Sonofusion Research Scuttled

Putterman and Suslick's Response to Taleyarkhan's Discovery and Other Science Misdeeds

A New Energy Times Bubblegate Special Report

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“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.”

— Professor Rusi Taleyarkhan

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By Steven B. Krivit

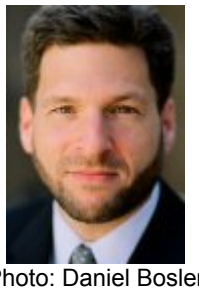
Photo: Daniel Bosler

In my first and only journalism class, in the 10th grade, I learned an important lesson: To get the best facts and the most complete truth, I had to do my own firsthand investigation and reporting.

Charles Seife, professor of journalism at New York University, did some firsthand reporting on the story about Rusi Taleyarkhan, professor of nuclear engineering at Purdue University, several years ago when Seife was a reporter for *Science*.

He relies on this experience in a chapter in his latest book, *Sun in a Bottle: The Strange History of Fusion and the Science of Wishful Thinking,* in which he writes about Taleyarkhan and his collaborators' work on sonofusion, also called bubble fusion.

However, in the latter part of the chapter, Seife dropped the ball. He relied on second-hand information: news reports written by journalist Eugenie Samuel Reich for *Nature.*

As this issue of *New Energy Times* shows, the related news stories by both Reich and Kenneth Chang of the *New York Times* are riddled with flaws.

Seife's second major failure is that he didn't distinguish between discrediting a scientist and discrediting the scientist's research. The consequences and significance of this flaw will give other professors of journalism profound material to discuss with their students. Seife's book is a stellar and unfortunate example of the collapse of the distinction between science and science politics.

Seife's third significant failure is that he embarks on an unprofessional method of science reporting when he writes, " I was convinced. Taleyarkhan was wrong: bubble fusion was a fiction."

How did Seife reach his conclusion? He found an expert in a competing (billions of dollars of federal grants) fusion laboratory working on a competing fusion program. The expert gave Seife a plausible-sounding explanation for why the Taleyarkhan group's sonofusion research was a mistake.

And what was Seife's unprofessional method of science reporting? He omitted a crucial step; As far as Taleyarkhan knows, the expert's speculations were never published in a peer-reviewed journal.

Is Seife's process any better than "science by press release?" No, his expert's speculations don't count unless they are published in a scholarly journal.

Journal articles are peer-reviewed. They are also vetted by the journal editors, and the editors consider the comments of the reviewers. Those journal editors also normally provide for a fair and level playing field. That is, if a comment or critique comes in to a journal, the editor normally follows the practice of informing the respondent of the comment and affords the person the opportunity to provide a thoughtful, careful response.

This is the ethical thing to do and one of the reasons why science journals trump science media as authoritative sources.

Nevertheless, a member of the Taleyarkhan group, Richard Lahey of Rensselaer Polytechnic Institute, told New Energy Times that the issue had already been addressed.

"The issue of the amount of tritium produced by the external neutrons is negligible (as verified by our calculations and direct measurements)," Lahey wrote. "We have written about this and resolved it long ago."

In writing that Taleyarkhan's work was a fiction, Seife causes untold harm to Taleyarkhan, his collaborators, their work and the field of sonofusion.

According to Taleyarkhan, Seife did not discuss his expert's opinion with Taleyarkhan. Left uncorrected, Seife's flawed book will do a great disservice to science, if not to society at large.

For further confirmation of Seife's opinion, he reports on the April 18, 2008, Purdue C-22 investigation, which concluded that Taleyarkhan (though not his co-authors) had, according to Seife, "deceived the scientific community by falsely claiming the Xu and Butt paper was independent confirmation of Taleyarkhan's original bubble fusion paper."

Purdue released its C-22 report on July 18, 2008. Ten days later, Taleyarkhan provided his 95-page appeal/rebuttal to Purdue. Had Seife read Taleyarkhan's appeal/rebuttal, he would have learned of the flaws in the Purdue report. By ignoring that information, Seife failed to give Taleyarkhan a voice in the matter.

During the last few months, while Seife's book was in press, the American Physical Society reviewed what Taleyarkhan had to say on the matter of independence. The APS listened. Five days ago, after significant discussion and inquiry (first draft, second draft, third draft,) the APS published its final decision: The Taleyarkhan group's claim for an independent confirmation stands.

Three days ago, Ann Finkbeiner, a visiting associate professor with Johns Hopkins University, wrote a review of Seife's book for the *New York Times*. Finkbeiner assumed that the section Seife wrote on bubble fusion was reliable.

She compared the Taleyarkhan group's effort to "cold fusion" and wrote that bubble fusion "couldn't be repeated, either, and likewise ended in disgrace."

Finkbeiner is correct that the Taleyarkhan group's paper published in *Science* was controversial. She is incorrect that bubble fusion couldn't be repeated. (She and Seife are also incorrect that "cold fusion" couldn't be repeated, either, but that is another topic.)

Sonofusion has been repeated by Taleyarkhan, on demand, and witnessed by outside observers, and it has been repeated by unaffiliated researchers William Bugg (Stanford) and Edward Forringer of LeTourneau University in Taleyarkhan's lab.

To my knowledge, the Bugg, Forringer and Xu/Butt papers have not been discredited in the scientific literature. Only Taleyarkhan - not his collaborators and not their work - has been discredited, by mainstream science media.

Seife's review of the scoping attempts performed by Oak Ridge National Laboratory scientists Dan Shapira and Michael Saltmarsh is substantially misleading; this matter is reviewed in another article in this issue of *New Energy Times*.

And, despite the fact that Purdue turned on Taleyarkhan and made him a scapegoat - to distract the government from more serious problems in the School of Nuclear Engineering - Purdue researchers Yiban Xu and Adam Butt did, in fact, replicate the Taleyarkhan group's experiment, and did so partially independently. In this issue of *New Energy Times*, readers will begin to learn the real story about Adam Butt and his involvement with the conflict at Purdue.

Now, if Purdue wants to vilify Taleyarkhan and his collaborators for honestly expressing their opinion that their work was replicated independently, I suspect that the Purdue administration will end up looking foolish, at best, and mean-spirited, at worst.

Ironically, the Purdue staff includes some of the world's experts in sonofusion, and should the university's administration figure this out - and clean house at the School of Nuclear Engineering - Purdue might take the opportunity to create funding opportunities rather than destroy them.

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2. Bubblegate Series Introduction

This investigation is about the scientific work of five researchers* on a team led by Rusi Taleyarkhan, a professor in the Purdue School of Nuclear Engineering, and the politics surrounding their research. The team is one of several groups that, for several decades, has been investigating and attempting to achieve acoustic inertial confinement fusion. The team is the first to claim success. It calls its version of this research bubble fusion.

Read more online...

* JaeSeon Cho (formerly with Oak Ridge National Laboratory), Robert C. Block (Rensselaer Polytechnic Institute), Richard T. Lahey Jr. (Rensselaer Polytechnic Institute), Robert I. Nigmatulin (Russian Academy of Sciences) and Colin West (formerly with Oak Ridge National Laboratory)

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3. Bubblegate Video Shorts

Rusi Taleyarkhan - Purdue's Unsung Hero
Rusi Taleyarkhan: "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 and it's difficult, you know...how can I answer that I know absolutely one hundred 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."

UCLA Failure to Replicate Oak Ridge Experiment
Seth Putterman explains how he and his UCLA students failed to replicate the Taleyarkhan group bubble fusion experiment.

Bubblegate: How Purdue Fabricated the Allegations
The two charges of which the 2008 Purdue C-22 Investigation Committee found Rusi Taleyarkhan guilty had been considered and dismissed by two earlier groups, the 2006 Purdue C-22 Inquiry Committee and the 2007 Purdue C-22 Inquiry Committee. Those same charges were also inserted into the 2008 Purdue C-22 Investigation Committee illegally. The two charges were fabricated by the investigation committee, as reported by New Energy Times on Oct. 14, 2008. We produced and published this video on Oct. 20, 2008. It lays out the details, step by step, to show how Purdue fabricated the allegations against Taleyarkhan. Eleven days after we published this video - on Oct. 31, 2008 - Purdue relieved Peter Dunn of his responsibilities as research integrity officer.

Putterman's $800,000 Failure to Replicate
In 2005, Seth Putterman said "Nothing is too wonderful to be true that it can't be reproduced in another experiment. And this is what distinguishes science from religion."

Many years ago, John Bockris said "Negative results can be obtained without skill and experience."

Bubblegate: Lecture of Adam Butt
Adam Butt's bubble fusion lecture with guests from Purdue administration Sally Mason, Linda Katehi, Lefteri Tsoukalas and Vince Bralts. A congressional investigation wrote that Butt said he "had nothing to do with the research."

4. Taleyarkhan's Nightmare

For Rusi Taleyarkhan, professor of nuclear engineering at Purdue University, March 1, 2006, was a most unfortunate day, the beginning of the biggest nightmare of his life.

On that day, a dozen academicians and government representatives came to his laboratory to observe research that appeared to contradict prevailing understanding of nuclear fusion - in particular, the acoustic inertial confinement fusion method. One of the observers had failed twice in his own attempt to replicate the Taleyarkhan group's experimental conditions and results. Years earlier, the same observer had obtained a U.S. patent for his own method of AICF. That method, despite the awarded patent, has not demonstrated fusion.

The Taleyarkhan group's claim appeared to have the classic hallmarks of science fraud - or self-delusion or perhaps incompetence. After the cold fusion controversy of 1989 that evolved from the University of Utah's announcement of the research of professors Martin Fleischmann and Stanley Pons, the mere mention of tabletop fusion evoked sniggering, if not outright dismissal, from science authorities.

The Taleyarkhan groups's claim was not "cold fusion," however; they claimed a novel method to create a thermonuclear fusion reaction in a tabletop experiment by the use of ultrasonic waves and an effect known as sonoluminesence, a process by which sound waves create energetic flashes of light.

But like "cold fusion," the claim appeared to contradict prevailing understanding of the conditions required to create nuclear fusion. Also like "cold fusion," their results were difficult to replicate. For quite a while, the principle of replication, which is considered the gold standard of scientific credibility, appeared to be working against the Taleyarkhan group.

Therefore, the observers in Taleyarkhan’s laboratory likely did not arrive that day with open minds, eager to learn where and how they had failed in their attempts or how the Taleyarkhan group's knowledge of a presumably new form of nuclear fusion failed to transfer to the would-be replicators.

Just a year earlier, on Feb. 16, 2005, Seth Putterman, a theoretical physicist at UCLA and one of the observers at this meeting, went public on worldwide television and helped the British Broadcasting Corp. convey the idea that he had performed an authoritative assessment of the Taleyarkhan group’s work by carrying out a precise replication and that there was, according to Putterman, nothing there.

Around the same time, Putterman and Taleyarkhan agreed to share $810,000 from a federal Defense Advanced Research Projects Administration grant to make another attempt at replication of the Taleyarkhan group’s experiment, this time in Putterman's

lab. $347,000 went to Putterman's research group, $145,000 went to Putterman's colleague Ken Suslick, a sonochemist at the University of Illinois, and $318,000 went to Taleyarkhan.

The unlikely marriage of Putterman, Suslick and Taleyarkhan seemed destined to fail. By the time the DARPA grant was funded, around May 2005, the BBC show had aired, and Putterman had slammed the Taleyarkhan group's research. How and why they agreed to bind themselves in this collaboration remain a mystery.

Taleyarkhan had intuitively sensed trouble with the Putterman group - as he said on the show - but he had no idea how bad it would turn out until after the show aired. Putterman, however, had a better idea of what was brewing at UCLA.

As close as I can track, it was around October or November that Putterman submitted the proposal to DARPA for the replication attempt of the Taleyarkhan group's external neutron-source experiment. But the UCLA replication attempt by Putterman or, more accurately, his students ended Nov. 4, 2004.

He knew at the time he put in the DARPA grant application or shortly thereafter that his group had failed to replicate and that the news would be broadcast on television worldwide. Perhaps Putterman was truly good-natured about the whole thing and thought that it was worth a second try. The BBC had given him $70,000 for the replication. Perhaps he could make it work with $347,000 from DARPA.

Putterman explained his motive for working with the BBC to *The New York Times*.

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

The Bubblegate timeline and library index clearly shows that Putterman and Suslick had staked a claim in the race for nuclear fusion from sonoluminesence and had conflicts of interest with the Taleyarkhan group involving intellectual property as well as intellectual primacy.

DARPA Review Meeting at Purdue

At 2 in the afternoon on March 1, 2006, Taleyarkhan's unforgettable nightmare began to take form. Putterman suddenly announced during a group discussion —with government sponsors present - that Taleyarkhan's results could be explained by ordinary means. Putterman suggested that Taleyarkhan's presumed evidence for a novel fusion reaction was merely the result of a common radioactive laboratory source, californium-252.

Putterman did not use the "f" word. He didn't have to accuse Taleyarkhan directly of fraud. It was assumed. As Taleyarkhan would write to me later, it was a "veiled manner of accusing fraud." The alternative to fraud - that Taleyarkhan was simply careless - was not even a consideration. Putterman provided the setup, his graduate student Brian

Naranjo provided the follow-through seven days later, and eight days later Nature and the *New York Times* did the rest.

Two hours after Putterman spoke up, Taleyarkhan received an e-mail and voicemail from Eugenie Samuel Reich, a freelance reporter working for *Nature* on a story about the Cf-252 contamination accusation. Reich told me that she convinced *Nature* to accept her story the prior week, on Feb. 22. It was, at its root, the same Cf-252 contamination accusation story that Putterman delivered to the DARPA group just two hours earlier. Taleyarkhan was bewildered. The attack on his work and his reputation seemed to have been coordinated and came without warning.

Reich told me that she initially contacted Taleyarkhan on Feb. 20, though she did not state whether she had begun to discuss Putterman's allegation with Taleyarkhan at that time. Perhaps Reich heard about Putterman's accusation when Taleyarkhan heard about it.

Perhaps Reich did inform Taleyarkhan on Feb. 20 and *Nature* on Feb. 22 that she was working on the Cf-252 contamination story. If so, then she would have had advance knowledge from Putterman and Naranjo.
Taleyarkhan responded to Putterman and Reich that the accusation of Cf-252 contamination needed to be vetted through scholarly journals, not through the science media.

On Wednesday, March 8, the world awakened to the news that Taleyarkhan was a fraud.

I was working late on Tuesday night and saw the story break just minutes after its publication on the *New York Times* Web site. *Times'* reporter Kenneth Chang didn't need to use the "f-word." It was implicit in the language ("investigation," "extremely serious concerns," "accusations") and context of his story.

The clincher came in this paragraph: "Brian Naranjo, a graduate student at the University of California, Los Angeles, said his analysis of data from the last scientific paper that was published by Dr. Taleyarkhan's group showed a chance of less than one in 10 million that the emission pattern could have been generated by fusion."

It was a tiny step for any reader or other news outlet to assume that Taleyarkhan was being investigated for science fraud.

I was concerned that Chang and the *New York Times* had blundered, because even the mere suspicions of scientific misconduct in the public sphere can destroy a scientist’s career. I had investigated the cases of three other scientists who had been wrongly accused of fraud in the media. I suspected that there was more to the Taleyarkhan story than Chang had told readers.

I shot off a critical e-mail to Chang around midnight, Pacific time. I got a reply from him within a few minutes. It was 3 in the morning in New York. Over the next 24 hours, we exchanged at least 18 e-mails, bickering and betting on our disparate points of view.

The meat of the Taleyarkhan news story had been prepared by Reich. She had shared an advance copy of it with Chang through an embargo agreement. Judging by the fact that no other news outlets published articles concurrently with either the *Times* or *Nature*, Chang probably received an exclusive advance copy of the set of four articles from Reich. (A year later, on Feb. 13, 2007, they again published articles in their respective outlets concurrently.)

Chang was not supposed to publish anything about the story until Reich's articles came out, but he did. However, he was as eager as she to be first with a major science-fraud story.

Reich's series of four articles provided more of the details. Hers hit the Internet about 3 a.m. The Reich articles were concise, extremely well-written and conceived, and devastating in their impact. The message - from *Nature*, arguably the most prominent science news source in the world, and the *New York Times*, perhaps the most influential newspaper in the world - was that Taleyarkhan's work was a chimera and that he fabricated his results by spiking the experiment with a Cf-252 source.

On Thursday, Chang published a more circumspect follow-up article.

Maggie Fox, with the wire service Reuters, borrowed the content of the *Nature* articles and apparently did none of her own research on the story. CNN cut and pasted the entire Reuters article and did no original research. United Press International, another wire service, failed to do its own research and used the content from Chang's article in the *Times*.

Rick Callahan, with the Associated Press, attempted to get quotes directly from the key sources but failed. Instead of delaying his article until he got his own quotes, and instead of writing a limited article based on the scant facts he was personally able to obtain and verify, he used and relied on the content of Reich's *Nature* articles.

The f-word entered the picture in a *Reuters* article, one hour after the *Nature* stories hit the Internet. By 11:30 p.m. on March 8, 2006, Google News reported that the story had appeared in 54 news outlets. Of those, only four outlets appeared to have performed original reporting; everyone else either republished verbatim one of the four stories that contained original reporting or borrowed quotes from the two articles which broke the story, in *Nature* and the *Times*.

Meanwhile, Chang sent me the following e-mail:

> The whole premise of your complaint to me yesterday, was that I left the man's reputation in tatters with a 500-word story. So either there is great power in the

> *Times*, which could have been even more incendiary with a bit of skillful prodding - or planting a story in *Nature* is really how you destroy someone's reputation, in which your complaint is simply that I did not let Rusi enjoy his last half-day of peace.

The fuel that provided the fire for the Reich and Chang articles came from two groups. The first group was professors in Taleyarkhan's university: the former head of the school of nuclear engineering, Lefteri Tsoukalas, and Tsoukalas' lover and a professor in the school, Tatjana Jevremovic. The second group was from UCLA and the University of Illinois, the collaboration of Putterman and Suslick.

Chang was correct on both counts. There is great power in the *Times*. Other news outlets worldwide borrowed quotes from him and republished them apparently unquestioningly. My analysis of Reich's articles (further on in this article) shows that planting a story in *Nature* is an effective way to destroy a scientist's reputation.

Only Callahan ran a story that included quotes from Taleyarkhan that day. Taleyarkhan directed Callahan, appropriately, to the Purdue News Service. Due diligence by most reporters that day generally was limited to the obligatory request for comment from Taleyarkhan.

We will return to the Chang and the Reich stories. For now, I will briefly summarize the various investigations and inquiries from 2006 through 2008. Two of them preceded the Reich and Chang articles by a month; the others followed the stories.

Seven Investigations Later, Still No Fraud

Seven investigations - or in some cases, reviews or examinations - have considered the merits of Taleyarkhan's research and the integrity of his conduct. The investigations have been primarily academic rather than judicial. Many of them appear to have run over the same ground. A question that future scholars may consider is, What is the relevance of the constitutional protection from double jeopardy in the context of academic investigations? Alternatively, the question may be answered by the courts.

According to Taleyarkhan, he has been the subject of more than 50 allegations, many of them overlapping and repeating each other. By August 2007, they had been narrowed to 34. By November 2007, they had been narrowed to 12, and in April 2008, the remaining 12 were dismissed. Two allegations in the April 2008 investigation - of mysterious origin - resulted in convictions in an academic trial conducted by his university.

The two charges of which the 2008 Purdue C-22 Investigation Committee found him guilty had been considered and dismissed by two earlier groups, the 2006 Purdue C-22 Inquiry Committee and the 2007 Purdue C-22 Inquiry Committee. Those charges also appear to have been inserted into the 2008 Purdue C-22 Investigation Committee illegally. The two charges were fabricated, as I reported in an article on Oct. 14, 2008, and in a video on Oct. 20, 2008.

Taleyarkhan first stated that the charges were fabricated on March 19, 2008, two days after he was given a copy of the draft report. After the report was finalized on July 17, 2008, Purdue gave Taleyarkhan a formal chance to appeal. But before the appeals process began, Purdue immediately published a press release telling the world that the university found Taleyarkhan guilty of research misconduct. In the next 10 days,

Taleyarkhan produced a 95-page rebuttal, but Purdue's appeals committee dismissed it outright.

I have read most of Taleyarkhan's appeal/rebuttal report. I have found two instances where Taleyarkhan has made minor errors. By and large, he provides evidence to show that Purdue was grossly negligent in the process that resulted in the April 18, 2008, final report and the two guilty charges.

Consequently, the university's dismissal of his appeal may land Purdue in hot water. Taleyarkhan has no reason to believe that it would do him any good to file a grievance with Purdue. On Nov. 5, 2008, Brian Wallheimer, a reporter with the West Lafayette, Indiana, *Journal & Courier,* reported that Taleyarkhan was preparing for a federal lawsuit.

Two years earlier, on Dec. 15, 2006, the 2006 Purdue C-22 Inquiry Committee investigation concluded and exonerated Taleyarkhan of all charges. Purdue announced the news (belatedly) on Feb. 7, 2007. In response, Reich and Chang, with the help of Putterman, Suslick and Tsoukalas, published (simultaneously on Feb. 13) hostile articles and escalated the rhetoric.

A few weeks later, on March 9, 2007, Purdue wrote strong protest letters to *Nature* and the *Times*. Purdue administrators Sally Mason and Charles Rutledge (now both retired from Purdue) delicately said that Reich and Chang had misrepresented the facts and that the reporters appeared oblivious to the ethics of protecting people involved in an investigation.

To the *Times*, they also wrote that the Chang article misled the public into expecting that a "successful and fair inquiry might ... include a public airing of every allegation, no matter how baseless."

To *Nature*, they also wrote that the Reich article misled the public into expecting that a "successful and fair inquiry" might likely include a finding of guilty.

Purdue's letters were not published.

Purdue Administration Turns on Taleyarkhan

On Sept. 5, 2006, Charles Rutledge, Purdue's vice president for research at the time, solicited misconduct allegations from the School of Nuclear Engineering faculty members Tsoukalas and Martin de Bertadano.

On Dec. 15, 2006, the Purdue C-22 Inquiry Commitee completed its investigation and exonerated Taleyarkhan of the allegations presented to the committee.

Around March 21, 2007, U.S. Rep. Brad Miller initiated a congressional investigation in response to anonymous complaints. Concurrently, the Office of Naval Research's inspector general received several anonymous complaints, which it forwarded to Purdue for more investigation. Miller told Purdue that its 2006 Purdue C-22 Inquiry Committee didn't do a good enough job when it exonerated Taleyarkhan. But Miller's investigation didn't produce any hard evidence or convictions against Taleyarkhan.

In a May 9, 2007, public letter to Purdue, Miller wrote that a contributing author to a paper (student Adam Butt) "said he had nothing to do with the research." However, on Feb. 10, 2005, Butt had explained in a videotaped presentation in front of top Purdue administrators how he was involved in the research and that it was part of his thesis work.

On Oct. 14, 2008, I reported and demonstrated how the two guilty charges Purdue came up with (labeled A.2 and B.2) were illegitimate and were fabricated. Within hours, Dunn initiated yet another investigation and demanded that Taleyarkhan produce all work product relating to his collaboration with Putterman.

On Oct. 20, I produced and published a video on Youtube which showed how Purdue created its charges against Taleyarkhan. I laid out the details, step by step, and provided original source documents. Eleven days after I published the video, on Oct. 31, 2008, Purdue relieved Dunn of his responsibilities as research integrity officer.

With the exception of the 2008 UCLA-Purdue Investigation, Purdue seems to be done with its previous investigations of Taleyarkhan. Because Taleyarkhan's work was funded by DARPA, the Office of Naval Research has the final word on the Purdue investigation. ONR Inspector General Holly Adams appears, however, to be undecided about Purdue's last investigation. On July 17, 2008, Adams notified Purdue that ONR accepted Purdue's April 18, 2008, C-22 Final Report.

However, Adams is supposed to issue her decree of acceptance or rejection of Purdue's sanctions. As we go to press, those sanctions have been out now for more than three months. The delay suggests that ONR may have problems with Purdue's nonresponse to Taleyarkhan's appeal or Purdue's sanctions, or both. Adams' July 17 communication occurred before Taleyarkhan submitted his appeal/rebuttal report to Purdue so she did not - officially - have the benefit of that additional, substantial body of information. She had, however, previously received an advance draft of Taleyarkhan's appeal/rebuttal in response to Purdue's draft report.

In the 2007 letters to *Nature* and the *New York Times,* Purdue wrote, “The Department of Health and Human Services integrity guidelines require American institutions to protect ‘the confidentiality of respondents, complainants, and research subjects’ when investigating allegations of misconduct.”

Yet in an Aug. 27, 2008, press release, Purdue announced the availability of, and provided, the 38-page April 18, 2008 Final Report of the investigation committee, which includes the identities and statements of parties to the investigation.

Taleyarkhan later wrote a 95-page appeal/rebuttal. Purdue later dismissed Taleyarkhan's appeal/rebuttal outright. Details of the appeal/rebuttal will appear in other *New Energy Times* investigations.

Chang Breaks the News

We now take a closer look at the two stories that launched the worldwide perception that Taleyarkhan was a fraud and his group's research was bogus. We begin our analysis with Chang's March 8, 2006, story.

Chang conveyed the impression that other scientists at Taleyarkhan's original laboratory, Oak Ridge National Laboratory, disconfirmed Taleyarkhan's positive results: “but two other scientists at Oak Ridge, using their own detectors, said they saw no signs of neutrons.”

Here are the facts, provided by Taleyarkhan, that Chang omitted:

> What is not appreciated is that Shapira and Saltmarsh did not run their experiments over months or even days. They made a single measurement [on one of my experiments] on a single afternoon over the period of about one hour while they were alone in Taleyarkhan's laboratory, using a different liquid scintillation detector.
>
> Indeed, they did obtain a statistically significant result for nuclear emissions for excess nuclear emissions time-correlated with the region of time when deuterated bubble clusters were imploding. Shapira or Saltmarsh did not investigate for tritium, nor did they conduct control experiment-related measurements with non-deuterated liquid. Because of the large size of their detectors, there were electronic issues related to gamma saturation in some time regions. The volume of the Shapira-Saltmarsh detector was 30 times larger than the one used by the Taleyarkhan team. Large does not always mean better since it can become readily overwhelmed, and this is what transpired for the Shapira-Saltmarsh detector system.
>
> A review of the raw data has already been published as a reference in the Science (2002) paper. However, the media has distorted this scoping attempt by Shapira-Saltmarsh, who are both accomplished scientists in their own right. The data obtained by the Taleyarkhan team were reviewed at length by ORNL management, taking assistance from resident expertise, and eventually, after many tens of in-depth assessments, ONRL decided that the work and data obtained were on solid ground. (See comments from Glenn Young, director of the physics division at ORNL in 2004, to the New York Times and also the ORNL

> position statement in consultation with Prof. Lee Riedinger, executive vice president for science and technology at ORNL, both of whom participated in in-depth reviews and audits.) ONRL also took into account - for due diligence - the Shapira-Saltmarsh scoping attempts. The Shapira-Saltmarsh effort was not "experimentation."

Chang wrote, “No other scientists have been able to reproduce the findings.”

This was not true. Chang failed to report that, before Taleyarkhan worked at Purdue, he had trained Purdue postgraduate researcher Yiban Xu on the experiment and that Xu had performed successful experiments. With Purdue student Adam Butt, Xu published a paper on May 3, 2005.

Chang also failed to report that Tsoukalas' group had performed several successful experiments and observed "positive evidence for excess tritium produced (and attributed to D-D nuclear fusion)."

Tsoukalas had been sufficiently convinced of his own experimental success that, on Sept. 19, 2003, he wrote on the wall in the laboratory, "Bubble fusion was achieved here."

Chang was entitled to ignore this because the draft paper with the positive data was never published.

However, had Chang investigated more thoroughly, he would have learned that Tsoukalas had been enthusiastic and eager to publish his group's independent replication in 2005. Chang would have learned that Tsoukalas had even submitted an abstract to the NURETH-11 conference claiming that his group had replicated the Taleyarkhan group's work reported in *Science*.

The Taleyarkhan group's abstract claimed "positive evidence for excess tritium produced (and attributed to D-D nuclear fusion). ... The results point to statistically observable tritium increases in post-cavitation deuterated acetone samples."

Chang also would have learned that skepticism of and antagonism toward sonofusion from a few senior professors in the Purdue School of Nuclear Engineering later drove Tsoukalas to take their side and pull the paper from the NURETH-11 conference. Around the same time, an e-mail from Jay Gore, a Purdue professor of engineering and director of Purdue's Energy Center, shows that communications from Putterman and Suslick helped fuel the skepticism within the Purdue School of Nuclear Engineering.

Chang wrote, “Meanwhile, Brian Naranjo, a graduate student at the University of California, Los Angeles, said his analysis of data from the last scientific paper that was published by Dr. Taleyarkhan's group showed a chance of less than one in 10 million that the emission pattern could have been generated by fusion.”

The above quote has several problems. Even if Naranjo's simulated theoretical speculations accurately represented Taleyarkhan's conditions, Chang fundamentally misrepresented the significance of Naranjo's theory. Theory does not disprove experiment, though experiment can disprove theory. On this ground alone, Chang was deceptive in suggesting that Naranjo's theory had anything authoritative to say about the Taleyarkhan work.

The second problem is that Naranjo, according to Taleyarkhan, did not construct his computer modeling simulation with an accurate representation of Taleyarkhan's physical experiment. On this ground, the Naranjo claim is false, and the Chang quote fails to tell the truth.

The third problem with the Naranjo theory is that the ratio of neutrons and gammas emitted from Cf-252 is roughly inversely proportional to those emitted from the Taleyarkhan group’s experiments. Hence, the Naranjo speculation fails on an empirical basis alone.

The fourth problem (for Chang) with the Naranjo allegation is that the paper was, at the time, not peer-reviewed and not published. Ordinarily, reporting on unpublished research is considered bad form for science journalists. Naranjo had uploaded the paper to an Internet Web site on March 7, hours before Chang's article published. Had Chang waited until Oct. 6, 2006, when Naranjo's paper published - concurrently with the Taleyarkhan group's reply - Chang would have avoided the ensuing problems.

When Chang's story hit on March 1, 2006, Taleyarkhan declined to comment on the Naranjo preprint.

Had Chang attempted to verify Naranjo's comments or waited until the two papers published, he might have learned of and reported the problems with Naranjo's theory.

In paragraph 10, Chang brings in a separate element to the Cf-252 subplot. Whereas the Cf-252 spiking allegation was based on Naranjo's theory, this next paragraph refers to a physical attempt from Naranjo's thesis adviser, Putterman, to confirm the Taleyarkhan claims: “With $350,000 from the Defense Department, Seth J. Putterman, a professor of physics at U.C.L.A. and the thesis adviser to Mr. Naranjo, has tried to build a replica of Dr. Taleyarkhan's apparatus and has not seen any signs of fusion.”

In fact, Putterman may have had little to do with any of the hands-on work. A BBC videotape of Putterman with students in the UCLA lab shows the students doing all the work. At one point, Putterman even asks one of the students to explain to him how part of the "plumbing" works.

The first, fundamental problem with paragraph 10 is Putterman's (and Chang's) insinuations that Putterman's failure to replicate means anything about the Taleyarkhan group's experiment. Failure to replicate means failure to replicate.

The second problem with paragraph 10 is that, yet again, Chang reports on unpublished research. The paper that the Putterman group eventually published on his failure to replicate the Taleyarkhan group appeared nearly a year later, on Feb. 9, 2007, and it hadn't been submitted until Sept. 1, 2006, five months after Chang reported "no signs of fusion."

The third problem is that Chang did not tell readers that Putterman is a direct competitor of Taleyarkhan's for government funding, intellectual property and intellectual primacy in the AICF field. Chang had an obligation to inform the public of Putterman's self-interest and failed to do so. Despite Putterman's public reassurances that the field of AICF remains legitimate, Putterman appears to have abandoned that line of research in favor of crystal-piezo fusion.

Chang's fourth problem is that he did not seek Taleyarkhan's opinion on whether the Putterman group had, in fact, replicated the experimental conditions and apparatus correctly. Had Chang done so, he might have learned from Taleyarkhan's collaborator, Richard Lahey of RPI - as did Erico Guizzo of *IEEE Spectrum* that the Putterman experiment was "doomed to fail."

The Putterman group's paper that eventually published on Feb. 9, 2007, misrepresented the Taleyarkhan group results. Putterman had gone public on BBC-TV on Feb. 16, 2005. The foundation of the Putterman group claim that their replication produced a null result was based on the issue of timing coincidences between the sonoluminesence flashes and the neutron signals. We will review that skirmish in more detail in a future article.

In paragraph 11, Chang writes, "Dr. Putterman said he told Dr. Taleyarkhan of the calculations last week on a visit to Purdue. 'He didn't have any clear answers,' Dr. Putterman said. 'From my perspective, his answers were not satisfactory.'"

The unsatisfactory answer that Taleyarkhan provided to Putterman, which Chang omitted to publish, was that he told Putterman he should follow the time-honored tradition and submit his comment to the journal that published the paper in the first place and that Taleyarkhan would respond to that, rather than participate in mud-slinging in the press.

In paragraph 12, Chang writes, "Californium is present in Dr. Taleyarkhan's laboratory, stored in a closet about 15 feet from the experiment - close enough to generate the results reported in Dr. Taleyarkhan's paper if it had been stored improperly."

In this paragraph, Chang introduces a third, independent factor. For review, the first factor was the theoretical calculation by Naranjo. The second was the context of the null result of replication attempt by the Putterman group. Now Chang introduces the idea that Taleyarkhan might have stored the Cf-252 improperly.

This speculation has a few problems, beyond the fact that the branching ratio of Cf-252 and the Taleyarkhan group's experiment are inversely proportional. First, if it was stored

improperly at Taleyarkhan's lab in Oak Ridge, all of the numerous controls that Taleyarkhan used would have showed positive signals, but they did not.
The second problem is that Cf-252 also would have had to be stored improperly in the pharmacy lab at Purdue where Xu performed his replication. The third problem is that it would have had to be stored improperly on March 1 and 2, 2006, when the dozen observers (including Putterman and Naranjo) were in Taleyarkhan's lab performing their evaluations, checking the signals and the background themselves.

The fourth problem is that, if there was a storage problem, it would fail to explain why the devices under test showed positive signals and the controls did not - unless some secret device was shielding and unshielding the Cf-252.

Chang offered his defense to my admonishments for his "shoot first, ask questions later" style of journalism. He seemed to assume that Taleyarkhan's experiment was child's play.

“It's really easy for Rusi to immediately and completely resuscitate any damage the last couple of days have caused him," Chang wrote. "As soon as someone else reproduces the finding - and the set-up described in the January paper is one that is easily in reach for many laboratories - he can go 'Nyah, nyah' all day long.”

Chang's attitude reflects an ignorance of the often-delicate nature of pioneering science. The test section, the heart of the experiment where all the action takes place, is not an engineered component that can be mass-produced at will. The test sections that the Taleyarkhan group used to create a nuclear reaction, as well as the ones used by Xu, Forringer and Bugg, were hand-crafted by one or several glassblowers at Oak Ridge National Laboratory.

Here's the second key point: Once the glassblowers made and the researchers tested a section that worked, it continued to work until it wore out. The fact that Taleyarkhan had conveyed to a certain extent the parametric requirements to others (the glassblowers) supports the idea that the phenomenon is a genuine, legitimate effect of nature, not just a figment of Taleyarkhan and his group's imagination.

The test sections did not last forever; glass cracked, key properties changed as a result of the neutron activation, etc. The test sections that worked were more art than technology. This is the nature of pioneering research. All of this knowledge, with the exception of the Bugg and Forringer replications which came later, was available to Chang on March 8, 2006.

The larger problem that results from Chang's style is that the premise that theoretically he can print a correction later doesn't work. The reality is that, once Chang and the *Times* (as well as Reich and *Nature*) published, they had a strong disincentive to publish major corrections.

As I mentioned earlier, the Taleyarkhan group published its response to Naranjo on Oct.

6, 2006, in *Physical Review Letters*. The group constructed the physical conditions on which Naranjo, with "useful discussion and comments" from Putterman, based his speculation. However, experiment trumped theory, as evidenced by *Physical Review Letters'* rejection of Naranjo's preprint.

> Excerpt From BBC Horizon "An Experiment to Save the World"
>
> **"**Nothing is too wonderful to be true that it can't be reproduced in another experiment," Putterman said. "And this is what distinguishes science from religion."

The experimental claims have been reproduced by Xu/Butt, Bugg, Forringer and even Tsoukalas.

The Reich Articles
Now let's examine the Reich articles for instances of false or omitted facts. Reich covers some of the same ground as Chang did, so I will comment only on content unique to the Reich articles.

Reich/Nature: "Is Bubble Fusion Simply Hot Air?"
Reich begins the article with the statement that the Taleyarkhan group "reaffirmed that energy can be generated by nuclear fusion taking place in bubbles."

This is false; the authors said no such thing. Evidence for a fusion reaction is one thing; energy generation is an entirely different matter.

Reich talks about how Tsoukalas' "team looked for radioactive tritium, one of the likely products from the fusion of two deuterium nuclei" and obtained "negative" results.

The next problem is that, at the time of her article, the Tsoukalas group's paper had not published. As she had
by citing the Naranjo preprint, Reich took a tremendous risk and violated a time-honored tradition by writing a news story based on a paper that not only had not published but also had not been accepted for publication. The Tsoukalas group's paper was accepted for publication on April 11, and it published several months later, in August 2006.

The fascinating thing about the Tsoukalas group's experiment and tritium results is that, when the experiments were taking place and in the months immediately following the experiments, they viewed the work as positive. But two years later, when some of the Tsoukalas group decided to submit the paper for publication, their perspective shifted 180 degrees to a null result.

Reich wrote that, by the spring of 2004, the Tsoukalas "team had completed several experimental runs, but had not seen any evidence for bubble fusion." In contrast to Reich's reporting, the group's draft report from around May 2004 reported nine

experiments with excess amounts of tritium and one experiment with negative amounts of tritium.

Ironically, had the Tsoukalas group proceeded on course, it, rather than Xu/Butt, might have published the first independent confirmation of the Taleyarkhan group's work.

An interesting confluence of events occurred around the time Tsoukalas was feeding information to Reich.
- Feb. 7, 2006: Tsoukalas forms illegal committee to probe Taleyarkhan.
- Feb. 20, 2006: Reich initiates her first contact with Taleyarkhan on this story.
- Feb. 9-22, 2006: Tsoukalas' committee conducts five meetings of the illegal committee.
- Feb. 23, 2006: Tsoukalas' committee concludes its probe of Taleyarkhan.
- Feb. 28, 2006: *Nuclear Technology* receives the Tsoukalas group's paper, more than two years after the group did its work.

In e-mail communications from Taleyarkhan to me from April 30 to June 7, he explains more about the background of the inversion of the Tsoukalas group paper's bias and how the Tsoukalas group withheld positive data from the public.

Investigators can start with this quote from the end of Reich's article to probe Tsoukalas' intent and conduct: "In light of the growing concerns over Taleyarkhan's work, and his repeated claims of positive results, Tsoukalas says that he and his colleagues have now decided to submit their negative results for publication. On 1 March their paper was with the journal Nuclear Technology and was being prepared for peer review."

Reich wrote, "Xu confirms that the tritium data were taken using the equipment that Tsoukalas and Jevremovic were also working on before it was removed by Taleyarkhan. But Tsoukalas and his team maintain that they never saw any raw data from the equipment that corresponded to a positive signal."

The accusation by Tsoukalas of unauthorized removal of lab equipment by Taleyarkhan is contradicted by an e-mail from Tsoukalas to Taleyarkhan acknowledging and thanking him for the update on Taleyarkhan's transfer of said equipment.

Reich blamed the alleged removal of equipment for the delay in the publication of the Tsoukalas group's results. She wrote that "without their equipment they couldn't generate any more data" and that Tsoukalas wanted to "avoid a split in the faculty."

As for Reich's comment about Tsoukalas' alleged care for the faculty, the affidavits, particularly those from his administrative staff, that are part of the legal record in Taleyarkhan's suit against Tsoukalas provide a stark contradiction.

The excuse that the Tsoukalas group did not have enough data is contradicted by the fact that, by the spring of 2004, it had enough data to write a significant portion of the draft paper. In fact, of the numerous editing notations in the draft, none refers to a need

for more data. And the context of the draft paper is very clearly a positive interpretation of the data. A future *New Energy Times* investigative report will show that the quote about their inability to generate more data was false.

Paragraph 14 requires careful attention. Let's examine the first sentence: “The researchers were therefore particularly upset when, in July 2005, Taleyarkhan asked the press office at Purdue to issue a press release on ‘peer-reviewed’ and ‘independent’ results that were positive for bubble fusion.”

Several facts contradict the idea that at least Tsoukalas, the department head, was upset about the press release.

One is a document written by Emil Venere: “Although a department head ordinarily is not involved in approval of news releases, we consulted extensively with Lefteri Tsoukalas during preparation of this release. … In fact, Lefteri enthusiastically approved this news release in an e-mail message the night before it was issued.”

Tsoukalas had seen a draft in the morning and wrote the following one-line e-mail at 8:25 a.m. to Venere, Taleyarkhan, Tsoukalas' administrative assistant, and Xu and Butt: “Dear Emil, Thanks. Very well written indeed. Lefteri”

Twelve hours later, Tsoukalas confirmed the final draft of the press release in an e-mail to Taleyarkhan and Venere: “Dear Emil, Thanks, This is a great story and you did a great job. Cheers, Lefteri”

Taleyarkhan initiated contact with the press office to write a press release. He made such effort jointly with Tsoukalas, a fact that Reich omitted to obtain and report.

The e-mail from Taleyarkhan to Venere, copied to Tsoukalas, says, “Dr. Tsoukalas (Lefteri) and I have discussed these developments and feel that a press release from Purdue is in order before things get out hand since reporters are getting wind of this development. Unlike for *Science* or *Nature*, engineering journals do not make press releases - at least Elsevier's does not. We've prepared a brief description/checklist of questions and associated clarifications (see attached) mentioning key aspects of this seminal development that needs to be publicized rapidly.”

Venere's submission package to a Purdue investigation committee provides plentiful evidence of Tsoukalas' direction, involvement and approval of the press release. Despite the fact that Venere felt pressed for time and thought that the press release "won't be very good," the document trail shows careful attention to detail. Venere even had time to conduct a fascinating interview with Taleyarkhan on July 8, 2005, and visit one of the laboratories.

Continuing our forensic examination of Reich's prose, we find the following: “The claims raised serious concerns at Purdue because the paper in which the results appear was

written by two members of Taleyarkhan's own lab: postdoc Yiban Xu and master's student Adam Butt."

As Taleyarkhan has stated in his C-22 appeal/rebuttal, "Xu testifies that he requested review feedback and assistance related to paper preparation for publication from Dr. Taleyarkhan and several of his colleagues."
Xu was not a member of Taleyarkhan's lab when he performed the experiment because it didn't exist, as Xu wrote to a Purdue investigation committee: "I conducted my experiments (reported in NED and in NURETH-11) in Tsoukalas' laboratory over about 5 months (from January, 2004 through May, 2004). Prof. Taleyarkhan's laboratory did not exist at the time. His laboratory was eventually set up in an off-campus space."

Reich wrote, "Taleyarkhan's claim that there has been 'independent confirmation' of his results is repeated in the first sentence of his recent paper in *Physical Review Letters*."

Taleyarkhan and his co-authors asserted their opinion that the Xu/Butt confirmatory experiments were independent.

Reich/Nature: "A Sound Investment?"
I have not dug deeply into the patent angle of this story. What I can say from simply reading it, assuming that patent examiner Ricardo Palabrica has been quoted accurately and in context, is that Palabrica failed to understand significant aspects of the Taleyarkhan group's experiment, such as the difference between "cold fusion" and AICF.

Reich writes, "The examiner refers to press reports about the row that ensued when Science published Taleyarkhan's paper despite objections by the scientists who peer-reviewed it, as well as a study by Dan Shapira and Michael Saltmarsh, also at Oak Ridge, who had tried and failed to reproduce Taleyarkhan's results."

This statement is deceptive and misleading. It reads as though Shapira and Saltmarsh tried and failed to perform a replication. As explained by a quote from Taleyarkhan earlier in this article, they did not.

Palabrica expressed great skepticism about the tabletop fusion claim. Oddly, Putterman was able to obtain an AICF patent in 1997, yet Putterman wasn't, and still hasn't shown positive fusionlike results in his AICF experiments. Palabrica applied an uneven standard.

Reich/Nature: "Bubble Bursts for Table-Top Fusion"
In this article, Reich writes that "Putterman has been a key critic of Taleyarkhan's work since 2002."

Reich misrepresented Putterman's role. She failed to report that Putterman is and has been a competitor of Taleyarkhan's for government funding. She also fails to report Putterman's patent interests, that Putterman uses a different AICF method and that

Putterman has failed to achieve fusion in his AICF experiments. Reich writes, “In January, Taleyarkhan and his colleagues published further positive results, in which they again cited the detection of neutrons as evidence for bubble fusion.”

Reich, like many of the critics she has called on to provide authoritative opinions, omits the elephant in the living room: tritium. In their *Science* (2002) and *Physical Review E* (2004) papers, the Taleyarkhan group measured statistically significant increases in tritium.

Taleyarkhan's Nightmare, the End of the Beginning
This brings us only to the end of the beginning of Taleyarkhan's Nightmare. Readers who are curious about the rest of the story may browse the online collection of source documents in this library index or wait for the next installment of Taleyarkhan's Nightmare.

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5. Bubblegate Investigation* Timeline

* Including reviews and examinations

Web page with names and affiliations of key players

2006 Tsoukalas Committee

Feb. 7, 2006 - Tsoukalas creates committee to investigate Taleyarkhan. Investigation is performed without regard to C-22 policy.
March 8, 2006 - Allegations from Tsoukalas (to whom Taleyarkhan reported) and Jevremovic, along with allegations from Putterman, Suslick, Naranjo, are published in *Nature*. Allegations are made without regard for C-22 policy.

Result: Committee disbanded

2006 DARPA On-Site Review

March 1, 2006 - Defense Advanced Research Projects Agency review team carries out on-site review, discussion and observation of several of Taleyarkhan's experiments.
- Putterman insinuates that Taleyarkhan spiked experiments with californium-252.
- No evidence of fraud or falsification is found by any participant during the review.
- Suslick later claimed to *New York Times* that he saw evidence for fraud, but he has not publicly revealed the basis for such accusations. All investigative committees have dismissed Suslick's accusations of fraud.
- Taleyarkhan admonishes Putterman and Suslick to include intervening shielding (ice packs).
- Review team witnesses positive results; observer (Tessien) provides signed affidavit.

March 2, 2006 - On request of Tessien, Taleyarkhan repeats successful demonstration, as shown on Tessien's affidavit.

March 8, 2006 - *Nature* article publishes UCLA modeling results based on Putterman/Naranjo computer predictions.
- UCLA predictions not performed according to Taleyarkhan et al.'s experimental conditions.
- UCLA computer predictions fail to represent Taleyarkhan et al.'s experiment accurately.
- UCLA computer predictions later empirically disproved by Taleyarkhan group.

Result: DARPA sonofusion program terminated

2006 Purdue Examination Committee

~March 8, 2006 - Purdue establishes Examination Committee. Investigation performed outside of C-22 guidelines.

June 20, 2006 - Examination committee headed by Rutledge completes work, keeps matter confidential.

Result: Bertadano (Sept. 12), Tsoukalas (Sept. 5) formally accuse Taleyarkhan of misconduct; C-22 inquiry is called.

2006 Purdue C-22 Inquiry Committee

July 11, 2006 - Jamieson establishes Inquiry Committee.
Sept. 5, 2006 - Rutledge solicits allegations .
Sept. 5, 2006 - Tsoukalas makes formal misconduct allegations against Taleyarkhan in letter to Purdue (Dunn).
Sept. 12, 2006 - Bertadano accuses Taleyarkhan of misconduct in Letter to Purdue (Dunn).
Dec. 15, 2006 - Formal Purdue C-22 Inquiry Committee completes inquiry. All allegations (fabrication, falsification and plagiarism) are dismissed. Committee finds insufficient reason for recommending the formation of C-22 investigation committee and determines no research misconduct transpired.
Feb. 7, 2007 - Purdue issues press release exonerating Taleyarkhan of research misconduct.

Result: All allegations dismissed. Anonymous accusers prompt congressional investigation. Congress calls investigation

2007 Congressional Investigation

March 21, 2007 - Miller, in response to anonymous complaints and in association with Office of Naval Research inspector general, initiates congressional investigation.
May 7, 2007 - Miller committee completes report. Report does not make any charges of fraud or misconduct. Report states that Purdue failed to investigate matters properly and fully.

Result: Purdue starts new C-22 inquiry committee.

2007 Purdue C-22 Inquiry Committee

Aug. 27, 2007 - Purdue C-22 Inquiry Committee provides report to Office of Naval Research with 34 allegations. Identifies only 12 allegations that should be examined by Investigation Committee.
Nov. 1, 2007 - Purdue charges C-22 Investigation Committee to conduct investigation of those 12 allegations.

Result: 34 allegations collected, 22 dismissed, 12 forwarded to C-22 investigation committee.

2008 Purdue C-22 Investigation Committee

March 17, 2007 - Taleyarkhan receives copy of draft of C-22 Investigation Committee report and sees that it contains allegations A.2 and B.2. Allegations A.2 and B.2 were not among the 12 allegations that the Investigation Committee was charged to consider.
April 18, 2008 - Purdue C-22 Investigation Committee submits report to ONR.
July 17, 2008 - ONR accepts C-22 Investigation Committee report. Purdue completes C-22 investigation. Thirty-day appeal period begins.
July 18, 2008 - Purdue issues press release stating that Taleyarkhan was found guilty on two charges of misconduct based on allegations A.2 and B.2 even though appeal phase had not ended.
Aug. 27, 2008 - Purdue denies appeal by Taleyarkhan, issues sanctions and press release.

Result: Two allegations of research misconduct are affirmed by Purdue.

2008 American Physical Society (Physics Review Letters) Review

Sept. 25, 2008 - American Physical Society/*Physical Review Letters* reviews Purdue sanctions. Initial draft of response states that Taleyarkhan "acted to falsify the research record." APS informs Taleyakhan, "We are obligated to inform readers of the extent that the decision impacts [your group's 2006 PRL paper]."
Oct. - Nov. 2008 - Taleyarkhan group responds to APS.
Dec. 12, 2008 - APS completes review and publishes editorial note regarding Purdue sanctions. APS publishes editorial note stating, "A university investigative committee came to a conclusion that questions the validity of the [first] sentence in the opening paragraph [of the Taleyarkhan group's 2006 PRL paper]." Editorial note says nothing about falsification, fraud or deceit. APS neither demands nor requests retraction from Taleyarkhan group.

Results: In scientific record, Taleyarkhan group's claim for independent replication stands.

2008 UCLA-Purdue Investigation

Oct. 14, 2008 - Peccei and Dunn initiate investigation.
Dec. 2, 2008 - Taleyarkhan provides 830-page report and two boxes of hardware to Dunn (Purdue).

Results: Dunn's responsibilities as Purdue's research integrity officer removed on Nov. 3.

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6. Questions Sent to Adam Butt

(Butt failed to respond)

Mr. Adam Butt
[address redacted]
Sept. 19, 2008

Dear Adam Butt,

The following questions concern the document dated Feb. 23, 2006, as shown on page four of this letter.

1. Have you ever signed a copy of the Feb. 23, 2006, document?

2. If you have signed a copy, can you please provide me with a copy?

3. Was this document written by you?

4. Was this document written entirely by you?

5. Was this document written willingly by you and without undue influence?

6. Can you please describe the circumstances in which this document was created? (Why, where and how it was created. Did you enter it into a computer, or did someone transcribe your oral statements?)

7. Were you involved in any experiments at the Purdue INOK laboratory?

8. Were any of those experiments related to bubble/sonofusion?

9. Were you involved in any bubble/sonofusion experiments at any other Purdue laboratory?

10. Please consider the following facts:

- The first part of the Feb. 23, 2006 document is written in third person, and the second part is written in first person.
- The first part of the Feb. 23, 2006 document refers to Rusi Taleyarkhan as "Professor Taleyarkhan," and the second part refers to him as "Prof. T."?

Can you explain these discrepancies?
11. Please consider the following facts:

MEDIA APPEARANCES

- The Purdue news service represented in the July 12, 2005 press release (in the related text and in the photo of you and post-doctoral researcher Yiban Xu) that you were a key participant in the research.
- On Feb. 10, 2005, you gave a 10-minute videotaped lecture in front of Taleyarkhan, Lefteri Tsoukalas, Sally Mason, Linda Katehi and a dozen other people which gave the appearance that you were highly qualified in bubble/sonofusion research.
- Throughout the Feb. 10, 2005, presentation, you spoke of bubble/sonofusion experiments that you were personally involved with. For example, you started the presentation with the statement, "This is a project that Alex and I are working on."

MASTER'S THESES AND RESEARCH

- Evidence presented to New Energy Times shows that you referenced sonofusion experimentation that you were involved with on Nov. 26, 2004 for your master's thesis.
- Signed statements provided to New Energy Times from other former students discuss your participation in and expertise with bubble/sonofusion experiments.
- Information presented to New Energy Times indicates that your Project Summary submission of late 2004 stated that you were working on sonofusion experimentation.
- Information presented to New Energy Times indicates that three people in the School of Nuclear Engineering saw you during 2004 and 2005 regularly performing sonofusion experiments with Xu.

NED JOURNAL AND NURETH-11 PAPERS

- Several signed statements provided to New Energy Times indicate that you signed off on the transmittal letter to the Nuclear Engineering and Design (NED) journal.

- Signed statements provided to New Energy Times indicate that the work of the NED and Nuclear Reactor Thermal Hydraulics conference (NURETH-11) papers was part of your thesis and that you, in fact, took credit for it as part of your thesis.
- Signed statements provided to New Energy Times indicate that you appeared grateful for the opportunity to perform related work for the NED and NURETH-11 papers, that you were happy and willing to do so.
- A signed statement provided to New Energy Times indicates that you performed a review of the draft manuscript for the NED paper, you offered corrections and suggestions and you willingly and enthusiastically signed the NED journal transmittal letter accepting co-authorship.
- Several signed statements provided to New Energy Times indicate that you agreed to work on the bubble/sonofusion group's international conference paper for NURETH-11 in late 2005.
- A signed statement provided to New Energy Times indicates that your name was added as a co-author to the NED and NURETH-11 papers because of your participation in the data acquisition of sonoluminescent signals and that you were "thrilled and gratified" to be part of the team.
- A signed statement provided to New Energy Times states, "Butt, as an author, was likewise involved in several significant aspects of test cell construction, data checks, data analyses and conclusions as well as for manuscript reviews."
- A signed statement provided to New Energy Times states, "From my experience of over 10 years in research, it has been common practice to be as inclusive as possible, especially in relation to offering co-authorship to students to help them feel appreciated and credited for their efforts, even if not as significant as those of others on the author list."

Based on the above facts and information, do you think that your inclusion as an author of the NED and NURETH bubble/sonofusion papers was appropriate?

12. With regard to the first three items in the summary of the Feb. 23, 2006 document, do you think these items accurately represent authorship and credit of the NED and NURETH-11 papers in context?

13. Several of your former peers wrote signed affidavits that stated that you were excited to go to France to present the NURETH-11 paper but that you did not go because of a delay in the government's processing of your passport. With regard to the last two items in the summary of the Feb. 23, 2006, document, do you think these items accurately represent authorship and credit the NED and NURETH-11 papers in context?

14. Are there any other comments you would like to make for the record?

Please respond by 5 p.m. (Pacific) on Friday, Sept. 26 so that we may include your comments in a forthcoming New Energy Times article.

Sincerely,

Steven B. Krivit
Editor, New Energy Times

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7. *New York Times* Reporter Confirms Error in Purdue Story

New Energy Times has confirmed that the following sentence, published in *The New York Times* on May 11, 2007, is incorrect: *"Mr. Butt signed a statement that he did not participate in any of the experiments or the analysis of the data ... ."*

On Dec. 4, 2008, the article's author, Kenneth Chang, told *New Energy Times* that it "was bad wording on [Chang's] part." Chang confirmed that the Butt statement he had received was unsigned.

Chang was the first to publicly release the Feb. 23, 2006, Statement of Adam Butt. The document was used by several investigation committees, including one in Congress, as a key piece of evidence to fault Taleyarkhan.

Numerous signed, sworn affidavits, now publicly available, contradict the referenced statement by *The New York Times*. (Student identification has been redacted for their protection.)

On April 18, 2008, Purdue University's C-22 Investigation Committee decided that Rusi Taleyarkhan, a professor in the School of Nuclear Engineering, had committed research misconduct.

Allegation A.2 states that the name of Adam Butt, a former student at the school, was inappropriately added as an author to papers related to bubble fusion work performed at Purdue.

The allegation and conclusion stated that Butt had little to do with the research. The investigation committee, as well as a previous congressional investigation, relied heavily on the Statement by Adam Butt. The document allegedly was created by Butt as a result of an illegal committee organized by Lefteri Tsoukalas, who has since been removed as the head of the School of Nuclear Engineering.

The document states that Butt was not involved in any bubble fusion experiments and gives the impression that Butt had little, if anything, to do with the research.

The unsigned, un-notarized document does not state that Butt wrote it willingly or without undue influence. *New Energy Times* spoke with Butt on Sept. 12, 2008. He refused to answer questions. *New Energy Times* mailed a letter with questions to Butt on Sept. 19, 2008. He failed to respond to the letter.

Taleyarkhan was present during the Jan. 31-Feb. 2, 2008, cross-examination hearings and Butt did not fare well when cross-examined about his Feb. 23, 2006, statement, according to Taleyarkhan.

"Butt was shaken up when reminded of having completely overlooked the documents he had submitted for his project work," Taleyarkhan wrote, "and the many close interactions he had with [Yiban] Xu for test cell construction for bubble fusion work."

Taleyarkhan wrote that Butt was cross-examined about the fact that he agreed well in advance to participate as co-author for a paper in January 2005.

"The statement given by Butt to the illegal investigation committee set up by Tsoukalas," Taleyarkhan wrote, "is vividly contradicted by the signed sworn affidavits/statements from Xu, Revankar, and students [REDACTED] and [REDACTED]."

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8. Peter Zimmerman, Bubble Fusion Skeptic Retreats

This summer, Peter Zimmerman, an emeritus professor of science and security at King's College London, went on the offensive and publicly attacked the research and character of Purdue professor of nuclear engineering Rusi Taleyarkhan.
Zimmerman is a former U.S. State Department adviser on arms control and was involved in national security for many years. He was also one of a dozen people on a federal committee that drafted the rules regarding standards of research integrity.

In an Internet discussion list, Zimmerman wrote that Taleyarkhan had committed fraud and deceit, and Zimmerman insinuated that Taleyarkhan was incompetent.

Zimmerman cited the April 18, 2008, Purdue C-22 investigation report and the alleged statement by Adam Butt as his prime sources for information.

Some of the statements Zimmerman made are as follows:

> "On two counts the [investigation] panel [found] Taleyarkhan dead to rights with research misconduct."
>
> "Taleyarkhan put the name of Adam Butt on a paper to dispel one referee's blunt criticism that the paper had been done entirely by Yiban Xu."

> "Taleyarkhan had Butt added even though Butt was a masters student and not competent to be much more than a clerical assistant. That's fraud."
>
> "Much worse, Taleyarkhan advised *Physical Review Letters* that an earlier paper in *Science* had been 'independently concerned.' One may quibble as to whether Taleyarkhan had fraud in mind, but the effect was to deceive the editors of *Physical Review Letters* ".
>
> "There are many other allegations where the C-22 [investigation committee] found factual basis for criticizing Taleyarkhan's work, blasting it actually."
>
> "[These] actions cast very serious doubt on Taleyarkhan's competent performance of his experiment, and absolutely demonstrated that Yiban Xu did not do his 'confirmatory' experiment independently. Rather, he used Taleyarkhan's apparatus and had Taleyarkhan, his former PhD adviser, looking over his shoulder and Xu knew what results were expected."
>
> "There is no grand conspiracy here; just a nasty debate between Taleyarkhan and his two students on the one hand and several very competent external scientists on the other hand."

On Sept. 10, *New Energy Times* asked Zimmerman some questions regarding the basis for his allegation of fraud, the Adam Butt document.

On Sept. 11, Zimmerman responded that he did not have access to the Adam Butt document, which implied that he had not read it. Zimmerman also deflected questions from *New Energy Times* and redirected the conversation toward technical matters regarding the bubble fusion research.

"The argument is simply over whether or not Taleyarkhan and his colleagues did a difficult experiment correctly or whether they screwed up," Zimmerman wrote. "Other [researchers] with better reputations in the community for doing difficult experiments correctly have tried to reproduce Taleyarkhan, et al. but have failed. The obvious conclusion is that Taleyarkhan and his group screwed up somewhere. My bet is that their neutron detectors were sensitive to the vibration ... from the bubble collapses. But I could be wrong. There are other problems, and counting neutrons is a really tough job.

"It is distressing to see Taleyarkhan and company press this (a) as if they had the key to a major source of energy, (b) as if they were being persecuted, and (c) they had done the experiment correctly, and that Putterman, et al. were screw-ups."

The same day, Taleyarkhan provided a detailed technical response to Zimmerman [*Taleyarkhan's original message has been edited for clarity*.]

Gentlemen:

With due respects to all and to Prof. Zimmerman, I respectfully offer a few notes and comments to correct the record associated with the science and technology of bubble (acoustic) nuclear fusion as published by myself and my colleagues Dick Lahey, Bob Block, Colin West, Robert Nigmatulin and JaeSeon Cho.

I leave it to Krivit and others in the press to collect the data Krivit writes about along with viewpoints. It may be useful for individuals like Zimmerman to review Krivit's *Special Report on Bubble Fusion* to better appreciate the human dimension, although much remains undisclosed.

1) Regarding the Bubble Fusion Papers by Taleyarkhan, West, Cho, Lahey, Block, Nigmatulin and Xu

— The fusion signal detectors we used were not only the active (electronic-based) types but also passive neutron track detector variety. In fact, the *Physical Review Letters* study used four separate detector types: three for neutrons, one for gamma signals.

— Control experiments were conducted, in each case keeping the same detectors, the same acoustic drive trains, etc., but only changing one parameter; changing the hydrogen-bearing liquid to a deuterium-bearing liquid. Additional experiments were also conducted for each hydrogen-bearing and deuterium-bearing liquid without acoustic drive. Only when the liquid was deuterium-bearing did the data provide 17 to 20+ standard deviation statistically significant measured values.

— Not only were 2.45 MeV neutron emissions measured at 17-25+ standard deviations, but these were measured in time-correlation with sonoluminescent light flashes.

— Gamma emissions in much lower amounts than the neutrons were also measured which occurred in between the neutron and sonoluminescent bursts, as would be the case after slowing down and being captured in hydrogen or other elements like chlorine since deuterium-deuterium fusion does not produce gamma photons. Gamma photon energy is consistent with expected energies of neutron capture in hydrogen and chlorine.

— Tritium, in statistically significant quantities - about four standard deviations - was also measured in addition to neutrons and gamma signals. Tritium was only measured when a) the test liquid was deuterated, b) while it was cavitating and c)

when the thermal hydraulic conditions were in the specific range. Tritium generation and monitoring is something that has no bearing on acoustic-drive related interference. Control conditions did not result in tritium. Tritium emissions are consistent with neutron emission levels within experimental error.

— Theoretical simulations of the conditions of our 2002 *Science* paper and the 2004 *Physical Review E* paper and the 2005 *Physics of Fluids* paper by Robert Nigmatulin et al. have provided physics-based backing for the potential for attainment of thermonuclear fusion conditions at the rates measured in the 2002 *Science* and the 2004 *Physical Review E* publications. Furthermore, the same theoretical framework provided input as to when not to expect success; which was then found experimentally and published in our group's 2006 *Physical Review Letters* publication.

— In 2006, Eugenie Samuel Reich, writing for *Nature*, published egregious charges against Taleyarkhan alleging fraud and fabrication - all based on a single, non-peer reviewed Web-posting by UCLA researcher Brian Naranjo. Three investigation committees during the past three years have looked into these charges and all three have rejected them for lack of merit. More than 34 allegations were made against Taleyarkhan over the past several years and all 34, including the ones on fraud/fabrication, have been rejected. Unfortunately, the April 18, 2008, Purdue C-22 investigation committee report does not include this revelation since these charges were already dismissed by earlier committees. Also note for the record that the Department of Defense's Office of Naval Research has formally accepted the conclusions of these three investigations.

— The charges of use of Californium-252 made by UCLA researchers has also been addressed scientifically in the Taleyarkhan et al. group's recent manuscript published in Nuclear Engineering and Design. This is besides the obvious flaws that were explained already in the published rebuttal to UCLA in *Physics Review Letters* in October, 2006.

— Finally, in this arena, professor Zimmerman misconstrues the Purdue 2008 C-22 investigation committee report's statements related to expertise in neutron monitoring by Taleyarkhan and Block. The statements made in the [2008] investigation committee report deal not with the work presented in the group's published papers but with a single occasion when an Oak Ridge National Laboratory (ORNL) group visited Purdue in 2003 - at Purdue's invitation - in their initial stage of getting up to speed on this research.

Scoping trial experiments were done for neutron measurements without the usual precautions and time-bearing steps for careful calibrations, etc. Those specific data were never intended for publication since such would require careful screening for background noise, etc. Such effects would also require control experimentation. The investigation committee report mentions and acknowledges this aspect.

The neutron and tritium monitoring methodology and approaches (as we used for deriving the published data in *Science* (2002), *Physical Review* E(2004) and *Physical Review Letters* (2006) has been reviewed in person by recognized stalwarts and pioneers of the field. These include Jack Harvey, Michael Murray, William Bugg, Bob Fleischer and other modern-day practitioners such as Larry Miller of the University of Tennessee-Knoxville, and Glenn Young, physics division director at ORNL. This is in addition to the neutron- monitoring expertise possessed by Bob Block (formerly of ORNL) and other researchers like Colin West. All concerns related to neutron pulse shape discrimination work as published by the group in *Science* (2002) were addressed in later publications and there has not been any technical challenge since that time to the neutron-tritium measured data published by the Taleyarkhan group.

2) Confirmation Studies (successful and not)

— For the one situation where Putterman, Naranjo et al. failed in their attempt, there are others which were indeed successful and which have been entered into the public domain (with signed testimonials and affidavits):

1. 2005 Nuclear Engineering and Design paper by Yiban Xu and Adam Butt.

2. 2005 Nuclear Reactor Thermal Hydraulics conference 11 Proceedings paper by Xu, Butt and Shripad Revankar.

3. 2006 Proceedings of the International Conference on Fusion Energy by Edward Forringer et al.

4. Report to Purdue University of successful replication experiments of the 2006 *Physical Review Letters* paper -by William Bugg (retired Head of Physics Univ.Tenn, Knoxville and presently a research professor at Stanford University)

5. Successful public demonstration on March 1, 2006. This was the all-day demonstration of bubble fusion in full view of the following visitors: Bill Coblenz from DARPA; Graham Hubler and Peter Schmidt from the Office of Naval Research; Ross Tessien, Felipe Gaitan and Wylene Dunbar from Impulse Devices Inc.; Ken Suslick and another one or two people from the University of Illinois; Seth Putterman and perhaps one more person from UCLA; JaeSeon Cho from Oak Ridge, plus about five to ten Purdue students and staff. Neutron production data were obtained and viewed by the various visitors that afternoon commensurate with the data published by our group in our *Physical Review Letters* 2006 paper of Jan.2006.

6. Testimonial of successful public demonstration on March 2, 2006. This is a distinct and repeat demonstration of the demonstration from March 1, 2006. This was performed at the direct request of Ross Tessien of Impulse Devices Inc. He was present at all times during the experiment; from start to finish.

— It is true that the Putterman et al. group published a paper in *Physical Review Letters* declaring they could not reproduce the data as published in Science (2002) and *Physical Review E* (2004). This is traced to non-attainment of the experimental conditions that were attained in the successful studies; *Science* (2002), *Physical Review E* (2004) and *Physical Review Letters* (2006). The mode of sonoluminescent production, degassing, acoustic profiles and importantly, the shape and rate of bubble clusters were completely incorrect at UCLA.

Video records were transmitted to the Taleyarkhan et al. group from UCLA/UIUC for feedback. Based on what we saw in these videos, we advised them to not expect success under these circumstances. We saw that the bubble cluster shapes in the UCLA experiments were elongated and nonspherical. They also deliberately added non-condensible gases to increase sonoluminescence emission levels, which is opposite of that utilized by Taleyarkhan et al. Indeed, in the 2005 NED/NURETH-11 papers by Xu, Butt and Revankar they specifically demonstrate that under the UCLA experimental conditions successful D-D bubble fusion will not occur. This is not to say that Putterman et al. are poor scientists; this has never been stated by any of Taleyarkhan's co-authors. Just that Putterman et al. did a different experiment. Their expertise-base has been developed around single(gas)-bubble sonoluminescence, not in on-demand nuclear-scale nucleated bubble clusters in degassed liquids.

—Yes, a scoping attempt was also made in early 2001 by physicists Daniel Shapira and Michael Saltmarsh to measure nuclear emissions while Taleyarkhan was at ORNL. What is not appreciated is that Shapira and Saltmarsh did not run their experiments over months or even days. They made a single measurement [on one of my experiments] on a single afternoon over the period of about one hour while they were alone in Taleyarkhan's laboratory, using a different liquid scintillation detector.

Indeed, they did obtain a statistically significant result for nuclear emissions for excess nuclear emissions time-correlated with the region of time when deuterated bubble clusters were imploding. Shapira or Saltmarsh did not investigate for tritium nor did they conduct control experiment-related measurements with non-deuterated liquid. Because of the large size of their detectors, there were electronic issues related to gamma saturation in some time regions. The volume of the Shapira-Saltmarsh detector was 30 times larger than the one used by the Taleyarkhan team. Large does not always mean better since it can become readily overwhelmed, and this is what transpired for the Shapira-Saltmarsh detector system.

A review of the raw data has already been published as a reference in the Science (2002) paper. However, the media has distorted this scoping attempt by Shapira-Saltmarsh who are both accomplished scientists in their own right. The data obtained by the Taleyarkhan team were reviewed at length by ORNL management, taking assistance from resident expertise and eventually, after many tens of in-depth assessments, ONRL decided that the work and data obtained were on solid ground. (See comments from Glenn Young - director of the physics division at ORNL in 2004 to the New York Times and also the ORNL position statement in consultation with Prof. Lee Riedinger, executive vice president for science and technology at ORNL, both of whom participated in in-depth reviews and audits.) ONRL also took into account - for due diligence - the Shapira-Saltmarsh scoping attempts. The Shapira-Saltmarsh effort was not "experimentation."

Yes, neutron detection is non-trivial. The same goes in spades and more so for conducting bubble fusion experimentation and controlling the shape and implosion of not one but clusters of hundreds of bubbles.

In no instance that this author is aware of, have either Taleyarkhan or any of the co-authors ever mentioned that this method for attaining bubble fusion in deuterated liquids is ready for immediately resolving the world's energy problems. Many issues

will need to be confronted and overcome in terms of scaling to breakeven before one is confident enough to make such claims. Unfortunately, the simplistic sound-bites of much of the media provide a distorted view of reality.

3) Regarding the work of Xu/Butt/Revankar

— Yiban Xu was NOT a doctoral student of Rusi Taleyarkhan, as stated by Zimmerman, nor was Xu his major advisor (Mamoru Ishii). Taleyarkhan had a courtesy, last-minute impromptu appointment on Xu's thesis defense committee - something requested of him by Ishii which constituted nothing more than to review his Ph.D. thesis, give comments for completeness and hear his defense. The Taleyarkhan rebuttal to the 2008 C-22 investigation committee report includes this clarification but Purdue has not, as of yet, corrected their report. During the time Xu conducted his studies, he was under the direction and sponsorship of Lefteri Tsoukalas, who served as principal investigator and who paid for Xu's salary from external sponsored funds.

— The laboratory where Xu worked was under the direction of Tsoukalas. The test cell equipment, the mode of nucleation, the protocol for nuclear emission detection, the test fluids, the drive train, the calibrations, the detection equipment, the thermal hydraulic enclosure that were used by Xu were all different, and in some cases radically so, from the ones used by Taleyarkhan at ORNL. Taleyarkhan nor anyone else from the original team participated in any of the Xu experiments, the data analyses, the drawing of conclusions or the drafting of the manuscript of Xu, Butt and Revankar. Yes, initial consulting was provided to Purdue from ORNL and that was acknowledged by Xu, Butt and Revankar in their publications. Xu and Revankar both have issued and signed a sworn affidavit to this effect. Every investigation committee that has looked at the facts and evidence have come to this conclusion.

— The only statement made in the Taleyarkhan et al. group's 2006 *Physical Review Letters* paper as related to the Xu, Butt and Revankar paper was: these "observations" have now been independently confirmed. The observations indeed were independently confirmed by Xu. The entire author team felt this was legitimately the case and maintains this till today. All co-authors have sworn statements to this effect and see nothing wrong. These facts were omitted in the C-22 investigation committee report but are present in the Taleyarkhan rebuttal correction which Purdue has not publicized.

— According to sworn affidavits of Xu, Revankar and several others, Xu worked over several months with Butt on conducting sonofusion studies. It was Xu's initiative to respond to a referee's comment that the work be looked at by a separate person upon which Xu took the initiative to request Butt for his participation; Butt was then asked by Taleyarkhan if he would like to do so and upon agreement, Butt then worked with Xu to perform cross-checking, re-analyses of data/drawing of conclusions, etc.—to Xu's satisfaction not Taleyarkhan's. The affidavits of Darla Mize and other administrative staff shed light into the human dynamics of the generation of Butt's statement.

— The lack of neutron detection capability by Xu for monitoring fast neutrons with pulse shape discrimination (gating of gammas)—before undertaking his confirmatory studies—is asserted in the 2008 investigation committee report. Yes, he did not have that specific expertise before his attempts but he was a 15-year veteran nuclear engineer from his days in China as an assistant professor from Tsinghua university. At Purdue, he picked up what he needed to learn from individual faculty at Purdue (not only from ORNL) as well, he learned about tritium monitoring from ORNL experts. This lack of prior expertise in these specific areas of neutron-tritium detection is misconstrued. It is not what a person knows but what he/she accomplishes after several months of full-time work with assistance/counsel from others. From there, the person describes their work in a manuscript, it then gets reviewed for the attainment of standards of scientific merit by experts chosen by the editor of a respected journal. No one has questioned the technical merit of the Xu/Butt/Revankar technical publications. It is unfair to judge someone for their past alone.

4) Final notes

It is to be noted that the two charges for which Taleyarkhan has been sanctioned are not present in any of the 34 allegations made for the round of investigations that concluded in 2008. This investigation was initiated by Congressman Miller in early 2007. These two allegations were inserted into the investigation process against the specific instructions to the investigation committee and Purdue charter for research integrity.

The same two charges that were inserted into the 2008 investigation committee final report (against all rules governing the process of investigation) were already ruled upon in 2006 by a duly appointed committee of distinguished peers—looking at the same facts and evidence. That review/investigation of facts—without political pressure from Congress—had exonerated Taleyarkhan.

Perhaps at some point Steven Krivit or some other reporter will also reveal to the public that Taleyarkhan received threats of punishment in early 2008 that were to be commensurate with the degree of lack of cooperation by Taleyarkhan with Purdue in their dealings with the federal government. Specifically, pressure was brought to bear on Taleyarkhan to agree that he had been afforded due process. All of this is despite the fact that issues of true outright fraud, discrimination and reprisal - as stated by several other employees of Purdue -were not included in the investigation.

5) Personal notes

If this all sounds egregious, amazing and unworthy of occurrence, I concur. I too, have been amazed at the twists and turns of the past several years. I leave it up to others to help set the record straight. As things stand, I bear the main brunt of the sanctions.

Only a person subject to such experiences as these can appreciate the toll it takes on family, health, finances and to the progress of science.

I have included my co-authors on the cc list so they may comment as appropriate and correct the above clarifications and notes for the record.

Respectfully submitted,
Rusi Taleyarkhan

On Sept. 12, Zimmerman, after reading what Taleyarkhan explain, replied as follows:

"I will review your papers and that of Putterman, et al. when I have time. As I said, I do not understand the attitude ... that sonofusion is some mysterious sound-mitigated new physics. It has always been within the realm of possibility that fusion conditions could be achieved in the collapse of a bubble containing deuterium from a large enough to a small enough diameter. After all, it works in the sun and in hydrogen bombs, although the size scales are much larger."

After Taleyarkhan wrote to Zimmerman, *New Energy Times* wrote to Zimmerman, "This thread started with your public statement that Taleyarkhan had committed fraud. Are you still so certain of that?"

Zimmerman began to question his assumption of the Butt Statement.

"Assuming that the statement of Adam Butt is correct," Zimmerman wrote, "then there was some research misconduct."

On Sept. 12, Zimmerman wrote, "I will have some interesting information you can quote if you will agree to go off the record on some other stuff. How say you?"

New Energy Times agreed to his request and received one off-record e-mail from Zimmerman. It did not contain anything useful or interesting and appeared to be a ploy from Zimmerman to either win favor or obtain information the *New Energy Times* investigation that had not yet been published.

New Energy Times wrote Zimmerman an e-mail on Sept. 13. Here are some excerpts from the letter:

Peter Zimmerman,

In your June 28 e-mail to the Hydrino Study Group e-mail list, you stated that you are "one of perhaps a dozen people on the committee that drafted the federal rules" on the standards of research fraud.

Why were you were so confident that the Adam Butt document provided proof of fraud on the part of Rusi Taleyarkhan, although, according to your Sept. 11 e-mail, you had "no access" to the Adam Butt document?

You wrote that Taleyarkhan intended to "deceive the editors of Physical Review Letters." *Do you want to remain on record as making this statement?*

You wrote that the Purdue C-22 research misconduct investigation committee "found factual basis for criticizing Rusi Taleyarkhan's work" and that this "cast very serious doubt on Rusi Taleyarkhan's competent performance of his experiment."

One of the main pieces of information used for criticizing Taleyarkhan's work was the alleged replication work performed at UCLA by researchers Seth Putterman and Brian Naranjo. In that experiment, the UCLA team deliberately added noncondensable gases to the experiment; this guarantees a failure.

This fact—and others like it—was in the public record. As Taleyarkhan wrote to you on Sept. 11, "in the 2005 NURETH-11 [Nuclear Engineering and Design and the Nuclear Reactor Thermal Hydraulics conference] papers by Yiban Xu, Adam Butt and Shripad Revankar, they specifically demonstrate that, under the UCLA experimental conditions, successful deuterium-deuterium bubble fusion will not occur." *New Energy Times* also published these facts on July 10, 2007, in "New Energy Times Special Report on Bubble Fusion/Sonofusion."

Do you want to remain on record as making the statement (shown above) regarding "factual basis for criticizing" Taleyarkhan's work and competence?

You wrote that the "Taleyarkhan group did not have a clue how to measure small neutrons or neutrons to high precision and made elementary experimental errors." *After reading the e-mail Taleyarkhan sent to you on Sept. 11, do you want to remain on record as making this statement?*

On Sept. 14 Zimmerman wrote back to *New Energy Times* and said that he would need to do some more reading, but it would take some time because he had a backache.

Web pages related to Peter Zimmerman:
http://en.wikipedia.org/wiki/Peter_D._Zimmerman
http://newenergytimes.com/Inthenews/2006/PatentOfficeToReinstate.htm
http://www.imaginaryweapons.net/Peter_Zimmerman_gets_political.html

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9. Taleyarkhan Debunks UCLA Accusations

[The following e-mail exchange took place on March 1, 2007, one year to the day after scientists from UCLA at a Department of Defense onsite review at Purdue accused Rusi *Taleyarkhan of fraud. The accusations, primarily made by Seth Putterman and Brian Naranjo of UCLA, implied that Taleyarkhan had fabricated his group's results - published before the onsite review - by spiking his experiments with a standard laboratory radioactive source, californium-252.]*

Steven B. Krivit: In my interview with you on Aug. 1, 2006, you mentioned that Ross Tessien and two other people from Impulse Devices were present on March 1, 2006, at the Defense Advanced Research Projects Agency onsite review. Were these two other Impulse Devices people also present and witness to the repeat of the experiment on March 2?

Rusi Taleyarkhan: The other two individuals from Impulse Devices were present on March 1 only; Tessien was present by himself on March 2. The other two people were also free to join, but they mentioned that they had other commitments back home and could not stay. I'm glad now that Tessien signed the acceptance of results that came out on March 1 as well as March 2.

SK: Has anybody asked the question, "How could your experiments have been spiked with californium on both of these days with witnesses present?"

RT: No. The air on March 1, 2006, was too clouded by Seth Putterman (UCLA) and Ken Suslick (University of Illinois) with their surprise announcement of code calculations which reporter Eugenie Reich (*Nature*) got to hear about thereafter or perhaps before. The obvious results [that we demonstrated on that day] were overlooked even with a dozen-odd people acting as witnesses.

SK: Has anybody even suggested that the demonstrations on March 1 and 2 were also the result of californium contamination/spiking?

RT: The obvious is overlooked in the feeding frenzy caused by the alleged computer code calculations for an imaginary experimental setup by UCLA'S Putterman and Brian

Naranjo. Claiming Cf rather than some other source like Pu or an accelerator-based source was convenient for them since the average neutron energy from Cf comes close to the deuterium-deuterium fusion energy of 2.5 MeV.

However, one crucial item that was overlooked by UCLA/Suslick relates to gamma emissions. A Cf source emits about four to five times more gammas than neutrons. For bubble fusion, on the other hand, the ratio is almost exactly the opposite. You get only about one gamma for every 10 neutrons. This fatal flaw/oversight was pointed out [by our group] to the referees and published in our October 2006 rebuttal in *Physical Review Letters* [1].

Fortunately, we don't have to argue about neutron-gamma emissions from Cf. As I've explained earlier, folks with conflicts of interest can be relied on to cherry-pick as convenient. As Brian [Josephson, Cambridge University] points out, teamed with a hungry reporter such as Reich, the mix is potent.

The Cf argument is moot for a more obvious reason; two other groups [Bill Bugg and Ted Forringer] whom I had never worked with in the past, taking several days each, performed their own experiments and successfully reproduced the spectra type we published in January 2006. These groups had no conflict of interest, and they were capable and experienced nuclear physicists with a sincere interest in finding out for themselves whether the *Nature* allegations had any merit.

They came to Purdue and ran their own experiments here with their own grant funding and resources. They ran their experiments using their own detectors after deliberately verifying the absence of Cf or, for that matter, any other neutron source. They obtained data - with up to nine standard deviations statistical significance - using two independent detector systems. They conducted their own analyses and wrote their own reports, which were peer-reviewed and accepted for presentation/publication at two international conferences in November 2006 (Bugg's report was sent to Purdue directly). The absence of any extraneous source has been specifically cited in their reports.

The obvious question here again is perhaps your very own: Has anybody even suggested that the experiments conducted during May and June by the Bugg and Forringer groups were also the result of Cf?
[Note: Reporter Eric Vance of the *Chronicle of Higher Education*, in an April 2, 2007, article "The Bursting of Bubble Fusion," deduced another problem with the Putterman/Naranjo accusation: "In order for this new [Cf-252 spiking] claim to be true, 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." For example, when the control experiments were running.]

SK: Has Putterman or Naranjo retracted his accusations of californium spiking?

RT: Not to my knowledge.

SK: Has Nature/Reich published a correction to the record with regard to their publication of Putterman and Naranjo's accusations of californium spiking?

RT: Again, not to my knowledge.

Reference

1. Taleyarkhan, R.P., Block, R.C., Lahey, Jr., R.T., Nigmatulin, R.I., and Xu, Y., "Reply to 'Comment on Nuclear Emissions During Self-Nucleated Acoustic Cavitation,'" Physical Review Letters, Vol. 97, p. 149404, (Oct. 3, 2006)

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10. UCLA-BBC Experiment is "Red Herring," According to Taleyarkhan Group

On Feb. 16, 2005, Seth Putterman appeared on a BBC Horizon television program and implied that he disconfirmed the Taleyarkhan group's external neutron source acoustic cavitation experiment. The Taleyarkhan group calls it bubble fusion. The Taleyarkhan group has told *New Energy Times* that the BBC show had little bearing on and was a diversion from the reality of their work.

The experiment was performed between Oct. 7 and Oct. 10, 2004. On Jan. 13, 2005, Brian Naranjo, apparently the lead researcher on the project, sent a summary of the group's work to the BBC.

The same day, Colin Murray, the producer of the show, informed Rusi Taleyarkhan that the group had failed to replicate his work, and West requested comment from Taleyarkhan.

On Jan. 17, Taleyarkhan sent his feedback to Murray.
"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," Taleyarkhan wrote, "all appear radically different from those used for the reported results by Taleyarkhan et al. in *Science* (2002) and in *Physical Review E* (2004).

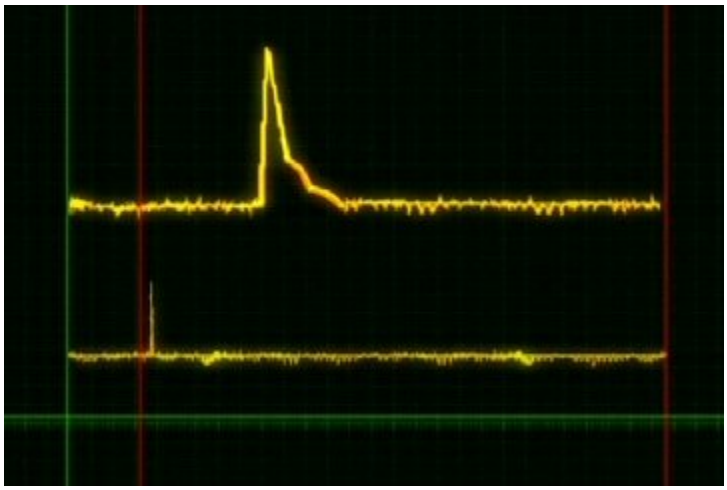
"The UCLA scoping experiments demonstrate yet another set of experimental parameters that may not lead to significant nuclear fusion during neutron seeded acoustic cavitation and could be useful as a guide for other researchers in the field. For the record, I had cautioned BBC about the potential perils of sponsoring such an undertaking, however noble the motive."

Although it is unusual to see science - let alone an important subject such as sonofusion - vetted on television, Putterman gave *The New York Times* his reasons on March 15,

2005: "'I'm desperate for money, and here's a chance to infuse my laboratory with overhead-free money,'" Dr. Putterman said. "'We had fun.'"
In the UCLA-BBC experiment, Putterman claimed that both Taleyarkhan's and his own team's attempted replication failed to show temporal synchronicity between the sonoluminescence flash and the neutron signal within a sufficiently precise time window.

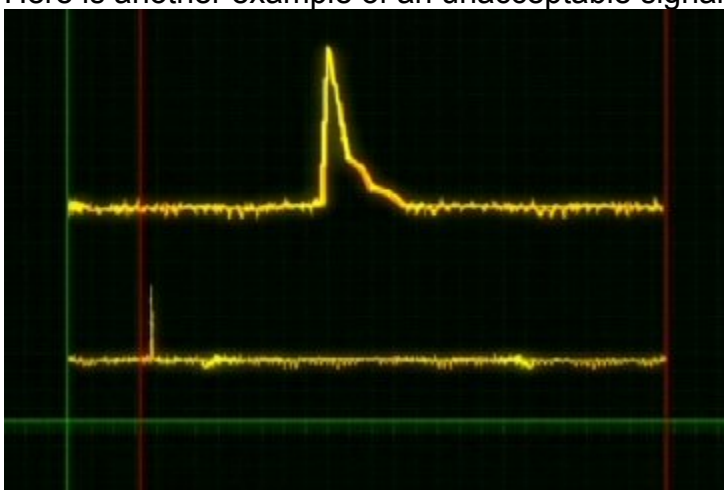
The images below are captured from the BBC film. The top yellow line indicates the neutron signal. The bottom yellow line indicates the sonoluminescence signal. The faint red lines indicate the precision of the timing window of the detector.

According to the film, the window, as shown below, is unacceptable and would fail to confirm the bubble fusion claim because it does not enforce a precise enough coincidence in the temporal synchronicity of the two signals.

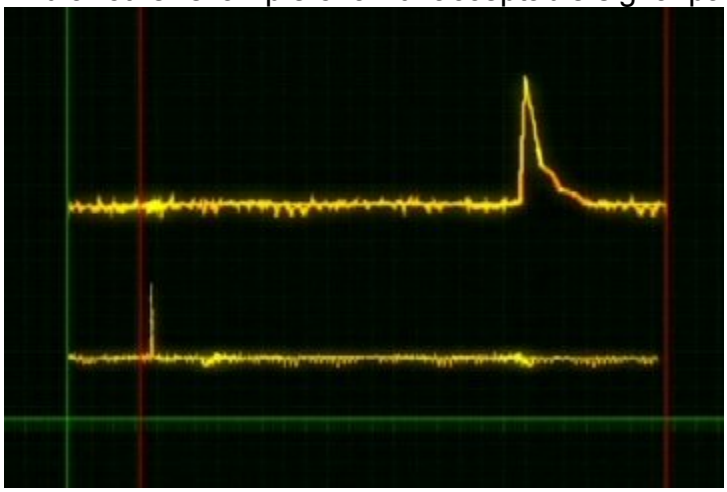


According to UCLA-BBC, if the timing window is greater than one nanosecond, the source of the neutrons can't be from the external neutron generator, and the neutrons were indeed from bubble fusion. Stated another way, in his opinion, Putterman, a timing coincidence of one nanosecond would constitute proof of bubble fusion.

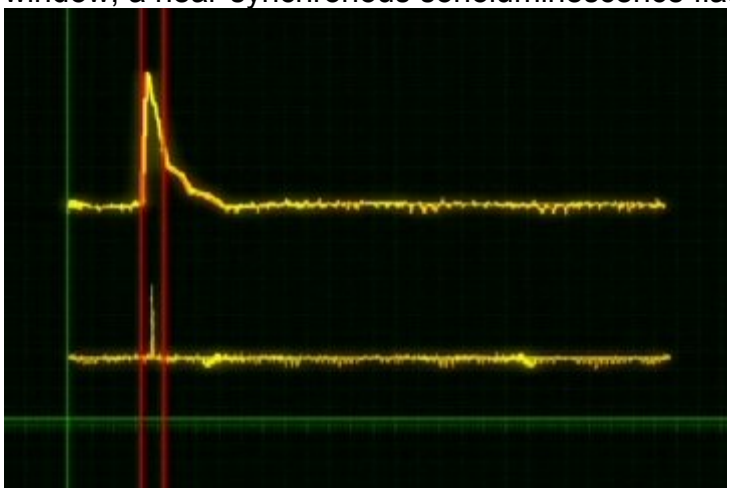
Here is another example of an unacceptable signal pair, according to UCLA-BBC:



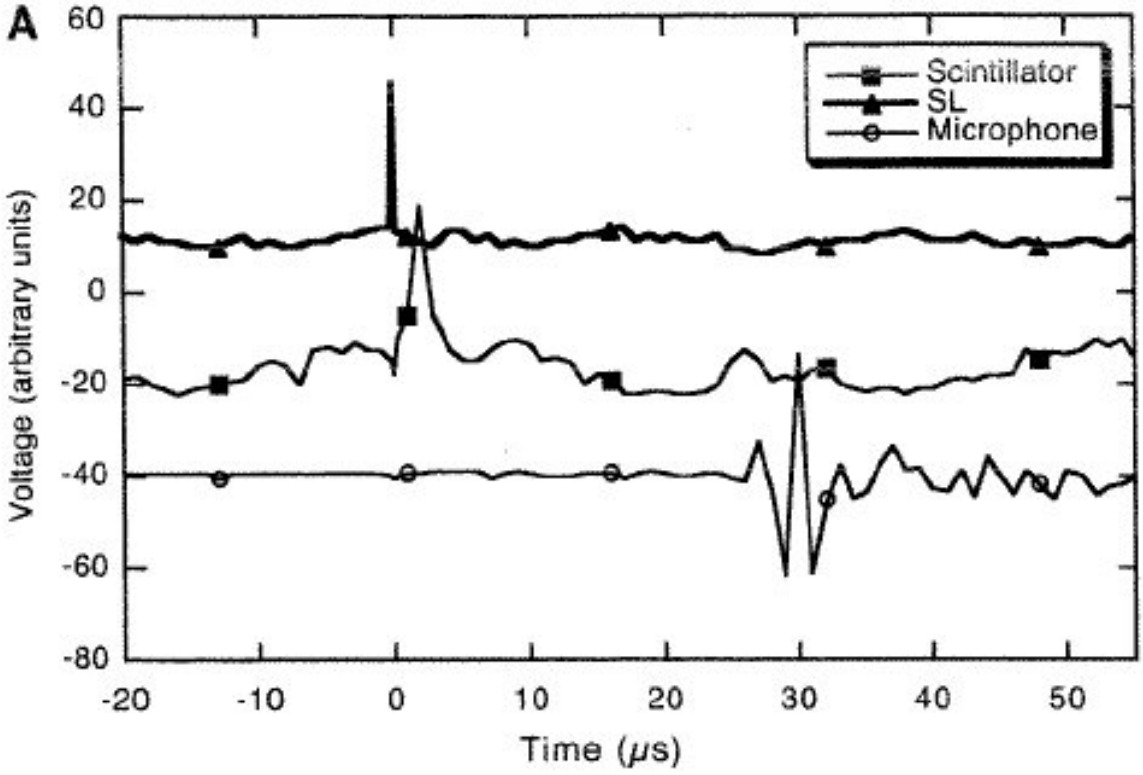
And another example of an unacceptable signal pair:



Then BBC showed what it would consider an acceptable timing window. In this narrow window, a near-synchronous sonoluminescence flash and neutron signal are observed.



The Taleyarkhan group says its signal pairs are temporally synchronous within 10 nanoseconds, as shown from Figure 5A (below) in the group's seminal paper from *Science* (2002).



According to the visual examples provided by UCLA-BBC, their demand for a nanosecond timing window is met or, if not, is within a hair-splitting difference.

On Nov. 12, 2008, *New Energy Times* asked the Taleyarkhan team to consider two questions:

1. On the matter of the nanosecond timing window, what is its response to UCLA-BBC's verdict that both the Putterman group replication attempt and the Taleyarkhan group replications were disconfirming?
2. Is the difference between one nanosecond and 10 nanoseconds critical as a requirement for confirmation?

"Putterman's story that sonoluminescence flash/neutron detection coincidence is the only way to prove there is sonofusion is rubbish," Colin West wrote.

Instead, West said, asking questions of Putterman and his associate Ken Suslick would be more worthwhile.

"I would rather we challenge the failures of Putterman and Suslick," West wrote. "Why have they not made a resonant chamber that shows the same performance as ours. For example, where are the 'clicks?' Why is the effect of a surface float different in their apparatus from the behavior in either my old Harwell chambers or the newer Taleyarkhan/JaeSeon Cho designs?

"Why do they not replicate our (degassed) experimental conditions? Why did the Naranjo model not include the cooling packs despite Rusi having reminded them that the packs were present and stressed that the effect of the packs was important?"

"The timing issue is a red herring," Richard T. Lahey wrote, "since, in our sonofusion work, we do NOT have a single bubble collapsing, as Putterman is accustomed to in his sonoluminescence experiments. Rather, we have a cluster of bubbles collapsing where the interior bubbles can emit D-D fusion neutrons at different times. We have explained all this in a detailed *Physics of Fluids* [1] publication which apparently none of our critics has read or understood."

Taleyarkhan also calculates that the time of flight for neutrons in this experimental configuration (performed when he was at Oak Ridge National Laboratory) casts further doubt on the meaningfulness of Putterman's timing issue.

Reference

1. Nigmatulin, R.I., Akhatov, I. Sh., Topolnikov, A.S., Bolotnova, R.Kh., Vakhitova, N.K., "Theory of Supercompression of Vapor Bubbles and Nanoscale Thermonuclear Fusion," Physics of Fluids, Vol. 17, p. 107106, (2005)

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11. UCLA Failure to Replicate

A group led by Seth Putterman, a physicist at UCLA, has failed in its attempt, [1] funded by the Defense Advanced Research Projects Agency, to replicate the sonofusion research of the group led by Rusi Taleyarkhan, professor of nuclear engineering at Purdue University. Academic and government investigators are beginning to ask why the attempt failed and how the money was used.

Taleyarkhan states he was advised by DARPA's director, Anthony Tether, that funds were approved for DARPA/Office of Naval Research award N00014-05-1-0459 in 2005 for UCLA (as the prime contractor) to collaborate with Purdue on an attempt to reproduce external neutron-induced D-D fusion in deuterated acetone, as published in *Science* [2] and *Physical Review E* [3].

In an e-mail to a sonofusion researcher on Nov. 12, 2008, William Coblenz, the DARPA program manager for the project, explained the termination of the DARPA program.

"The DARPA Sonofusion [program] had as its goal the independent verification of the claims made of acoustic cavitation induced inertial confinement fusion," Coblenz wrote. "The program ended without verification of the fusion claims, and I would not expect the agency to fund similar work in the future."
In the final report for the project, [4] Putterman writes to the government, "This project was to attempt to achieve a 'carbon copy' of the apparatus that [Taleyarkhan] used to generate data published in *Science* and *Physical Review E*."

In their paper [1], the UCLA researchers, along with Ken Suslick, a sonochemist with the University of Illinois, reported that they failed to replicate the Taleyarkhan group's results.

Failure to Replicate Results or Failure to Replicate Experiment?
In the final report for the project, Suslick writes to the government, "An exact duplicate of Taleyarkhan's reactor was built."

However, that is impossible because, as the Taleyarkhan group has told *New Energy Times*, even the group itself did not know how to make an exact duplicate; it was hit and miss. The fabrication of the test sections was, and still is, an art.

Only after the test sections were used and the Taleyarkhan group could see the spherical bubbles (not cometlike), could see their rapid formation (many per second, not one every few seconds) and could hear the loud clicks did the group believe that the test sections made by the Oak Ridge glassblowers had a good chance of working.

Putterman writes in the final report, "At a kick-off meeting at Purdue in May 2005, Taleyarkhan presented us with blueprints for his acoustic chamber. ... Based upon these blueprints, UCLA organized for 'identical' parts to be produced by various contractors."

Taleyarkhan provided no such thing. He had only a hand-drawn sketch with some specifications, nothing at all like any mechanical drawings. Lahey, on the other hand, had later developed mechanical drawings for the test section, and he offered them to Putterman, but Putterman did not respond to Lahey's offer. New Energy Times reported this on July 10, 2007, in our Special Report on Bubble Fusion/Sonofusion. (Putterman failed then and more recently to provide comment to *New Energy Times* on the design matter.)

The Taleyarkhan group explained that there were at least two explicit, significant variations from the stated configuration and protocol in the UCLA group's attempt.

The UCLA paper [1] states that the "acetone was degassed." However, it also states, "Adding small amounts of air increases the SL yield."

The Taleyarkhan group says that such an addition would absolutely cause the experiment to fail.

The UCLA paper also states, "In most runs, however, the top plunger was near to but not in contact with the free surface."

Taleyarkhan states that, if the top plunger is not in contact with the surface of the liquid, the experiment will fail. He states that these details were presented to UCLA during the May 2005 workshop.

Putterman and Naranjo declined to respond to a *New Energy Times* question about these variations in the protocol they used.

Also, for inexplicable reasons, the UCLA group did not appear to search for tritium. Neither its published paper [1] nor the final report [4] to the government mentions an attempt to measure increase in tritium. Considering that this signature is equally convincing of fusion, if not more convincing than neutron signals, the lack of reference to an attempt to measure tritium increase is concerning.

Where Did the Money Go?

Taleyarkhan expressed outrage to *New Energy Times* that some DARPA funds were used not to attempt the replication but to discredit his group's work.

In fact, a UCLA paper published in *Physical Review Letters* [5] states that DARPA funds were used for computer-modeling simulations of the Taleyarkhan group's *Physical Review Letters* [6] experiment and speculates about how the Taleyarkhan group's work is wrong.

But Taleyarkhan wrote that he had a conversation with Coblenz about this use of DARPA's funds, and according to Taleyarkhan, Coblenz did not approve the UCLA modeling 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 PRL studies which were done not with external neutrons but with dissolved alpha emitters. He denied asking for/approving this."

On May 23, 2007, New Energy Times sent Taleyarkhan's quote to Coblenz and Jan R. Walker, a DARPA representative, with a request for comment. Walker responded but declined to comment "due to the ongoing investigation/inquiry."

What's worse is that, according to Taleyarkhan, the UCLA modeling, as published in *Physical Review Letters* [5], doesn't even accurately represent the work reported by Taleyarkhan's group [6].

Specifically, Taleyarkhan states that the UCLA [5] work failed to take into account the use of ice packs. Putterman should have known better, Taleyarkhan says, because Putterman was present at a March 1, 2006, DARPA review meeting at Purdue when Taleyarkhan told him of the importance of their use.

Despite the fact that Taleyarkhan advised Putterman about the requirement to use ice packs, Putterman's colleague, Brian Naranjo, uploaded a preprint to the Internet a week later with no reference to ice packs.
Months later, on Oct. 6, 2006, the Naranjo paper published - still without reference to ice packs.

Putterman and Naranjo failed to respond to *New Energy Times* about the question of why the ice-pack reference was omitted.

Taleyarkhan states that the UCLA modeling published in *Physical Review Letters* [5] has been discredited by two Taleyarkhan group papers, one in *Physical Review Letters* [7] and the other in *Nuclear Engineering and Design* [8]. Naranjo attempted to respond to the Taleyarkhan group's *Physical Review Letters* paper, but Naranjo's manuscript was rejected by the journal.

New Energy Times asked Putterman and Naranjo whether UCLA had made any other responses to the two refutations by the Taleyarkhan group [7] [8]. Putterman and Naranjo failed to respond.
There are other strange problems. The DARPA-approved work was an attempt to address the Taleyarkhan group's work reported in *Science* [2] and *Physical Review E* [3], not the work published in *Physical Review Letters* [6].

Finally, in Putterman's government report, he writes that he used some of the money from the award to build "a world class neutron detector," which does not appear to be part of the requirements for a replication of the Taleyarkhan group's work.

New Energy Times requested a clarification on the use of these DARPA funds from Putterman and Naranjo. They have not responded.

References:
1. Camara, C.G., Hopkins, S.D., Suslick, K.S. and Putterman, S.J., "Upper Bound for Neutron Emission From Sonoluminescing Bubbles in Deuterated Acetone," *Physical Review Letters*, Vol. 98, p. 064301, (Feb. 9, 2007)
2. Taleyarkhan, R.P., West, C.D., Cho, J.S., Lahey Jr., R.T., Nigmatulin, R.I., Block, R.C., "Evidence for Nuclear Emissions During Acoustic Cavitation," *Science* Vol. 295, p. 1868 (March 8, 2002)
3. Taleyarkhan, R.P., Cho, J.S., West, C.D., Lahey, Jr., R.T., Nigmatulin, R.I., Block, R.C., "Additional Evidence of Nuclear Emissions During Acoustic Cavitation," *Physical Review E*, Vol. 69, p. 36109-1, (2004)
4. Joint project of Seth Putterman PI, Rusi Taleyarkhan and Ken Suslick to Reproduce Nuclear Fusion in Collapsing Bubbles Surrounded by D-Acetone
5. Naranjo, B., "Comment on 'Nuclear Emissions During Self-Nucleated Acoustic Cavitation,'" *Physical Review Letters*, Vol. 97, p. 149403 9Oct. 6, 2006)

6. Taleyarkhan, R.P., West, C.D., Lahey, Jr. R.T., Nigmatulin, R.I., Block, R.C. and Xu, Y., "Nuclear Emissions During Self-Nucleated Acoustic Cavitation," *Physical Review Letters*, Vol. 96, p. 179903(E), (Jan. 2006) Erratum
7. Taleyarkhan, R.P., Block, R.C., Lahey, Jr., R.T., Nigmatulin, R.I., and Xu, Y., Reply to [Naranjo] 'Comment on 'Nuclear Emissions During Self-Nucleated Acoustic Cavitation," *Physical Review Letters*, Vol. 97, p. 149404 (Oct. 6, 2006)
8. Taleyarkhan, R.P., Lapinskas, J., Xu, Y., Cho, J.S., Block, R.C., Lahey Jr., R.T. and Nigmatulin, R.I., "Modeling, Analysis and Prediction of Neutron Emission Spectra From Acoustic Cavitation Bubble Fusion Experiments," *Nuclear Engineering and Design*, Vol. 238, No. 10, p. 2779-279, Aug. 2008

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12. Taleyarkhan Responds to Suslick's Accusations of Fraud

On July 28, 2008, *New Energy Times* published *Welcome to Bubblegate* to our blog site.

On July 31, Professor Ken Suslick, a sonochemist with the University of Illinois, posted this response:

> There has indeed been malfeasance by the upper Purdue Administration, at the level of Dean and above. Krivit says Taleyarkhan has been “exonerated” of scientific fraud. “Exonerated” is NOT the same as “not investigated.” Taleyarkhan has not yet been formally investigated by Purdue for true scientific fraud: i.e., falsification of data, selective (”cherry-picking”) of data, contamination of apparatus with tritium and with Cf (a neutron calibration source known to be in his lab), intentionally misleading misuse of statistics. The decision by the Purdue Administration not to formally investigate is extremely troublesome. It has analogy to convicting Al Capone of tax evasion rather than murder.

Suslick had submitted numerous allegations against Taleyarkhan to Purdue. Very similar anonymous allegations were considered by Congress and the Office of Naval Research.

On July 31, a person with the name of "Randy Starline" posted a response to Suslick:

> To Ken Suslick:
>
> Each and every one of your arguments and “scientific” allegations can be addressed from the very first paper of Rusi Taleyarkhan and his team’s work published in the journal *Science*.

Your californium concerns are completely bogus. You don't need to be a fusion guru to be able to completely disprove your argument that californium is present.

And it is based on the very fact that the spectra of fusion sources and californium are radically different, notably in the presence of gammas (or lack thereof for fusion). Note that Taleyarkhan's results showed no gammas; therefore, no californium is present.

Also note that the Taleyarkhan team's experimental setup included ice-pack shielding. It doesn't take a scientific genius to expect that this would bring energies of the detected neutrons *down*, thereby crying out for the need for *proper* bookkeeping when it comes to accounting for this crucial aspect of the setup.

That said, it is very easy for you folks who were "attempting" to replicate his work to simply juggle those books by ignoring those darn ice packs, thereby likewise deliberately falsifying the data yourselves.

Now, according to every newspaper and report in which you, Seth Putterman (UCLA), and your other anti-Taleyarkhan cronies in the scientific community who have been quoted liberally slandering Taleyarkhan and his work, *that* is called scientific fraud and constitutes a crime.

If you take your and your fellow "replicators'" results and properly account for the various aspects of the setup (and if you were even using the correct setup yourselves), you will see that your data fall *right on top* of Dr. Taleyarkhan's results.

New Energy Times asked Taleyarkhan to review and respond to the comments from Suslick and "Randy:"

Randy has correctly pointed out that Cf produces a lot more gammas than neutrons. D-D thermonuclear fusion produces NO gammas. Suslick/UCLA's cherry-picking of just neutron spectrum shape is something Randy points out accurately, as well as their failure to account for gammas which, if it were Cf, would be the dominant signal. We measured virtually no gammas - only a small amount - which come out when a fraction (5-10 percent) neutrons interact with structural atoms of H, Cl,.

Randy also correctly points to the fact that, if UCLA (Naranjo) had included the ice packs in his data for the spectrum shape for neutrons, his data would then come on top of what we measured, as we showed in our new NED paper last month.

What is not said in all of this is that UCLA/UIUC were told on March 3, 2006, to include the ice-pack shielding, but despite the admonishment, they retained their

predictions all through 2006. To date, they did not tell Eugenie Samuel Reich (Nature), either, who took their February 2006 Web posting (non-peer-reviewed paper) as the gospel truth and started this saga of slander. Randy correctly cites them as having engaged in what they accuse the Taleyarkhan team of having committed.

Also, after three years of investigations, all allegations of fraud (especially the well-publicized ones related to Cf) have been rejected by the Feds. Randy is supported by this, too.

Despite the fact that the 2007 Purdue C-22 Inquiry Committee dismissed the allegations of californium spiking, the 2008 Purdue C-22 Investigation Committee brought them back for reconsideration in the Purdue C-22 Investigation Committee Final Report. Taleyarkhan provided an authoritative response to the californium spiking allegations in his appeal/rebuttal to the Purdue report.

The Inquiry Committee asked Brian Naranjo of UCLA whether his computer simulation would be more authoritative than an eyewitness who attested that there was no californium spiking. The report said that Naranjo said that his simulation did not have that power and that the committee had no record of an eyewitness to such data fabrication.

On May 11, 2007, Suslick had told the West Lafayette, Indiana, *Journal & Courier*, *""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.""*

The Journal & Courier wrote, "Suslick is giving Purdue documentation that he says proves Taleyarkhan was involved in research misconduct."

Suslick was an eyewitness to - and participant - in the experiments performed on March 1, 2006, during a Defense Advanced Research Projects Agency review meeting. JaeSeon Cho, formerly with Oak Ridge National Laboratory, wrote the following in a signed and sworn affidavit on Jan. 26, 2008.

Neither I nor anyone from Purdue, to my knowledge (and I was present for virtually the entire time), prevented any of the visitors from getting answers to their questions on sonofusion. In fact, on March 1, 2006, Ken Suslick was invited to choose the CR-39 detectors to use for monitoring neutron emission during the self-nucleated experiments. At the end of the experiments, positive results for neutron emission for the self-nucleated experiments were obtained, as read by several people.

Cho's statements about positive results are also supported by the signed affidavit of Ross Tessien of Impulse Devices, on March 2, 2006.

13. Tsoukalas* and Franklyn Clikeman Change Tritium Confirmation to Disconfirmation**

* At the time, the head of Purdue University School of Nuclear Engineering
** Emeritus professor, Purdue University School of Nuclear Engineering

BEFORE

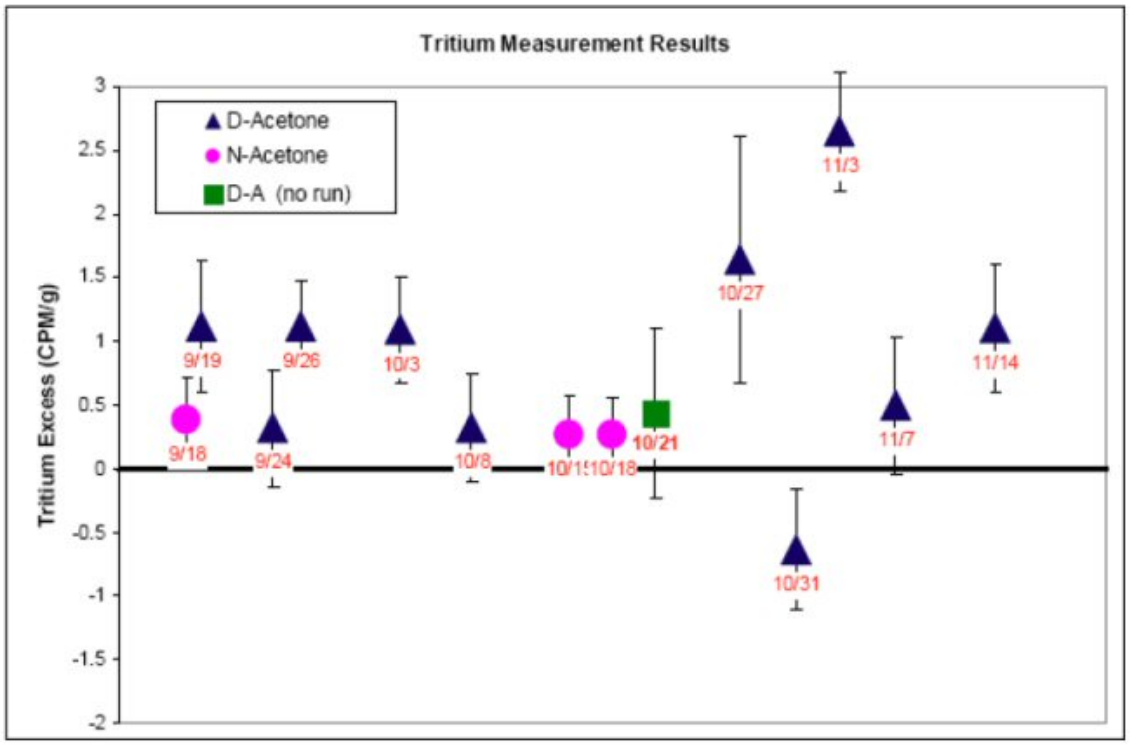

Figure 5: Measurement of excess tritium per gram of acetone in sample /CPM per gram/

DRAFT: ~Jan. 2004
"Tritium Evidence in Acoustic Cavitation Nuclear Emission Experiments"

Abstract
"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 [1] - provide positive evidence for excess tritium produced (and attributed to D-D nuclear fusion.)...The results point to statistically observable tritium increases in post-cavitation deuterated acetone samples..."

Conclusion
"Tritium measurements in deuterated acetone subjected to acoustic processing as reported by Taleyarkhan et al [6] point to positive evidence for increased tritium production. Several runs were made under nearly identical conditions to establish repeatability. Most (but not all) involving deuterated acetone point to a net growth in tritium counts."

AFTER

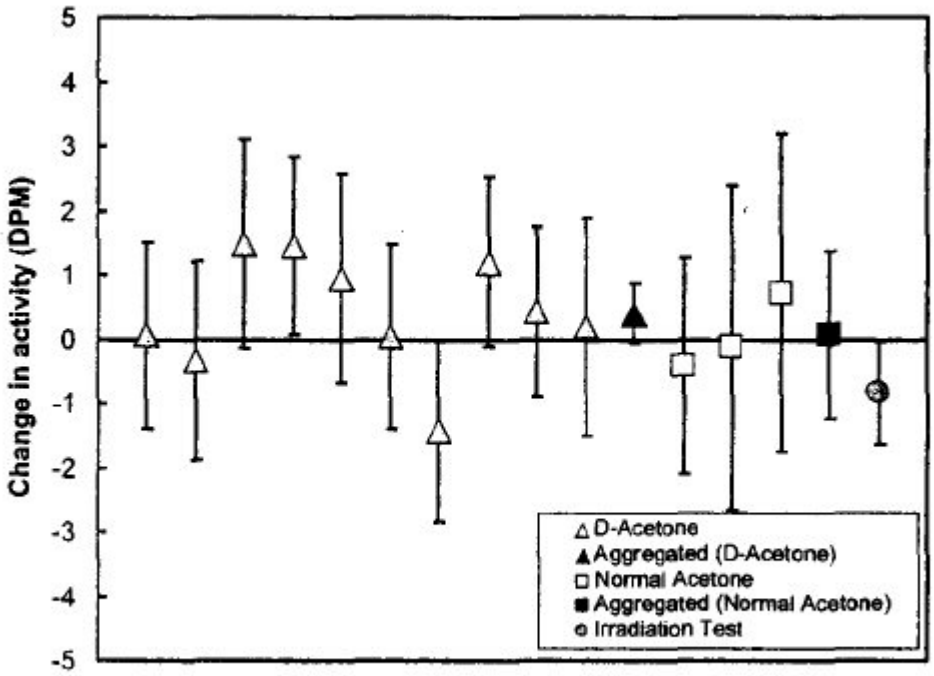

Fig. 6. Tritium measurements.

SUBMITTED: Feb. 2006, PUBLISHED: Aug. 2006
"Tritium Measurements in Neutron-Induced Cavitation of Deuterated Acetone"

Abstract
"An attempt to reproduce the tritium measurements in an acoustic cavitation experiment with deuterated acetone has shown no evidence of tritium production attributed to D-D fusion. The average number of disintegrations per minute observed is within 1 sigma of zero.

Conclusion
"Evidence of tritium production attributed to D-D fusion sought in this study. The average dmp count observed is within 1 sigma of zero. Therefore, the claim of Taleyarkhan et al.[1] was not confirmed in the experimental study presented.

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14. The Independence of the Xu/Butt Replication

Allegation A.1
Source: April 18, 2008, Purdue C-22 Investigation Committee Report

Dr. Taleyarkhan with falsifying intent caused his name not to be included in the author bylines of the NED and NURETH-11 papers to disguise the extent of his involvement in the execution of the work reported and in the writing and submission of the papers.

Conclusion A.1
Source: April 18, 2008, Purdue C-22 Investigation Committee Report

Even though a large volume of evidence clearly shows that Dr. Taleyarkhan was heavily involved in every aspect of the research and publication of the manuscripts, the Committee concludes that there are a number of reasons why a senior mentor's name may not appear as an author of a publication. Thus we cannot unequivocally deem the omission of his name as an intentional act of falsification of the author byline.

Conclusion A.1 Rebuttal
Source: July 28, 2008, Taleyarkhan C-22 Appeal/Rebuttal Report
(Taleyarkhan states that Purdue's conclusion was misleading. His additions are in blue and deletions are in red.)

Even though a large volume of evidence clearly shows that Dr. Taleyarkhan **and his colleagues (Drs. West, Lahey, and Cho) were was heavily** involved in **every some** aspect**s** of the research and publication of the manuscripts, the Committee concludes that there are a number of reasons why a senior mentor's name may not appear as an author of a publication. Thus we cannot unequivocally deem the omission of his name as an intentional act of falsification of the author byline.

Source: July 28, 2008, Taleyarkhan C-22 Appeal/Rebuttal Report

Additionally the following are pointed out:

• Two duly constituted Committees (the 12/15/2006 C-22 Inquiry Committee Report; and ONR-mandated Aug.27, 2007 Inquiry Committee Report) have unanimously concluded that there was no research misconduct for this category of allegation.

• Referring to affidavits of Dr. Xu (1.31.08, para. 16), and Revankar (1.31.08, para.9) it is sworn by these two co-authors of their NED/NURETH-11 manuscripts that there was no involvement in any of the crucial aspects related to deriving observations of successful sonofusion results from an experimental confirmation study (as tabulated in Table 1).

Table 2 identifies the metrics developed by an earlier duly formulated 2006 C-22 Inq.C to show that there was no involvement nor participation in key aspects related to experiment design, setup, experiment conduct, data acquisition, detector usage and calibrations, data analyses and the drawing of conclusions.

There indeed was involvement and consulting assistance provided by the original discovery team members (Taleyarkhan, Cho, West, Lahey, Nigmatulin and Block) for test cell setup, and training on operation, as well as for review feedback, composition of manuscript and for reaching out to publishing bodies.

• Neither Taleyarkhan nor any other member of the original team was involved in the capacity of being a referee, nor as an editor with the authority to accept or reject the NED and NURETH-11 papers. All members of the original discovery team have provided sworn affidavits to this effect.

Therefore, while it is useful to point to areas where Taleyarkhan and colleagues were involved with the NED/NURETH-11 papers, it is equally important to point out the substantial and crucial areas where they were not. The NED work was a confirmation study, not an undertaking for reaching a novel discovery.

Source: July 28, 2008, Taleyarkhan C-22 Appeal/Rebuttal Report
[Bracketed additions are clarifications by New Energy Times]

Table 2. Judging the Independence of Dr. Xu et al. Confirmatory Studies - Per Metrics from the Dec. 15 2006 C-22 Inquiry Committee Report

Criterion / Question	Response	Clarification
Location: Was location the same as the ORNL experiments?	No	Dr. Xu et al. experiments were performed at Purdue vs ORNL.
Sponsorship: Did Dr. Xu et al. receive funding from Taleyarkhan et al. (2002)?	No	Dr. Xu was funded by Purdue's School of Nuclear Engineering.

Detector: Was the tritium detector/spectrometer the same as that used by Taleyarkhan et al. (2002)	No	Dr. Xu et al. used a different tritium detection spectrometer.
Calibration of Detector: Was the Dr. Xu et al. tritium spectrometer calibrated by Taleyarkhan et al. (2002)?	No	The calibration was performed by [one of the world's foremost experts DoE/ORNL employee] Dr. M. Murray who traveled to Purdue during 12/2003 to deliver and set up the LS6500 Beckman spectrometer.
Measurement protocol: Was the protocol for tritium detection the same as that used by Taleyarkhan et al. (2002)?	No	The protocol and approach for liquid scintillation counting for tritium was different. Dr. Xu et al. detected tritium production using an organic liquid -based Ultima-Gold scintillation cocktail versus the use of an aqueous-based Ecolite cocktail used by Taleyarkhan et al. (2002, 2004)
Test apparatus (reactor cells): Were the test cells used by Dr. Xu et al. the same as the ones used by Taleyarkhan et al. (2002)?	No	Dr. Xu et al. used different test reactor cells that were different and distinct than the ones used to conduct the experiments in Oak Ridge, TN by Taleyarkhan et al. (2002)
Experimental approach: Did Dr. Xu et al. use an identical experimental approach for nucleation and sustaining the sonofusion experiments as reported by Taleyarkhan et al.(2002)?	No	The experimental approach for timing, starting and maintaining the sonofusion process was radically different. Dr. Xu et al. used a fundamentally different experiment approach; whereas, Taleyarkhan et al. used an accelerator based 14 MeV neutron microsecond-pulsed neutron source, Dr. Xu et al. at Purdue used a continuous spectrum randomly emitting neutron sources, both Cf-252 and Pu-Be
Facility, geometry and functionality: Was the thermal-hydraulic design of the experiment station identical to that of Taleyarkhan et al. (2002)?	No	Dr. Xu et al. used an experimental geometry which was radically different (i.e., they used an enclosed passive freezer containment versus the use of an active chilled-air driven cooling system used in the ORNL experiments).
Participation in experiments: Did anyone from Taleyarkhan et al.(2002) participate in the conduct, data acquisition and analyses of the Dr. Xu et al.	No	Dr. Xu formally testifies that he conducted the experiments, acquired the relevant data and analyzed the data independently and drew conclusions with no participation by the Taleyarkhan et al. (2002,2004) team

experiments?		members.
[Experimental Assistance:] Did Dr. Xu/Mr. Butt receive assistance/tutoring from Taleyarkhan et al.(2002,2004) team members on test cell design, setup and operations?	Yes	This assistance has been acknowledged by Dr. Xu et al. directly in the Acknowledgment sections of their 2005 NED and NURETH-11 papers.
[Editorial Assistance:] Did Dr. Xu/Mr. Butt receive review comments on their manuscript, technical writing/composition assistance and assistance for reaching out to Science, PRL and Elsevier Publishing House for consideration for acceptance and publication in NED.	Yes	Dr. Xu testifies that he requested review feedback and assistance related to paper preparation for publication from Dr. Taleyarkhan and several of his colleagues.
[Peer Review:] Did Dr. Taleyarkhan et al. (2002,2004) manage the editing, independent reviews and decision to accept or reject the Dr. Xu et al. NED manuscript?	No	This function was performed by Dr. G. Lohnert, Principal Editor of the Elsevier Journal of Nuclear Engineering and Design. Dr. Lohnert has provided sworn testimony to this effect to state that the decision to accept was his and his alone and based on the merits of the paper and in no way was he influenced by Taleyarkhan et al. (2002,2004).

New Energy Times notes the following:

1. Neither Taleyarkhan nor his co-authors made the test section (cell) used in the Xu/Butt replication. The test section was made by one of three glassblowers at Oak Ridge National Laboratory who had developed an understanding of and had the required skills to fabricate such test sections.

2. Taleyarkhan occasionally visited the School of Nuclear Engineering's loaned space at the pharmacy laboratory when Yiban Xu was performing the replication experiments. However, Taleyarkhan states he was there on other business with other students. The lab was not "Xu's lab." It was under the general direction of Lefteri Tsoukalas. Xu worked at one of several workstations within the so-called pharmacy lab. Taleyarkhan states that, during the five-month period when Xu was running the reported experiments (running a complete experiment from start to finish generally takes 8-10 hours), entry-exit logs required by the Nuclear Regulatory Commission show that Taleyarkhan was in the lab no more than a few minutes at a time for a total of no more than about two hours over the entire five-month period.

New Energy Times Independence Assessment of Xu/Butt Replication: 5 points
(See Criteria for Evaluating the Independence of Scientific Replications)

At most, the Taleyarkhan group is guilty of expressing its opinion - which may or may not be shared by others - that its work was independently replicated.

15. Purdue Explanation of Fabricated Allegations Looks Like Cover-Up

WEST LAFAYETTE, IN -- After fabricating allegations of research misconduct against Purdue professor Rusi Taleyarkhan, Purdue administrators attempted to defend their actions, *New Energy Times* recently learned.

On Oct. 14, 2008, *New Energy Times* reported that the Purdue C-22 Investigation Committee, with the help of Purdue administrator Peter E. Dunn and Purdue counsel William Kealey, fabricated the allegations

New Energy Times recently learned that, on July 28, 2008, Taleyarkhan wrote in his Appeal/Rebuttal report to Purdue that he realized the allegations were fabricated. On Aug. 21, 2007, the Purdue Appeals Committee offered its explanation for its inclusion of the new allegations.

The Appeals Committee wrote in its report on Aug. 21, 2007:

The 2007 Inquiry Committee forwarded a total of twelve allegations to the Investigation Committee, which the Investigation Committee then "aggregated and restated" for the sake of clarity. It is our opinion that the broad issues in allegations A.2 and B.2 were identified by the 2007 Inquiry Committee and should not be considered to be "new" charges. ... In our opinion, C-22 does not prohibit the Investigation Committee from considering charges that are derived from the original charges if they choose to do so.

The Inquiry Committee forwarded 12 allegations to the Investigation Committee for consideration. As defined by the C-22 policy, these allegations had to be submitted to the dean in writing and signed. The Investigation Committee considered those 12 allegations and reviewed - and dismissed - all 12. But then somebody (as yet unidentified) involved with the Investigation Committee added two other allegations.

Purdue's C-22 policy governing research integrity does not prohibit the Investigation Committee from adding its own charges, though this would be in stark contrast to the spirit of the American system of justice, which requires no overlap among accuser, judge, jury and executioner. Further, it is contrary to the practice Purdue used with the other allegations; they all had named sources.

Here is why the Purdue explanation begins to look like a cover-up. The Investigation Committee went to great lengths, as shown in its Final Report, to show how the allegations were cross-referenced between the Inquiry Committee and the Investigation Committee. As I reported on Oct. 14, 2008, and in a video on Oct. 20, 2008, the cross-reference fails.

Second, the Investigation Committee explains its procedure for evaluating the allegations in the Executive Summary section of the Final Report. The first sentence says that the "Inquiry Committee forwarded twelve allegations."

Third, the Executive Summary does not mention that the Investigation Committee brought in new "charges that are derived from the original charges."

Fourth, the Investigation Committee wrote that it "aggregated and restated" the allegations "for the sake of clarity." If the committee intended to provide clarity, it failed.

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16. Bubblegate Affidavits Page

Before Lefteri Tsoukalas was removed as the head of the Purdue School of Nuclear Engineering, an illegal committee that he organized produced the Feb. 23, 2006, Statement from Adam Butt. Someone provided this document to Kenneth Chang of *The New York Times*. Chang has declined to confirm or deny whether Tsoukalas provided the document. Chang has confirmed that he received other related documents from Tsoukalas. Chang made the decision to publish the unsigned, unnotarized, unsworn, unverified document.

The Statement from Adam Butt caused severe problems for Rusi Taleyarkhan, a professor in the School of Nuclear Engineering. In response, numerous people came to Taleyarkhan's defense with testimonials and affidavits. A number of the affidavits accuse Tsoukalas of serious grievances. Some of them are now part of the public record in legal proceedings.

Go to Affidavits Page

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17. Bubblegate Timeline and Library Index

The main purpose of this index is to provide a comprehensive archive of all documents related to Bubblegate. This index includes some minor overlap with the Bubblegate News and Press Releases Index because there are key news items and press releases that have played such a significant role in the events that they are included here as well. This index also serves as a chronology: events with no related documents are also listed.
Go To BubblegateTimeline and Library Index