he would help him develop a bubble/sonofusion program at Purdue.

Taleyarkhan begins his account of the related circumstances by showing a photo of a wall in the Purdue laboratory taken on Sept. 19, 2003 showing the phrase "Bubble fussion [sic] was achieved [here]." Taleyarkhan states that the word "here" was cut off in the photograph. Signatures of Taleyarkhan, Tsoukalas and Jeremovic follow.[1]

Erik Vance, writing for *The Chronicle of Higher Education*, erroneously reported this phrase as "Here we created bubble fusion," and said merely that "someone"

had scrawled their names on the wall. The people who signed it are not just "someone." One is Tsoukalas, one of the lead characters in this controversy, who claimed to have achieved bubble fusion in 2003. He and colleague Jeremovic defamed bubble fusion and Taleyarkhan in the Reich articles in 2006.[2]

Tsoukalas led a group performing bubble/sonofusion research at Purdue starting in late 2003, with Taleyarkhan's assistance.

On May 14, 2004, Tsoukalas sent an e-mail to Taleyarkhan[3] telling him of the findings of positive tritium results by Frank Clikeman, a member of Tsoukalas' group:

> Subject: urgent
> From: "Lefteri H. Tsoukalas" <tsoukala@ecn.purdue.edu>
> Date: Fri, 14 May 2004 13:51:51 -0500
> To: Rusi <Rusi@ecn.purdue.edu>, Rusi Taleyarkhan <taleyarkharp@ornl.gov>
>
> Rusi,
>
> Are you in a number I can call you to chat a bit.
> Please go ahead with the Smalley idea, it is great! Let's coordinate with Jay and Linda.
> Also, we have some major developments in Frank's work. Good news!!
>
> Lefteri

ANALYSIS BY L.TSOUKALAS GROUP MEMBER (F.Clikeman) --> Transmitted by Tsoukalas to Taleyarkhan (5.14.04)

	Beckman Data		Beckman Data with Ultima Gold Stds.		
	DPM	± σ	DPM	± σ	NOTES
D-Acetone 7 hr run 9/19/03	3.50	2.7	2.16	3.36	
D-Acetone 7 hr run 9/24/03	3.60	2.3	4.13	2.53	
D-Acetone 7 hr run 9/26/03	0.05	2.8	-0.22	3.12	
D-Acetone 7 hr run 10/3/03	3.50	2.6	3.76	2.90	
Average of 1Ci Runs	**2.66**	**1.3**	**2.46**	**1.5**	**Positive tritium results** 1Ci test results
D-Acetone 7 hr Run 10/8/03	3.50	2.5	3.94	2.76	
D-Acetone 7 hr run 10/27/03	0.54	2.6	0.60	2.90	
D-Acetone 7 hr run 10/31/03	-0.16	2.1	1.50	2.75	
D-Acetone 7 hr run 11/3/03	-1.60	2.4	-1.55	2.68	
D-Acetone 7 hr run 11/7/03	2.00	3.6	2.47	4.00	
D-Acetone 7 hr Run 11/14/03	-1.60	2.9	-2.05	3.19	
Average D-Acetone	**1.45**	**0.8**	**1.56**	**0.96**	**Positive tritium results** Overall for 1Ci and 10 Ci tests
N-Acetone 7 hr Run 9/18/03	1.17	1.9	0.83	2.23	
N-Acetone 7 hr Run 10/15/03	-2.09	1.6	-1.82	1.76	
N-Acetone 7 hr Run 10/18/03	-2.16	1.8	-2.86	1.88	
Average N-Acetone	**-1.03**	**1.0**	**-1.28**	**1.14**	**Null (negative) results for Control Expts.**

The data from Clikeman's spreadsheet say "Positive tritium results."

Any evidence of tritium at significant levels above background is indisputable proof of a nuclear reaction.

On Jan. 19, 2005, about one month before the BBC bubble fusion broadcast, Tsoukalas sent an e-mail to the BBC[4] that shows obvious enthusiasm and pride: "Two years ago, a Purdue group under my guidance and with the assistance of Dr. Taleyarkhan initiated a series of scoping experiments to look for tritium production. The work was, therefore, not truly ‘independent’ (since assistance from Dr. Taleyarkhan included design details and setup of the test cells used in the experiments) although operation and data gathering was conducted independently.

"I could share that our raw unpublished results look promising and encouraging. ... Statistically significant tritium increase appears to be realized from neutron-seeded cavitation tests with chilled deuterated acetone, whereas corresponding tests with normal acetone and those with irradiation alone gave null results."

When quoted in the press at later dates, however, Tsoukalas tells a different story.

> "Tsoukalas said Taleyarkhan claimed in 2005 that Tsoukalas' team had confirmed the fusion findings. In fact, Tsoukalas said, his team had failed to replicate the results."
>
> - *Chicago Tribune*, Feb. 9, 2007[5,6]

> "'Initially we thought, "Well, maybe he did it once and couldn't do it again,"' says Lefteri H. Tsoukalas, head of the School of Nuclear Engineering, where Mr. Taleyarkhan worked at the time. 'But he was very adamant. "We do it all the time. We just press the button and do it." Then we thought maybe it was just sloppy work. He got carried away; you know, sometimes people talk themselves into something.'"
>
> - *Chronicle of Higher Education*, April 2, 2007[7]

The omission of the fact that Tsoukalas was directly involved in and supportive of bubble/sonofusion research at one point is significant. Were he to have revealed in 2007 that he once was a strong supporter of bubble/sonofusion – a claimant no less – a different picture than the one he had been telling the press would have emerged much sooner.

Taleyarkhan says that Tsoukalas removed positive tritium results from the paper published in *Nuclear Technology.* Taleyarkhan provided *New Energy Times* with an early draft of the Tsoukalas paper, which listed Purdue student Anton Bougaev as the lead author rather than Tsoukalas.[8]

The earlier draft concludes with strongly positive findings: "Tritium measurements conducted in controlled experiments to investigate nuclear emissions during acoustic cavitation of the organic deuterated fluid -- reported by Taleyarkhan et al., in *Science*, 8 March, 2002, -- provide positive evidence for excess tritium produced (and attributed to D-D fusion). ... The results point to statistically observable tritium increases in post-cavitation deuterated acetone samples, suggesting the possibility of D-D fusion taking place. Samples of normal acetone and deuterated acetone not subjected to cavitation do not show statistically observable changes in tritium."

This contrasts with the related text in the published paper, which summarizes the findings as follows: "An attempt to reproduce the tritium measurements in an acoustic cavitation experiment with deuterated acetone has shown no evidence of tritium production attributed in D-D fusion."

New Energy Times asked a panel of experts to review and analyze both papers, especially the tritium graphs, to provide a possible explanation for the 180-degree change in the paper's conclusion.

Based on the information available in the two documents, all three of the reviewers found sufficient justification for the Tsoukalas group to restate its findings as null rather than positive, though some uncertainties remained.

In general, the reviewers concluded that the measurement error is too large for a positive determination.

One reviewer noted that Revankar is not included on the final paper and that "Bougaev, the original writer of the 2004 version, had significant insights and appeared deeply involved in the work. His name was lowered from first to almost last, and some of his insights were removed."

Another reviewer commented: "This paper in both its draft and final forms is concerned with the search for evidence of phenomena at the very threshold of detectability, where post-experimental data correction and manipulation is used in an attempt to lift a very weak signal out of the noise floor.

"So small is the effect being sought that the values assigned to chemical quenching in the scintillation cocktail, optical quenching in the photomultiplier, and artifacts such as chemiluminescence can swamp the data and cast uncertainty as to whether or not a phenomenon appears to the investigator as a discrete and definite entity independent from noise.

"The authors' apparent change of heart may have come about during the process, when they decided what values to assign to variables such as the quenching index parameter and the overall counting efficiency, when we would expect most investigators to err on the side of caution.

"The paper is not a refutation but a failure to replicate and, rather than being any sort of evidence of fraud or bad faith by anyone, is testimony to the great difficulties faced by scientists working with phenomena at the very threshold of detectability. Over time, such thresholds almost always fall as knowledge and equipment improves, so no prudent investigator would assume the Tsoukalas group's failure to replicate is anything like the final word.

“As far as the tritium graph, they clearly believed they had a signal when they wrote the early draft.

“However, when it came to working up the data, an apparent policy decision was made to use the most conservative values and metrology techniques for corrections, adjustments and calibrations, so the team would either report a clear

and unambiguous signal which others could replicate, or they would not. All those involved would have been very much aware of the critical scrutiny which would be brought to bear on their work if they reported a positive finding, so it is not unreasonable for them to have adopted a conservative approach to their working up of the data."

A third reviewer was concerned that there may have been "something fishy." He wrote that he did not "think all of the positive effect was eliminated," but without more of the original data, he was unable to make a clear determination.

Perhaps the fishy odor emanates from the fact that the Tsoukalas paper would have been more honest if it had stated that the experiment produced data at the threshold of their detection limit, rather than "no evidence of tritium production."

Taleyarkhan suggested that human and political factors had much to do with Tsoukalas' previous enthusiasm for bubble/sonofusion and the apparently cordial relationship between him and Taleyarkhan before 2005.

"From July 2005 onwards," Taleyarkhan wrote, “very serious and deep divisions were erupting within the School of Nuclear Engineering, mainly over egregious improprieties associated with promotion and tenure. I voted my conscience, and that did not go well with some of the others. I then was subjected to a campaign of vendetta targeted against me.

"Around that same timeframe, a very serious grievance charge was filed against a senior faculty member for gross improprieties by a junior faculty member whose promotion/tenure/existence at Purdue was wrongfully affected by actions of a member of the Purdue faculty, and I was asked to provide input, which I did along with various other faculty.“

New Energy Times contacted the junior faculty member.

"I prefer not to be quoted in any more press stories," he said, "but I am willing to confirm that I filed a grievance. That process is confidential, and I do not want to compromise the confidentiality of the various players in the process."

"Quite a few of these inputs were negative on Tsoukalas," Taleyarkhan wrote. "Upon seeing the inputs, this violently inflamed the situation and advancement of harassment tactics toward me by Tsoukalas and his group."

CHAPTER EIGHT

Tsoukalas Runs Afoul of Purdue Policy

Taleyarkhan believes that Reich was secretly interviewing Tsoukalas and Jevremovic in the days and weeks before the Reich articles. Taleyarkhan thinks that the two faculty members violated the laws of Indiana governing academic conduct by taking their allegations of research misconduct directly to the press rather than first going through administrative channels.

Tsoukalas formed his own "fact-finding" committee, as he called it, on Feb. 7, 2006, asking Chan K. Choi to be the chair and suggesting Franklyn M. Clickeman and Karl O. Ott for membership.[1]

These three committee members delivered their signed report two weeks later. In their report, they stated that they held several, apparently secret meetings unknown to any of the Purdue administration.

On Feb. 17, the three met with Purdue student Adam Butt.[2]

On Feb. 22, the committee met with Butt and co-author Yiban Xu. They kept no minutes, and apparently no third-party witnesses or counsel to represent any party attended.

The third co-author, according to Taleyarkhan, Shripad T. Revankar, refused to participate, recognizing the illegitimacy of the proceedings.

Butt provided testimony for Tsoukalas' committee, though his testimony raises numerous questions, not the least of which is about possible duress and intimidation. Xu appears to have provided some testimony, as well, though source documentation for this has not entered the public domain.

Taleyarkhan described what happened the day the Reich articles published.

"On March 8, 2006," Taleyarkhan wrote, "I was requested to participate in a meeting with Provost Sally Mason and several others. Mason told me of her desire, per Purdue policy, that an inquiry should be started by the research office and I offered my full cooperation but also offered my surprise since I mentioned that Tsoukalas had already started his investigation committee. Mason appeared

surprised and asked around the room if anyone knew of this. Assistant Dean [Edgar J.] Martinez, who was in the room, reacted with total surprise, and so did all the others.

"Mason called the dean (Linda Katehi) on the phone and told her to call Tsoukalas and give him a piece of her mind to let him know that she cannot tolerate a Purdue administrator going to the press like this but also told Katehi to not put anything in writing via e-mail but to talk directly with Tsoukalas. The Tsoukalas committee was disbanded summarily after that day and a formal one started."

Mason was asked to comment on Taleyarkhan's account and wrote that she has "no memory of instructing the dean to convey anything to Dr. Tsoukalas other than the need to follow policy, which does include maintaining confidentiality."

Taleyarkhan wrote that, soon after that, a series of harassment/discrimination actions took place, though he did not provide specific details to *New Energy Times*.

"In several instances," Taleyarkhan wrote, "I had to appeal to the provost and dean to have Tsoukalas' actions reversed, but his harassment/discrimination was allowed to continue. Finally, the new dean, [Leah] Jamieson, decided that I would report directly to the dean and not to Tsoukalas.

"Soon after March 2006, nearly half the faculty of the School of Nuclear Engineering left/resigned/retired from Purdue out of disgust. (Karen Vierow, Sean McDeavitt, Tom Downar, Al Solomon. Also affected unduly was [one other professor])."

Later that year, on Oct. 16, word came to *New Energy Times* that Tsoukalas had been removed as the head of the School of Nuclear Engineering. *New Energy Times* called Purdue spokeswoman Jeanne Norberg, who was unable to confirm that Tsoukalas had been removed. Instead, she forwarded the official statement from Jamieson, Purdue dean of engineering:

"Lefteri Tsoukalas has submitted his resignation, effective immediately, as Head of the School of Nuclear Engineering to me, and I have accepted his resignation.

“I want to thank him for his almost five years of service to the School and to the College of Engineering at Purdue. The School has seen many positive changes under his leadership.

“Professor Vince Bralts, former Head of the School of Agricultural and Biological Engineering and Associate Dean for Resource Planning and Management, will serve as Interim Head until a head search can be completed. Provost Mason has confirmed his appointment.

“We will initiate the head search at an appropriate time in the future. It is my hope that we will be able to complete the search during the Spring 2007 semester.

“In my meeting this morning with the Nuclear Engineering faculty and staff, I also reaffirmed my support for our School of Nuclear Engineering. Especially in today's world, this is an important school for Purdue's College of Engineering, and I am committed to its long-term health and preeminence."

The next day, Jamieson sent an e-mail to Reich in response to the rumor that Tsoukalas had been removed.

"The statement that you received from Jeanne Norberg," Jamieson wrote, "is the accurate statement, and it is what I presented to the faculty and staff in the School of Nuclear Engineering Monday morning."

New Energy Times asked Joseph L. Bennett, vice president for university relations, whether Tsoukalas had been removed or whether he resigned voluntarily.

"The circumstance of Dr. Tsoukalas' resignation is a confidential personnel matter," Bennett wrote.

On May 10, details of a congressional report dated May 7 were released by a representative, referencing an April 12, 2007, letter from Purdue president Martin Jischke to Congress.

On Page 2, in Paragraph 4, the report says that Jischke's letter discusses "the removal of the head of the school who initially brought the allegations to the University's attention."

> "Eventually, Mr. Tsoukalas says, he had to act on his doubts. After stepping down last year as head of the school, he began talking to the news media, taking the critics' side and demanding that the university investigate Mr. Taleyarkhan for research misconduct."
>
> - *Chronicle of Higher Education,* April 2, 2007[3]

> "Tsoukalas said he resigned in October as head of the nuclear engineering school so he could speak freely about his concerns over the research."
>
> - *Chicago Tribune*, February 9, 2007[4]

> “Mr. Tsoukalas says he wanted the experiments to work. ‘I would be the happiest person in the world if this was true because I worked very hard to bring this guy here,’ he says. ‘I cannot tell you how much time I spent trying to accommodate and make Rusi succeed. But I wouldn't go as far as covering up fraud.’"
>
> -- *Chronicle of Higher Education,* April 2, 2007[5]

Building on the aspersions Tsoukalas had cast in the March 8, 2006, Reich articles, he continued making defamatory comments about Taleyarkhan as conveyed by the news outlets quoted above and to Chang of *The New York Times*.

On May 11, 2006, the *Times* published a July 2, 2006, letter from Tsoukalas that contradicts his earlier proud proclamations of bubble/sonofusion success.[6]

"Our own research (see attached paper)," Tsoukalas wrote, "and that of others raises serious questions about what was put in the peer-reviewed prestigious journals."

Tsoukalas says in the *Times*, "What is very important, crucial I would say, is that he faked replication. And this he did here at Purdue, and Purdue can be harmed because of this.”

Tsoukalas' aspersion comes devoid of specifics or substance. That *The New York Times* published such a letter is surprising. No evidence is known to *New Energy Times* to suggest that the replication by Xu/Butt was "faked" or even that it failed. The independence of the replication reasonably could be questioned, but that is a different matter.

Mason replied to Tsoukalas on July 9, 2006, and implored him to work within the university's policies: "We're entering the stage of formal investigation into this work as outlined under executive memorandum C-22. It's extremely important that we follow our procedures to a T. This will make the reporters very unhappy because we guarantee confidentiality while the investigation is ongoing.

“However, we will get to the bottom of what has happened. My hope is that we will also demonstrate the integrity of our institution. For integrity to be maintained, we need to allow the process to work. I know the committee will be contacting you at some point for input. You can be most helpful by sharing with them, and only with them, what you understand to be the facts of this case. I will continue to withhold any judgment until the committee is prepared to issue a report."

When the letter from Tsoukalas to Mason and her reply were published in the *Times* on May 11, 2006, they included the note: "The e-mail was provided to *The Times* by Dr. Tsoukalas."

Another highly destructive confidential document was provided to, and published by, *The New York Times* on May 11, 2006. This was the Feb. 23, 2006, "Statement from Adam Butt." Unlike the letter exchange between Tsoukalas and Mason, this downloadable file on *The New York Times'* Web site did not specify the source.

New Energy Times previously attempted to obtain this document from multiple sources, including a related congressional investigative committee and the Purdue administration. None would release it.

Knowing that Chang had obtained other confidential documents from Tsoukalas, we asked him whether he had obtained it from Tsoukalas. Chang declined to confirm or deny.

Robert Service quoted Tsoukalas in *ScienceNow* on Feb. 7, 2007: “'It's outrageous. I don't know what Purdue is doing,' says Lefteri Tsoukalas, a nuclear engineer at Purdue who initiated some of the allegations against Taleyarkhan. Tsoukalas said he spoke to the original review committee last spring and was asked to resubmit written allegations in September. Yet, Tsoukalas says, neither he nor anyone else he knows who has been involved in the case was ever interviewed by the latest panel."[7]

A quick read of a few highlights of Purdue University Office of the President Executive Memorandum C-22 suggests that, in fact, Purdue does know what it is doing.[8]

A few excerpts from C-22: "Personal relations may make objectivity difficult or impossible. Therefore, anyone appointed to conduct inquiries or investigations

related to allegations of research misconduct must be ... free of real or apparent conflicts of interest."

"The mere suspicion or allegation of wrongdoing, even if totally unjustified, is potentially damaging to a person's career. Consequently, no information about charges of a lack of integrity in research may be disclosed except to the appropriate university and federal authorities."

"The individual against whom an allegation has been raised must be afforded the rights of due process, which in this context shall mean being fully informed of all allegations."

CHAPTER NINE

Science Propaganda

pro·pa·gan·da

- the spreading of ideas, information, or rumor for the purpose of helping or injuring an institution, a cause, or a person

- ideas, facts, or allegations spread deliberately to further one's cause or to damage an opposing cause; *also*: a public action having such an effect

-- Merriam-Webster

Suslick and Putterman have been unrelenting in their critique of Taleyarkhan over the last two years.

A significant part of this attention comes from Suslick. A Feb. 9, 2007, *Chicago Tribune* article reports, "Neither Suslick nor other outside experts have directly accused Taleyarkhan of fraud."

However, Suslick not only has disputed Taleyarkhan's claim that his students performed an "independent" replication but also has said that something more sinister and substantial is afoot.[1]

Suslick has not, to the knowledge of *New Energy Times*, published his evidence for Taleyarkhan's error in the scientific literature, as Putterman and Naranjo had attempted to do with their debunked californium-252 speculation.

Primary scientific adjudication belongs to the science journals, not the science press. The journal editors and their referees - not reporters, not members of Congress and not university administrators - are entrusted with passing judgment on science.

If an erroneous paper has entered the scientific literature, the initial jurisdiction should be with the original journal or a competing journal.

Once a journal publishes or rejects a submitted scientific paper, then the matter is fair game for the science reporters, but not before.

Why has Suslick been unable to demonstrate to the scientific community evidence that his concerns are valid? (Suslick has declined repeatedly to speak with *New Energy Times*.)

> "Last June, Dr. Suslick sent an e-mail message to Peter E. Dunn, Purdue's associate vice president for research, saying he believed that Dr. Taleyarkhan's work might be fraudulent. Dr. Suslick made his accusations based on what he saw during a visit to Dr. Taleyarkhan's laboratory in March last year as part of a review of a Defense Department grant that Dr. Taleyarkhan was receiving for fusion research."
>
> -- *The New York Times*, Feb. 13, 2007[2]

A visit to Taleyarkhan's laboratory? Yes, the very same visit witnessed by a dozen observers, including one who signed an affidavit testifying to positive results.

In a manner reminiscent of Sen. Joseph McCarthy's House Un-American Activities Committee, the content of Suslick's e-mail -- so far kept secret from the public, the press and Taleyarkhan -- has been mentioned as credible and significant evidence in public news stories and by a congressional investigation.

The only hint of the content of this e-mail comes from Reich in a Feb. 13, 2007, article in *Nature*[3]: "Suslick is one of several researchers worried that Taleyarkhan's work may be fraudulent, and he wrote to Purdue about his concerns in June 2006. These include the apparent duplication of data between reports of supposedly independent experiments (first raised by *Nature*) and a report that the spectrum of neutrons that Taleyarkhan claims to have detected from bubble fusion exactly matches that of a standard radioactive source called californium."

The matter of californium has been debunked in this report.

The second claim is perplexing. According to Reich, when data from one of Taleyarkhan's replicators look identical to his original data, it means that the replicator faked the data and borrowed it from Taleyarkhan. But when Naranjo's californium-252 data looked like Taleyarkhan's data, Reich suggests it means that Taleyarkhan faked his data.

Suslick has suggested that "Taleyarkhan just hasn't done enough" to prove his experiment and that he and Putterman have tried, precisely repeated Taleyarkhan's experiment, and failed.[4,5]

In her Feb. 13, 2007. article for *Nature*, Reich writes: "Seth Putterman of the University of California, Los Angeles, who has also been trying to replicate bubble fusion, thinks that Taleyarkhan's work is invalid. 'Purdue's defence of Taleyarkhan's approach to scientific research taints their reputation,' he says. 'If Purdue were interested in maintaining their credibility, they should have appointed external members to their panel.' [Purdue provost Sally] Mason, who is ultimately responsible for academic affairs at Purdue, did not respond to *Nature*'s requests for comment.[6]

Nature Publishes Further Misleading Reports

After Reich's March 8, 2006, articles on Taleyarkhan and bubble/sonofusion, Reich failed to let up on her quest to find fraud and wrote several more articles.

The most striking aspect of these articles as well as her initial four-part story is what she has neglected to tell the reader.

Reich identifies Putterman as "one of the most prominent critics." She identifies Suslick as a chemist "who has been attempting to replicate Taleyarkhan's claims."

Putterman and Suslick are not just scientists who have "been attempting to replicate Taleyarkhan's claims." They are scientists who have been attempting to stake their own claim in the bubble/sonofusion turf. This is not a trivial point.

What's more, Putterman and Suslick are proponents of their own competing bubble/sonofusion method.

In not one of her articles to date has Reich informed the public of the underlying conflict of interest, putting Reich in the precarious position of omitting key facts and appearing, at best, biased and, at worst, guilty of conspiracy.

Taleyarkhan got wind of Reich's next article-in-progress on June 30, 2006. Because his trust in Reich had been compromised, he sent *Nature* editor Jo Marchant a letter requesting that she confirm whether *Nature*, in fact, had commissioned Reich to do another story on Taleyarkhan and his work.[7]

"Eugenie is an experienced and well-respected news reporter," Marchant replied, "and by approaching you as part of her research for this story, she is abiding by the conventions of good journalism. I believe that she will do a thorough and objective job, and I would appreciate it very much if you could cooperate with her, so that we can include your side of the story. Please rest assured that her aim and mine is to produce a balanced and accurate news article."

On July 4, Taleyarkhan informed *New Energy Times* of the new angle Reich was going after: speculations that DARPA funds had been wasted on "disputed work" and that accounting improprieties had taken place at Purdue.

"I have just learned that Putterman has been trying a different tack to discredit our team," Taleyarkhan wrote to *New Energy Times*. "This time, he alleges research wrongdoing: that my team did not acknowledge DARPA as having funded my work reported in January 2006 in *Physics Review Letters*. He has been helping Eugenie Reich to now write another news story in *Nature* that might be appearing in the coming week. The charge is totally off the wall and based on naive wishful accounting and perhaps desperation. Not getting anywhere in the technical arena, this group is resorting to absurdity."

Josephson had been watching the evolution of this controversy and later placed the following text on his Web site.[8]

"...Putterman's belief that Rusi Taleyarkhan, leader of the group that developed the bubble fusion process, used DARPA funding for an experiment for which the use of that funding had not been approved."

On Aug. 14, 2006, Putterman took exception to Josephson's posting: " I don't believe that I ever made such a claim," Putterman wrote. "Could you please cite chapter line and verse from the *Nature* article where the quoted claim is made?

"Surely, I never intended to make such a claim. In fact, I fully believe, and have always believed, that use of DARPA money for Taleyarkhan's published work on bubble fusion was an appropriate use of that money. This includes the January *Physics Review Letters* [paper]. My concern is that Rusi failed to simply thank DARPA for the largesse which made the January *Physics Review Letters* [paper] possible."

New Energy Times asked Taleyarkhan how he knew Putterman was behind the effort. He provided the following e-mail from Reich:

> Fri, 30 Jun 2006 11:46:02 -0700 (PDT)
> From: "Eugenie Reich" <eugenie.reich@gmail.com>
> To: "'Rusi Taleyarkhan'" <rusi@ecn.purdue.edu>
> Subject: request for comment
> Date: Fri, 30 Jun 2006 14:46:06 -0400
>
> Dear Professor Taleyarkhan,
>
> The following points that may be covered in a story I will send to *Nature*. I am requesting comment on all these points but will take in all, none, or one as you prefer. By the end of Wednesday I request a response to guarantee inclusion.

The PI [Principal Investigator, (Putterman)] on the 2005 DARPA grant where you are a subcontractor says based on a review of the books using information provided in February by Purdue, you have used DARPA money to fund the January 2006 PRL paper.

This information is based on payment of your salary by the grant during the months leading up to paper submission, as well as part-payment of the salary of your post-doc, and transfer of your money to collaborators like your former collaborator at ORNL credited with cross-checks.

-What is your position on this?
-DARPA is not acknowledged in the PRL paper. Was this deliberate or accidental?
-Have you sent an erratum to PRL requesting acknowledgment of DARPA or do you intend to?
-Do you have any comment on the issue I followed up with Professor Lahey recently, whereby identical measurements appear in your MST paper, your DARPA 2005 slideshow, as well as Yiban Xu's NED and Nureth-11 papers.
-Are these the same data duplicated?
-Are they the results of different experiments that happen to look the same, as Professor Lahey suggests?

I am sure you are aware of this issue and appreciate clarification.

The *Nature* editor can be reached at j.marchant@nature.com and her name is Jo Marchant.

Best Wishes,

Eugenie

Eugenie Samuel Reich
phone: +1 617 354 0329
cell: +1 617 821 1538
Articles and Resume

New Energy Times asked Taleyarkhan to respond to Reich's insinuation of improperly failing to acknowledge the source of DARPA funding. He explained the accounting with regard to his time spent on the research. From the information Taleyarkhan provided, he has a suitable defense against Reich's allegation.

However, nothing short of a full audit by DARPA or its related organization, the Office of Naval Research, likely will provide a reliable answer to this question.

One question that is unanswered in Reich's article is a possible motive for Taleyarkhan to intentionally neglect to acknowledge DARPA's support. Taleyarkhan wrote that he "warmly acknowledged" DARPA in his previous DARPA-funded work.

Taleyarkhan's side of the story was not well-represented in *Nature*'s article.

The disinformation contained in the Reich article also perpetuates the mythology of the Naranjo-Putterman californium claim as fact:

"It was clear things weren't going well when concerns about the validity of Taleyarkhan's bubble-fusion work were reported on 8 March," Reich wrote. "These included an analysis by Putterman's postdoctoral student, Brian Naranjo, showing that the neutrons described in Taleyarkhan's latest paper, published in *Physical Review Letters* on Jan. 4, came not from fusion as claimed but from the radioactive decay of standard lab material."[9]

The Naranjo hypothesis was debunked by Taleyarkhan a few months later, on Oct. 3, 2006, in the same issue as the Naranjo paper. At the time of this July 20, 2006, article by Reich, Naranjo's paper had not been accepted; that wouldn't occur until Sept. 6.

Still on the hunt, Reich continued her fraud investigation in December, as if Taleyarkhan's response to Naranjo did not exist.

She sent an e-mail to Taleyarkhan on Sunday, Dec. 3, 2006, demanding responses to extremely serious allegations by 5 p.m. on Monday.[10]

Reich's next article, published on Dec. 7, 2006, quoted Tsoukalas, who described his continuing "disappointment over the slow pace and secrecy of the university's response" to his complaints.

The article brings in the erroneous Naranjo-Putterman analysis again, despite the fact that this had been debunked two months earlier.

She brings in the charges of Suslick, who Reich says had "sent a confidential note in June to Purdue's associate vice president for research, Peter Dunn, stating that he believed Taleyarkhan's research claims are fraudulent." This is the same Suslick reported by the *Chicago Tribune* who had never "directly accused Taleyarkhan of fraud."[11]

The basis for Suslick's claims, according to Reich, follow:

1. Naranjo’s analysis
2. The fact that other teams were unable to repeat the work
3. Data that were “cherry-picked” by Taleyarkhan.

Claim 1 has been dispensed with.

For claim 2, the facts are thus: The Putterman/Suslick team failed for many reasons, which have been reviewed. The Xu/Butt team at Purdue which performed their laboratory work, though not the report writing, independently of Taleyarkhan succeeded in and published their replication. On this, there is no debate.

The Forringer team succeeded in their replication, though not completely independently, either, because they used Purdue facilities and materials provided by Purdue.[12]

For claim 3, Suslick has not publicly disclosed how much of the data was "cherry-picked" and whether this selection significantly skewed the analysis. If Taleyarkhan truly provided an unbalanced representation of the data, Suslick's claim’s having merit would imply that Xu, Butt, Bugg and Forringer also conspired with Taleyarkhan and cherry-picked data.

In her next article, on Feb. 13, 2007, Reich continued in the same vein.[13]

CHAPTER TEN

The Investigations

> "The line between investigating and persecuting is a very fine one."
>
> -- Edward R. Murrow

Purdue Investigates

Through 2006, two administrative investigations by Purdue officials took place.

To the dismay of many reporters as well as critics of Taleyarkhan and bubble/sonofusion, few details of these investigations have been released publicly.

The general assumption has been that Taleyarkhan and his work were the dominant or exclusive subject of the investigations.

Considering what is now known to readers of this report of the role Taleyarkhan's supervisor and department head played in this drama, this initial perception likely was not completely accurate.

On June 20, 2006, Purdue administrators completed their first investigation and exonerated Taleyarkhan.

On Dec. 15, 2006, Purdue administrators completed their second investigation and again exonerated Taleyarkhan, though they did reprimand him for some indiscretions.

The committee determined that Taleyarkhan had used poor judgment in multiple ways with regard to his involvement with the work and publication of Xu and Butt, which Taleyarkhan claimed was independent.

The committee also affirmed that the Xu/Butt research should not be accepted in the scientific community as an independent replication. The replication never should have been asserted as independent.

In part, Taleyarkhan, and the Purdue administration that wrote the original press release announcing the "independent" replication, painted a target on their back.

A true independent replication should have, at a minimum, been performed elsewhere. It is asking too much of the scientific community to accept as independent a replication performed at the same institution.

Confusing the matter, and most likely frustrating spectators, was the fact that Purdue did not publicly report the completion of this investigation until Feb. 7, 2007. Nor did it release any report or details about the investigation, aside from the press release, leaving spectators speculating and critics complaining.

The Inquiry Committee Final Report was never released publicly; however, it was leaked and published first by the *Journal and Courier* on May 18, 2007. A copy of this report is available on the *New Energy Times* Web site.[1]

Overall, what is stated in the Purdue report seems objective, fair and factual. It absolves Taleyarkhan where it appears appropriate to do so and reprimands him where it appears appropriate, as well. The Purdue report matches the findings of almost all other aspects of the *New Energy Times* investigation.

However, Purdue made a monumental and egregious error by what it neglected to report in its Feb. 7, 2007, press release announcing the completion of the second investigation. Purdue failed to tell the public, the press and, most important, the scientific community that it and Taleyarkhan had retracted their claim that the Xu/Butt replication was performed independently.

This was a major blunder. Because the Purdue administration, not Taleyarkhan, was clearly running the show at this point, it gets a failing grade in its media management for withholding this information and for failing to demonstrate a sufficient duty of care to Taleyarkhan. Had it acknowledged, in a timely manner (not two months after the fact), that the claim of independence was retracted, Taleyarkhan very likely would not have had to endure further investigations.

The Inquiry Committee Final Report states, "Butt was told by Dr. Taleyarkhan, not Dr. Xu, that he would be an author."

A statement prepared by Taleyarkhan for a congressional investigative committee states, "Butt was invited by Dr. Xu to be co-author."

New Energy Times asked Taleyarkhan for comment.

"The inquiry committee final report," Taleyarkhan wrote, "makes a presumptuous statement based on the report of Tsoukalas' illegally set-up committee from which the Butt report was generated. They certainly did not check with me on this."

New Energy Times provided Taleyarkhan's comment to Bennett and asked for a response. Bennett replied that Purdue has no comment on this.

Congress Investigates

The drama does not end with the completion of Purdue's investigation.

On March 22, 2007, Rep. Brad Miller, D-N.C., chairman of the House Committee on Science and Technology's Subcommittee on Investigations and Oversight, announced its own investigation into Purdue's investigation.[2]

In his letter to Jischke the day before, Miller does not state who requested or initiated the investigation.[3]

The letter does refer to Purdue's response to concerns raised by other Purdue professors, Eugenie Reich's articles in *Nature* and Suslick's allegations.

On May 7, Miller's committee completed its report. The report appears to rely heavily on the Reich articles in *Nature*, news articles from *The Chronicle of Higher Education* and other outlets, confidential documents from Purdue's various investigations, Suslick's secret allegations of fraud, Tsoukalas' testimony and the apparent testimony of Purdue student Adam Butt.

In the course of the Miller investigation, Taleyarkhan provided a comprehensive document and evidence package in his own defense. The testimony from Taleyarkhan apparently was largely ignored. Key points from his statement are included below:

> "My group provided a scientific rebuttal to the allegations on fraud related to californium (Cf-252). ... To directly settle matters, we conducted additional experiments, this time actually using the alleged Cf-252 laboratory neutron source, and showed directly that, when one uses the actual instrumentation with all built-in complexities of three-dimensional effects and settings of instruments-cum-data acquisition trains, there is no agreement either in terms of spectrum shape nor in terms of intensity. This is true for the neutron spectrum, but more importantly for the gamma ray emissions where there is an absolute reversal of data (that is, between the alleged Cf-252 laboratory source and that resulting from bubble fusion as published by our group in 1/06 in the *Phys. Rev. Ltrs.* Journal).
>
> …
>
> "Much ado is being made of the student Adam Butt's name being together on the Xu et al. NED (2005) manuscript. As explained to Purdue University, Mr. Butt had approached me wishing to perform graduate research in sonofusion. He was first advised to *learn by*

doing and to start to work with Dr. Xu, with the goal to understand the science and review and audit the experimental work of Dr. Xu. By mutual consent, Dr. Xu agreed to do this, which was also a prudent measure for enabling due diligence. I did not interfere with this aspect and the extent of reviews and audits conducted by Butt. Butt was invited by Dr. Xu to be co-author, and he happily accepted, providing his comments and corrections to the draft prepared by Dr. Xu, posing thereafter for photographs for Purdue's July 12, 2007, *Press Release*, and responding to queries. Mr. Butt played a role in terms of reviewing the actual data analyses, and as such it was Dr. Xu's decision to have him as co-author. I had nothing to gain by having a totally unknown student with no credibility in the field as co-author on Dr. Xu's manuscript.

…

"A charge was made in the March 8, 2006, *Nature* article by Tsoukalas alleging that I had taken away Tsoukalas' equipment. This is a particularly damaging charge to my standing in the scientific world, since it attacks my personal character — effectively alleging that I stole and diverted Tsoukalas' property. This charge has gained particular notoriety because it comes from my own supervisor. In actuality, this is a reversal of fact, as documented and accepted in writing by Tsoukalas in an e-mail dated July 2, 2004. This e-mail exchange documents that the equipment was moved with Tsoukalas' acceptance, direction, gratitude and willingness and with significant effort on my part. I had offered in writing to move the equipment back, but the offer was not accepted by Tsoukalas, who instead offered his gratitude for my actions.[4]

…

"To summarize, despite the tireless unwarranted assaults suffered by me, I have abided by the time-honored scientific traditions of methodical conduct and reporting of scientific research. Importantly, the phenomenon of sonofusion has now been replicated and reported on publicly several times by groups other than mine (which announced the discovery in 2002). These unaffiliated independent groups of researchers conducting their own measurements have replicated my previously published results. Any and all other allegations of research misconduct brought to the attention of Purdue per EM C-22 guidelines have been systematically reviewed by Purdue University using its due-process approach. The review results have absolved me of alleged research misconduct as cited in their Feb. 7, 2006, press release. I humbly request that the committee recognize the facts of this case and help direct the

closure of this chapter so that I can get back to putting my life back together."[5]

Based on the appearance of the congressional staff report, the committee looked at everything that was presented, saw relative coherence in most of the data from everyone else and the news media, and wrote its report on that basis.

However, the committee's work resulted in no further sanctions or penalties for either Purdue or Taleyarkhan and merely concluded that Purdue should conduct yet another internal investigation, this time with new committee members.

The Miller report makes numerous references to and re-propagates erroneous facts and invalid assumptions clarified earlier in this report. There is no need to belabor these points further.

How the Miller report became public is interesting, however.

On May 10, Taleyarkhan alerted *New Energy Times* that Chang of *The New York Times* was working on a story for the next day regarding the Miller report. Taleyarkhan included documents from the Miller investigation provided to him from Chang.

New Energy Times immediately contacted Alisha Prather at the public affairs office of the House Committee on Science and Technology Investigations and Oversight Subcommittee to confirm the story and get further information.

Initially, Prather declined, stating that nothing was set to go out to the press until the next day. When *New Energy Times* informed Prather that we had some documents in our possession, she reacted with surprise because, she said, nothing had been officially released, even under embargo.

Prather said that she knew where the advance copies of the report came from, anyway, because they had "only gone out to one person."

In a follow-up e-mail, Prather implored *New Energy Times* to refrain from releasing a news report before May 11 because, as she said, "we'd like to appear as fair as possible."

The next day, May 11, Chang presented a review of the committee report, which was also released by Miller's office that day. The *Times'* story included confidential, previously unreleased documents that appeared to depict Taleyarkhan guilty as charged by his accusers, as well as by the Miller committee. Chang, like Miller, largely disregarded Taleyarkhan's side of the story.[6]

Taleyarkhan's full response to Chang's request for comment is reproduced below:

> Ken:
>
> I and several of my colleagues are appalled at the note from Representative Miller.
>
> Basically, One must question:
>
> - Why did this memo/letter omit ANY/ALL mention of the positive findings and supporting evidence from the many reports from Purdue University committees that ultimately prevailed to have Purdue's peers and administrators make its February 2007 decision to issue its press release concluding no misconduct and no need for further investigations? Why this intentional cherry-picking of points from the written reports specially chosen to portray a negative image?
>
> - Why did this memo/letter completely disregard the mass of public information and supporting evidence/information sent to it by me separately?
>
> - Why is it that the same people/detractors who openly as of last December eagerly awaited Purdue's verdict on my work during the year-long reviews and examination of facts come out to challenge the same, once the verdict was announced, because the outcome was not what they wanted to hear? Smacks of sour grapes and reminiscent of the political knee-jerk demand for a recount during political season.
>
> - Fundamentally, why would Purdue administration want to side with me rather than the accusers, one of them who was an administrator himself who fell from grace for his many actions totally unrelated to bubble fusion?
>
> - Why is it that, per rules of engagement, the requirements for confidentiality in state of Indiana's C-22 Process are being selectively applied to me? Why is it that the illegal actions of some go intentionally unpunished even when they openly are defiant of the admonishments from the provost and the rules we are all expected to abide by?
>
> - By any stretch this transmittal represents a gross travesty of justice. Where are the Jesse Jacksons and Al Sharptons of the Asian community during this episode that has caused this biased

and openly one-sided smear campaign?

Rusi Taleyarkhan

Yet Another Federal Investigation

But wait, there's yet another federal agency investigating Taleyarkhan.

This investigation is being conducted by the inspector general of the Office of Naval Research, which is charged with investigating research funded by DARPA.

The investigation, according to the Miller staff report, concerns the charges of fabrication or falsification of data.

Taleyarkhan asked the Office of Naval Research how or who initiated the investigation. Holly Adams, the inspector general, stated in an e-mail that “the source's identity is, and will remain, undisclosed."

The investigation appears to include an audit of the DARPA/UCLA grant, of which Taleyarkhan was a subcontractor.

The Naranjo paper lists DARPA as a supporter of the UCLA work.

"I've talked recently with Coblenz," Taleyarkhan wrote, "and asked if he had commissioned Putterman to use DARPA funds to perform modeling/simulations of my *Physical Review Letters* studies which were done not with external neutrons but with dissolved alpha emitters. He denied asking for/approving this."

New Energy Times contacted Coblenz to confirm. His public affairs officer Jan Walker replied on his behalf, "I'm sorry, but we are not able to comment at this time due to the ongoing investigation/inquiry."

New Energy Times contacted Putterman for his response, but he declined to comment.

CHAPTER ELEVEN

Graduate Student Victim of Collateral Damage

The May 11, 2007, article by Chang included a damning statement allegedly written by Purdue student Adam Butt on Feb. 23, 2006, and given to Tsoukalas' "fact-finding" committee.[1]

There are several points of concern about the Butt document.

As noted earlier in this report, one point is the question of intimidation by Butt's professors during a secret meeting, and a second point is possible duress suffered by Butt. In addition, the fact that Butt's letter switches from third person to first person suggests that the letter may not be completely his own or that he had assistance or was coerced. And the letter is not signed, which is extremely troubling.

New Energy Times called Butt to discuss these anomalies:

"Hello, this is Adam."

"Hi, my name is Steven Krivit. I'm a reporter with *New Energy Times*. I wanted to ask you some questions about the Taleyarkhan work."

"Uh, sorry, I can't answer any questions. No comment, I apologize."

Shortly after this exchange, *New Energy Times* received the following detailed explanation from Taleyarkhan.

> "Related to Adam Butt, for the record, here are the facts of the situation per my recollection:
>
> - In early 2004, Butt requested to join my group and conduct research into sonofusion, together with a joint M.S. with the School of Aeronautics Engineering.
>
> - The School of Aeronautics Engineering refused to support Butt in terms of financial support.
>
> - However, I accepted to fully support Butt as a student, and he

joined my group around May-July 2004.

- To get Butt's research started out, I directed him to learn the art/science by getting involved with the sonofusion experimental confirmatory work undertaken by Xu. To me, this appeared it could also be helpful to Purdue and to Xu for due-diligence checks by a third party.

- Xu agreed to have Butt review his experimental data, the manner in which the data were archived, transferred, analyzed and reported. I did not interfere in the conduct of this exercise, nor did I ask for clarification on what exactly Butt did. Butt did come back after a few weeks and reported to me that he had completed this assessment to his satisfaction and saw nothing wrong in Xu's reported results.

- Around December 2004, Xu received an invitation to submit his paper to *Nuclear Engineering and Design* from Editor-in-Chief Gunter Lohnert, thinking this would be a fine tribute to honor Dick Lahey of RPI for whom the special Festschrift edition was being prepared.

- Xu prepared his manuscript and added Butt's name to it as a courtesy gesture. Butt felt honored/motivated at the invitation and did look over the manuscript and offered his comments to Xu. There was no indication of any reluctance on his part at the time.

- Xu/Butt's paper review/acceptance, etc. was handled by Gunter Lohnert himself with no participation by me.

- The Xu/Butt paper in *Nuclear Engineering and Design* was published, I believe, in mid-2005.

- Purdue prepared a press release for which Butt/Xu were interviewed along with input from Tsoukalas (then the head of the School of Nuclear Engineering). In the press release issued by Tsoukalas which he fact-checked and formally approved for release, he took credit for directing and sponsoring this research of Xu/Butt. As part of the press release, Butt and Xu posed for photographers in front of their experiment station.

- Come Aug. 14, 2005, Butt invited me to his wedding and requested to include my family.

[Taleyarkhan wrote about the conflicts that developed in the department pertaining to Primary Committee voting on tenure and promotions. This statement has been relocated to an earlier section

of this report.]

- Around November-December 2005, Butt came to me and told me he would like to finish his MS thesis work for Nuclear Engineering and Aeronautical Engineering. I had earlier communicated with him the agreed-on requirements for research accomplishment for a good MS thesis. He finally completed the bare minimum expectations, and I signed off on his thesis for the Nuclear Engineering MS degree, but there was friction involved in terms of what Butt thought was OK to do to get a MS degree and my own expectations. During that time, surprisingly, Butt offered to me that he would not like to be associated with Nuclear Engineering for a Ph.D. and wished to go back to Aeronautical Engineering or elsewhere to get a job.

- On completing his MS thesis and knowing Butt was not continuing with my group further, I stopped supporting Butt financially. Mind you, all this time he was not supported by Aeronautical Engineering, but by me.

- A month later, in December 2005, Butt wrote to me stating he was surprised that he was not getting his monthly salary and demanded that he get it even though he was not working for me. He claimed he was having difficulty paying his bills and especially with a wife also on hand and expected to be paid by Purdue. Rather than hold ground, out of empathy I paid him from my funds in good faith and bid him farewell but to not expect anything more from me. During this transition, he may have had a difficult time because the School of Aeronautical Engineering had refused to support him in the past, and he may have been without salary for a while. I believe he left with a chip.

- In Jan. 2006, my group's paper in *Physical Review Letters* appeared. Around that time [Feb. 7], Tsoukalas set up his own illegal investigation committee without my knowledge or consent to find fault with my group's sonofusion research. State of Indiana (Purdue Univiversity) rules, per C-22, require individuals alleging research misconduct to report them to the dean/provost, who should initiate a review in utmost confidence.

-Tsoukalas appointed his crony Choi to head his own investigation committee, unknown to Purdue's higher administration, I think. The Choi committee called in Revankar, who was more experienced and recognized the illegitimacy of the process and refused to participate, calling the committee's existence illegal. Xu went in to meet not knowing the rules but stopped offering information after suspecting what seemed to be a request to denigrate a colleague in a

witchhunt. Butt, the most vulnerable member of the three, when summoned, talked with Choi, etc., then wrote his report of involvement.

- Butt, in his report, did admit to doing checks, etc., of Xu's experiments and does admit to reviewing the draft of the paper with his name on it and offering comments. However, the manner of communication and the way the Tsoukalas team portrays it to the world gives one the impression Butt was coerced into all of this. There is the other matter of his name being on the paper prepared by Xu/Revankar/Butt for the NURETH-11 conference in Avignon, France. Butt was to go there to present the paper and was elated at the opportunity. Xu should have been the ideal person to give the talk, but he could not go due to visa problems. Not having received his U.S. passport in time, he could not go in November 2005. He knew for several months of all of this, but now facts are being twisted to give a different impression. The talk was given by Revankar, who was going to the conference for his other paper, anyway.

-I believe Adam Butt has been put in a very difficult situation and compromised. To the extent possible, I would not like to see Butt pressured, and he should be left out of this silly game where the science of sonofusion is being forgotten and a National Enquirer-like farce is being propagated.

New Energy Times tried again to contact Butt by telephone and then by e-mail. Butt failed to reply.

CHAPTER TWELVE

Interview with Rusi Taleyarkhan

[The following interview was conducted on May 15, 2007.]

Steven Krivit: When and where were you born?

Rusi Taleyarkhan: In a small village called 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 No. 1 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. Hhe offered me a position as a research associate, or, as we call it, 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 Corp.

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. 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 ever since I was growing up, I had respect for Americans, Abe Lincoln, George Washington, those kinds of heroes, along with people like Mahatma Gandhi 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. 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 the Indian equivalent of M.I.T., and I became captivated with nuclear science and engineering.

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

SK: I read something about you on the Internet regarding weapons research that came out of your work with bubble/sonofusion. 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 U.S. As a result, 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 because 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 significantly greater than that from the best of high explosives.

Having done this sort of work resulted in understanding of how to intensify the explosive effects with "control" and of 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 has become an item I cannot talk about further because of 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 nation's 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. 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 thermohydraulics 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 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 me, Lahey, Nigmatulin and West on the original proposal. He also had Putterman, Suslick and Crum involved. Bob Apfel, a 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. 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 1991-94 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 1998-99. 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 single-bubble sonoluminescence 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 single-bubble sonoluminescence, for being able to grab a single bubble in a sound field and make it oscillate. Each time it oscillates, it implodes and creates flashes of light. The field of sonoluminescence started out in the 1930s in Europe by Frenzel and Schultes. West 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, they coalesce, they have a mind of their own. Despite a thousand 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. If you keep doing that often enough, you'll cause damage because of the impact 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 find a way to control it, to whatever extent that we could. With single-bubble sonoluminescence, it was like having one soldier walking on a bridge versus ours, multibubble, an army walking across. 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 single-bubble sonoluminescence 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.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.

We increased the size not by a factor of 10 but by 100,000. We start from the nanoscale and go up to the multimillimeter scale. You can see the bubbles grow up in front of your eyes, then implode into nothing. That was an innovation that we brought.

SK: From what you know, is the approach that Putterman uses the single-bubble sonoluminescence 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 single-bubble sonoluminescence, 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, then cause fusion conditions to take place. It was an extension of Gaitan's single-bubble sonoluminescence experimental approach.

SK: Do you know whether his approach has achieved 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? Single-bubble sonoluminescence or multibubble sonoluminescence?

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. 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 Schultes were doing, Tessien is using kilowatts of power.

SK: When did you first hire on to Purdue?

RT: My official tenure at Purdue started toward the end of August 2003. However, I was simultaneously offered a program director appointment for a typical four-year period by Tony Tether, the head of DARPA, who reported directly to Donald Rumsfeld. This was agreed on with Purdue administrators as a bilateral benefit and granted me an off-site assignment to Washington, D.C., starting from December 2003. The agreement with Tsoukalas was that he, along with Bertadano and Jevremovic, would take charge and ownership of bubble fusion research while I was gone for the four years. A year earlier, I had gotten them started in bubble fusion experiments.

As things transpired in December 2003 regarding family health issues, I had to reluctantly inform Dr. Tether of my inability to join his team, since I was well aware of the very significant commitment of time away from family that I would otherwise have needed to make to do my job well. This changed the dynamics and attitudes with my colleagues at Purdue, as I instead joined Purdue full time and began leading the bubble fusion research.

"There has been many a day that I'd come home dejected, desperate, but not until somebody really goes through trials and tribulations of that type, being called all kinds of things, nasty things -- you know, it shakes your self-confidence and your value as a human being sometimes.

"My life has been audited, my instruments have been audited and my books have been audited. The data speak for themselves, the data had to speak for themselves. It's difficult … to … know absolutely 100 percent sure that it is what I think it is. I just have to look at the data, and the data had been looked at very carefully. In the history of publications, I probably will not be able to find one that has gone through this level of scrutiny. If you do, let me know."

-- Excerpt from BBC interview with Taleyarkhan in September 2004

CHAPTER THIRTEEN

Assessment

> "Given that Suslick and Putterman have both investigated Taleyarkhan's past claims, they think it odd that they were not consulted by the editors of *Physical Review Letters* about the paper. 'There are other people who are very knowledgeable about this,' comments Martin Blume, editor-in-chief of the American Physical Society."
>
> -- *Nature*, Jan. 10, 2006[1]

The fervor in response to Taleyarkhan's claim that his students independently replicated his work is surprising. So what if Taleyarkhan (and Purdue) got away with calling the Xu/Butt experiment an independent replication?

How far will Taleyarkhan progress if his experiment can't be widely replicated by people who are not affiliated with him? If DARPA decides to fund one or two more replication attempts and they fail, will anybody care whether Xu and Butt "independently" replicated Taleyarkhan's experiment?

Are Taleyarkhan's critics so lacking in their confidence in the scientific method, the wisdom of the science community and the common sense of society to think that the inappropriate use of the word "independent" would allow him, or any scientist for that matter, to pull the wool over everyone else's eyes if that is what he was trying to do?

As for Taleyarkhan, the task that lies ahead should be clear. Lahey offers wise guidance in his interview in this report. Taleyarkhan should make sure that independent parties learn the required details of his experiment and develop the requisite skill to demonstrate the reality of a new phenomenon of nature. If he succeeds, the science community will benefit greatly, and so could the rest of society.

His critics need to go back to their own labs and do their own meritorious work rather than seeking success at the expense of their integrity.

And journalists need to demonstrate renewed rigor in heeding this advice from the Society of Professional Journalists' Code of Ethics: "Seek the truth, ... report it, ... test the accuracy of information from all sources and exercise care to avoid inadvertent error."[2]

We permit Rusi Taleyarkhan to have the final word in this report:

> "We learn from history that, for any major discovery, a lag time exists between the first announcement of the discovery and its independent confirmation or replication. For example, in recent memory, Einstein's Nobel prize-winning work related to the photoelectric-effect work of 1905 was at first considered heresy but eventually confirmed, more than a decade later."
>
> -- Statement by Taleyarkhan to the Miller Investigation Committee[3]

Acknowledgments

New Energy Times is a project of New Energy Institute, a 501(c)(3) organization whose mission is to investigate, analyze, educate and report on the progress of new, sustainable and environmentally friendly energy sources and research.

We wish to thank our sponsors for their generosity in making this report possible, all of the sources who have contributed to and participated in this report, the five technical reviewers for their support, and the phenomenal expertise of editors Sally Robertson and Cindy Goldstein.

Bubble/Sonofusion Controversy Timeline

First Two Taleyarkhan Published Papers and Related Events

2002 (March 8) Taleyarkhan publishes first paper in *Science*
2002 (July 25) Suslick publishes critique of Taleyarkhan in *Nature*
2002 (August) Shapira and Saltmarsh publish null result in *Physical Review Letters*
2003 (November 7) Purdue announces hiring of Taleyarkhan
2004 (March 22) Taleyarkhan publishes second paper in *Physical Review E*
2004 (September 17-18) BBC *Horizon* films on-site at Purdue
2004 (October 7-10) UCLA performs replication attempt for BBC*
2004 (~October/November) DARPA proposal for UCLA replication attempt submitted
2004 (November 4) UCLA finishes replication attempt for BBC
2005 (January 13) Xu/Butt submit manuscript to *Nuclear Engineering and Design*
2005 (January 13) BBC informs Taleyarkhan of UCLA's failed replication attempt
2005 (January 19) Taleyarkhan provides feedback to BBC
2005 (February 7) Xu/Butt paper accepted in *Nuclear Engineering and Design*
2005 (February 16) BBC broadcasts UCLA failed replication attempt
2005 (March 3) Suslick plasma paper publishes in *Nature*
2005 (~February/March) DARPA proposal for UCLA replication attempt approved
2005 (April 7) Lahey visits UCLA, observes DARPA replication attempt
2005 (April 28) UCLA publishes crystal piezo fusion work in *Nature*
2005 (~May) DARPA proposal for UCLA replication attempt funded
2005 (May 3) Xu/Butt publish replication in *Nuclear Engineering and Design*
2005 (July) Purdue Primary Committee votes on tenure and promotions
2005 (October 25) Nigmatulin publishes bubble fusion theory in *Physics of Fluids*

First Purdue Investigation and Related Events

2006 (January 10) Taleyarkhan self-nucleated paper accepted, *Physical Review Letters*
2006 (January 11) Coblenz requests review meeting at Purdue
2006 (January 27) Taleyarkhan self-nucleated paper published, *Physical Review Letters*
2006 (February 7) Tsoukalas creates "fact-finding" committee
2006 (February 20) Reich initiates contact with Taleyarkhan
2006 (February 23) Date on statement attributed to Butt
2006 (February 23) Tsoukalas' "fact-finding" committee delivers report to him
2006 (March 1) DARPA review, observation and witnesses of Purdue experiment
2006 (March 1) Putterman insinuates Taleyarkhan fraud via Cf-252 spiking
2006 (March 1) Reich contacts Taleyarkhan regarding Cf-252 spiking
2006 (March 2) Tessien observes second experiment at Purdue showing excess counts with CR-39 nuclear track detectors.
2006 (March 7) Naranjo's unpublished paper uploaded to Web
2006 (March 8) Chang writes first related news article for *The New York Times*

2006 (March 8) Reich writes first related news article for *Nature*
2006 (March 9) Chang writes second article
2006 (May 2) Taleyarkhan publishes erratum to January 27 PRL paper
2006 (May 14) Forringer performs successful experiment in Taleyarkhan's lab
2006 (June 6-7) Bugg observes successful experiment in Taleyarkhan's lab
2006 (June 20) Purdue completes first investigation, exonerates Taleyarkhan
2006 (July 20) Putterman/Reich/*Nature* imply Taleyarkhan misused funds
2006 (August) Tsoukalas publishes null results in *Nuclear Technology*

Second Purdue Investigation and Related Events

2006 (September 5) Tsoukalas initiates formal allegations in letter to Dunn**
2006 (September 12) Bertadano writes letter to Dunn with allegations**
2006 (September 22) Dunn writes letter defining Inquiry Committee**
2006 (September 21) UCLA submits manuscript of null result from DARPA replication
2006 (October 6) Naranjo's Cf-252 paper publishes in *Physical Review Letters*
2006 (October 6) Taleyarkhan's reply to Naranjo publishes in *Physical Review Letters*
2006 (October 16) Tsoukalas' removal***
2006 (November 17) Forringer's confirmation of Taleyarkhan accepted for publication
2006 (December 7) Suslick charges published by way of Reich in *Nature*
2006 (December 15) Purdue completes second investigation, exonerates Taleyarkhan
2007 (February 1) Naranjo submits comment on Taleyarkhan's reply, *Physical Review Letters* rejects
2007 (February 7) Purdue reports the completion of second investigation
2007 (February 9) UCLA publishes failure to replicate in *Physical Review Letters*
2007 (February 13) Suslick, by way of Chang/*New York Times*, suggests fraud
2007 (February 13) UCLA, by way of Chang/*New York Times*, claims precise replication
2007 (February 13) Suslick, by way of Reich/*Nature*, alleges Taleyarkhan fraud.
2007 (February 13) Putterman, by way of Reich/*Nature,* claims Purdue work is invalid
2007 (March 9) Purdue sends letters to editors of *Nature* and *The New York Times*

Miller Congressional Investigation

2007 (March 21) Congressman Miller initiates investigation
2007 (March) Office of Naval Research inspector general initiates investigation
2007 (May 7) Miller committee completes report

Third Purdue Investigation and Related Events

2007 (May 10) Miller committee provides Chang of *New York Times* exclusive
2007 (May 10) Purdue initiates third investigation
2007 (May 11) Miller committee press release

2007 (May 11) Chang of *New York Times* breaks story of Miller committee report

* Source: BBC *Horizon* documentary
** Source: Dec. 8, 2006, letter from Selander to Dunn
*** Source: Miller Report page 2

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