

December 7, 2006

Peter E. Dunn, PhD  
Associate Vice-President for Research  
Purdue University  
W. Lafayette, IN

Dear Dr. Dunn:

**Re: Response to your letter dated November 30, 2006**

I have reviewed your letter and package of materials transmitted via Mr. Selander dated November 30, 2006. The short and simple answer to your question is: the documents show absolutely no misconduct of any kind, including the question of research misconduct in “reporting.”

A more detailed explanation is set forth below:

**A. Transmission of information exchange on the 2005 *Nuclear Engineering and Design (NED)* paper by Xu et al.**

Your memorandum of October 23, 2006 requested information exchanges between me and Dr. Xu for the 2005 *NED* paper by Xu and Butt through January 31, 2005. This was done as requested and a response was transmitted to you dated October 31, 2006. The earlier unsuccessful attempts by Xu to publish in higher-tier media (*Science* and *PRL*) were considered moot. Note: *Science* and *PRL* submissions both have extreme length restrictions and different formatting requirements, etc. which ends up usually as a condensed single page report unless it is new work in which case it is allotted up to ~ 4 pages. The manuscript published in *NED* was different and far more extensive in size (8 journal pages).

In short, I believed I was supposed to respond directly relative to the *NED* paper and that is what I did. I did not include materials related to Dr. Xu’s efforts to publish in *Science* or *PRL* because they were not requested, nor do I believe they are relevant to the specific allegations of research misconduct against me.



## A. Response to memorandum of November 30, 2006

Your memorandum of 11/30/2006 asks for a response to actions on my part related to advice and guidance provided to Xu for his efforts to publicize his confirmatory work in media other than NED.

In that regard the following clarification is provided to the C-22 review committee:

1. The central point that confirmatory research results for reported results of nuclear fusion were experimentally obtained, analyzed and conclusions drawn independently by Xu has already been positively affirmed by Dr. Xu to you separately in his unequivocal signed statement to you dated October 27, 2006 (and earlier announced via Purdue's own Press Release and then to the worldwide media).
2. The other central point related to publicly acknowledging that Xu indeed received requested advice, guidance and assistance from several individuals for the overall work reported in his *NED* paper has also been published and acknowledged by him several times (in his published *NED* manuscript and in several news articles) and signed upon in his memorandum to the review committee. The worldwide research community (composed of editors of journals and anonymous referees) was appraised of this aspect in the manuscript submitted by Xu for possible acceptance of the research work.
3. The fact that I offered assistance in communicating with the editor-in-chief (Prof. G. Lohnert) of *NED* journal to advise him of the availability of a potential manuscript for his journal has also been transmitted to you earlier. The decision to invite and review the *NED* manuscript for acceptance was made independently of me and this has also been publicly accepted and announced by G. Lohnert. The extent of my involvement in the 2005 *NED* manuscript has been discussed in Dr. Xu's memorandum to you of October 27, 2006.
4. The primary allegation of misconduct made by Dr. L. Tsoukalas to the C-22 Review committee rested on the central point of the **only claim** related to the 2005 *NED* paper made by me and my co-authors of my January, 2006 *PRL* journal paper **"these observations have been independently confirmed."** Nothing more, nothing less. See (last 2 lines of left column) of the reproduced page on the next page. As you will notice, there were no claims made to any other aspect of the work reported by Xu et al. The facts that Xu et al. received technical guidance, apparatus, and advice were already acknowledged by them. However, they performed their experiments independently, analyzed their data independently and drew the resulting conclusions independently. Therefore, the claim of the 1/06 *PRL* paper is justified as already accepted by

the American Physical Society (APS) with their decision to publish based on advice they received from anonymous referees.

### Nuclear Emissions During Self-Nucleated Acoustic Cavitation

R. P. Taleyarkhan,<sup>1,\*</sup> C. D. West,<sup>2,†</sup> R. T. Lahey, Jr.,<sup>3</sup> R. I. Nigmatulin,<sup>4</sup> R. C. Block,<sup>3,†</sup> and Y. Xu<sup>1</sup>

<sup>1</sup>Purdue University, West Lafayette, Indiana 47907, USA

<sup>2</sup>Oak Ridge National Laboratory, Oak Ridge, Tennessee 37830, USA

<sup>3</sup>Rensselaer Polytechnic Institute, Troy, New York 12180, USA

<sup>4</sup>Russian Academy of Sciences, 6 Karl Marx Street, Ufa 450000, Russia

(Received 19 September 2005; published 27 January 2006)

A unique, new stand-alone acoustic inertial confinement nuclear fusion test device was successfully tested. Experiments using four different liquid types were conducted in which bubbles were self-nucleated without the use of external neutrons. Four independent detection systems were used (i.e., a neutron track plastic detector to provide unambiguous visible records for fast neutrons, a  $\text{BF}_3$  detector, a NE-113-type liquid scintillation detector, and a NaI  $\gamma$  ray detector). Statistically significant nuclear emissions were observed for deuterated benzene and acetone mixtures but not for heavy water. The measured neutron energy was  $\leq 2.45$  MeV, which is indicative of deuterium-deuterium (D-D) fusion. Neutron emission rates were in the range  $\sim 5 \times 10^3$  n/s to  $\sim 10^4$  n/s and followed the inverse law dependence with distance. Control experiments did not result in statistically significant neutron or  $\gamma$  ray emissions.

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PACS numbers: 78.60.Mq, 25.45.-z, 28.20.-v, 28.52.-s

*Introduction.*—Previously, we have provided evidence [1(a),2–4] for 2.45 MeV neutron emission and tritium production during external neutron-seeded cavitation experiments with chilled deuterated acetone, and these observations have now been independently confirmed [5].

[1(a),2,9]. Four independent nuclear particle detection systems were utilized in the new study. This included use of a long-established passive-type track-edge fast neutron detector (i.e., CR-39<sup>TM</sup> plastic detector from Landauer, Inc.) that is insensitive to  $\gamma$  rays and that is well-known [9–11]

Now, let me directly address actions taken and my position related to your enclosed emails during and around December, 2006 leading up to Dr. Xu's submissions to and interactions with referees of PRL journal.

5. Upon successfully obtaining confirmatory data, as has already been acknowledged by me, my advice was sought by a freshly-minted inexperienced Ph.D student (Xu) on the best avenues and methods for dissemination of the same. As is well-known and quite obvious, one normally strives for the highest-impact journal or venue for their work. If not successful there for any number of reasons, one then submits for consideration elsewhere as appropriate. Due to this, the obvious first choice was the journal Science, which is one of the world's two pre-eminent scientific journals where my original discovery was published and during the course of which I developed a rapport with the editor-in-chief Dr. Donald Kennedy (also past President of Stanford University).
6. At the time (mid 2004) when Xu had completed his confirmatory work and wished to consider a journal such as Science, I found he had no experience whatsoever, in journal-based publication and definitely no experience communicating with, nor publishing in science-based journals. Assistance was sought and I agreed to help with the mechanics upon conferring with my colleagues.

7. Upon request for guidance during casual conversation with Xu, I conferred with my two original team members whom I respect for their experience and maturity (especially Professor Richard T. Lahey, Jr. – ex Dean of Engineering at Rensselaer Polytechnic Institute (RPI), a senior executive-cum-scientist at General Electric Co., and member of the National Academy of Engineering, as also physicist Dr. Colin West with over 50y research experience prior to his recent retirement from Oak Ridge National Laboratory (ORNL) as Director of Nuclear Technology Research, and Director for the nation’s largest science project the USDOE’s \$2.5B Advanced Neutron Source Reactor Project at ORNL, also a past senior physicist-scientist at Harwell, United Kingdom. You have email correspondence between me and these colleagues from RPI and ORNL to this effect. Upon discussion, it was deemed appropriate that I (due to my existing links with the journal Science) would drop a note to Dr. Donald Kennedy informing him of this development, which I did (per my email of October 11, 2004). Barring Science, the next in line could be the *PRL* journal. With the introduction and advice on submission protocol, Dr. Xu submitted his manuscript summary officially to Science for consideration.
8. Science journal staff advised Dr. Xu that his confirmatory report would be better suited for a technical journal upon which Xu submitted his manuscript to *PRL* staff with advice from me on submission protocol to American Physical Society (APS) journal editors where I have published before.
9. This point now leads up to and relates directly to Dr. Dunn’s cited (my) emails to Dr. Xu of December, 2004. Upon submission to *PRL*, Xu received referee comments that were in his view inexplicable and astounding to him and sought advice and guidance. To one experienced in the field one knows this can happen with varying frequency (almost luck of the draw) with the *PRL* journal that the chosen referees (under cover of anonymity) can at times prove unreasonable and arrogant. In this instance, the issue was not technical, but philosophical. In particular, one of the referees (Referee A) was demanding a totally different type of experiment (i.e., use of lasers to nucleate bubbles rather than use of neutrons and also to monitor for neutrons with totally different techniques). This was missing the point completely; Xu’s paper was about reporting results of experiments to confirm results with procedures used in already published papers by Taleyarkhan et al. Due to inexperience, Xu sought advice from me on how to respond to such philosophically-bent questions and demands.

Before responding to Xu’s request, for due diligence, I conferred with my colleagues (Dr. Lahey of RPI and Dr. West of ORNL) who both willingly offered input on offering assistance to Dr. Xu for his response. **Would they have gone along with this if the practice deviated from commonly accepted practice? Absolutely no!!** Their email communications with me dated October 22, 2004 to November 30, 2004 are part of the package you have transmitted to me. You will note that, in none of this communication with Drs. Lahey and West who together comprised a combined ~100y research

experience base covering research practices on two continents (in the USA and in Europe) that any concern was voiced or transpired to the effect that such mentoring to a junior colleague could be anything but above-board. **Dr. West's advice-cum-feedback via email to me dated December 12, 2004 were not part of the November 30, 2006 package (from Dr. Dunn) and is attached to this letter.** You will note the first and last statements of this stalwart's email letter to me: (i) **Yes, Reviewer A's comments are so off the wall that it is difficult to see how one could respond.;** and, (ii) **"Please feel free to share my comments with the authors if you think it appropriate."** This is indeed the bulk of what I shared in my guidance offered to Dr. Xu in my December, 2006 emails to him.

**Specifically included in my December 2006 email guidance were Dr. West's opinions and comments (somewhat paraphrased with my own knowledge of the field) relating to Referee A stating: (a) "Of-course .. neutrons are detected indirectly... how else can one detect neutrons?; (b) Time of flight information ...there is no time zero marker accurate enough...; (c) .. tritium can be detected in other ways .. but beta counting (using liquid scintillation spectrometry) is the standard method .. the some other decay idea that the reviewer mentions is completely ruled out by the control experiments..; (d) .. agree that the idea of "seeding" the bubbles with .. other than neutrons is an obvious one – so obvious, in fact, that even thought of if .. but it is not what these researchers are reporting."** Then, Dr. West's comments included for Referee B stating: (a) .. don't think the neutron detector "would be swamped with radiation from the Cf-252 source" and the results show it was not; and, in my own words explained what Dr. West brings up: "The reviewer seems not to know that it is not the fast neutrons themselves that give the scintillation signal, but their knock-ons...." The advice was communicated to Dr. Xu as envisioned by myself, Dr. West and Dr. Lahey. None of this advice and guidance in any way impacted the actual experimental work that Dr. Xu conducted nor the data and results he observed.

### **The December 14<sup>th</sup> Email**

A review of my December 14, 2004 email to Dr. Xu confirms my statement that it has nothing to do with my trying to influence the results or conclusions of Dr. Xu's work; rather, it is a simple attempt to help phrase responses to statements made by referees, which were for the main either mistakes made by the referee in understanding what the experiment was about, or pointing out additional information to the referee which answered the referee's question. Dr. Xu asked me to comment and I did. Before doing so, I talked with other colleagues (as you would normally expect) and got their comments as well. Dr. Xu was certainly free to do as he wished with my comments, but I think Dr. Xu was probably aware of most (if not all) of this information anyway, and what I was really doing was summarizing and trying to help him organize it in English which, as I have stated before, was somewhat of a problem for him.

Please let me review some of the comments made in my December 14<sup>th</sup> email:

## **Referee A**

Referee A's first criticism is, "Neutrons are used to seed cavitation, which the referee objects to suggesting a different mode for seeding."

The response I suggested to Xu simply points out a fact. Xu was doing a confirmatory experiment. The only way to do a confirmatory experiment is to try to do it the same way as the experiment you were trying to confirm. My experiment was seeded with neutrons. Therefore, so was Xu's. My comment to Xu about this referee's criticism was that it didn't make much sense. But, most importantly, it has absolutely *nothing* to do with influencing how Xu did the experiment, how he reported it, etc. The experiment was already done.

Referee A's second criticism was essentially, "Neutrons are detected indirectly (in a scintillator)". The response is simply "of course they are". It's the only way to do it. I know that, Colin West knows that (as you can see from his email to me), and Xu knows that. It is simply a statement of fact.

Similarly, Referee A's third criticism that "time of flight method was not used" is factually true, but totally irrelevant. The reason is you have no accurate-enough zero starting point. This is pretty simple, and straightforward as also pointed out by Dr. West but the referee apparently missed it. Again, it has nothing to do with the validity of Xu's work.

These were all the comments I made regarding Referee A. I conclude by saying "the referee is asking for a different type of experiment to be conducted". This is absolutely true. Xu knew, as does everyone else, that if you do a different experiment it does not confirm the original experiment. Since the whole purpose was to confirm, it makes no sense to change the experiment.

## **Referee B**

My comments with regard to Referee B are pretty much in the same vein. For instance, the first comment/critique is "was the counting done for six hours for each sample...". Again, answering this is simply saying yes it was done that way. Xu knows that, we all know that, and it's simply putting parameters on the experiment which were either originally included and missed by the referee or were included after the referee pointed out that the information was missing. The second critique is in response to Referee B's comment that he was unable to understand part of the paper. My comments were simply an effort to make the explanation understandable. Again, they do not affect Xu's work and conclusions.

Referee B's third and fourth questions/critiques are simply requests for information which Xu had available to him. Again, for sake of completeness, I included in my email to Xu a way to point that out to the referee.

Referee B's fifth comment/critique is that the "LS Detector would be swamped by the emissions from the CF-252 source...". The simple response here is the fact that the

detector was not saturated. This is simply a description of the facts as observed by Xu and also communicated by Dr. West.

Referee B's sixth comment asks a question about neutron emission based on different sources. The response is that there is no answer to his question because the neutron source that the referee was speaking of was not available to the Xu group. They did not have one, so they could not report an answer to his question. Xu knew this. It is simply a fact.

Referee B's seventh comment/critique asks about the significance of a part of a figure. The response is to point to some well established calibration curves which is what was done by Xu. This was simply to fortify something that he had already done.

Comment/critique eight from Referee B is a simple statement that the referee has made an error as to how the data is interpreted. He was simply wrong about something and Xu (who certainly knew this because he did the analysis) is given a way of explaining this in English.

These are all the comments that were made about Referee B's work.

In summary, there was nothing said or suggested that would in any way influence the work or its outcome.

It is requested that this C-22 Review committee note the circumstances in which the advice and guidance was offered by me (together with internationally-known scientists) when sought for addressing the philosophically-bent questions from referees of PRL during their submission.

I ask: "Would I myself help *other* students or close academic colleagues who request assistance for responding to queries in areas that I specialize in?" Absolutely!! In fact, I have freely provided advice and mentorship (as you will note from the extensive email and other communications that have been sent to you on October 13, 2006) to the very individuals (Drs. Tsoukalas and Bertodano and other students) who have initiated this C-22 review. It is in my style and nature to provide feedback in such a way as to make it easiest for the receiving party to accommodate the same if acceptable, for their specific needs. At times it may be more than asked for. But that is an individual's prerogative.

10. It is important to note that in the final analysis, neither of Dr. Xu's submissions to *Science*, nor to *PRL* were accepted for publication. The related advice and guidance of my email transmittals (of December, 2006) to provide responses to the philosophically-bent questions and demands were not in any way needed nor used for his submission to the *NED* journal. For the *NED* journal submission where I specifically was serving as co-editor for the Festschrift Edition I played no role in the review nor the acceptance of the manuscript which was the central focus of the allegations made by Drs. Tsoukalas and Bertodano.

Finally, let me add some information on what practice I have myself encountered during the submission, response, acceptance and publication of my own seminal publications in *Science* (2002) and in *Phys. Rev. E* (2004) while I served as scientist at Oak Ridge National Laboratory (ORNL). It is usual practice in national laboratories (definitely at the time at ORNL) for manuscripts to be extensively edited and/or written with input of the key technical content from authors by so-called technical writing staff. This was indeed done for my two cases while at ORNL. The manuscript was then reviewed by my co-authors and revised further. Thereafter, a mass of tens of referees covering ranks of management and technical staff provided their scientific and other inputs before revision again and then submitted to *Science*. Upon receipt of comments from referees we conferred with experts around the world for their advice and guidance. Thereafter, upon acceptance for publication, the *Science* journal had their own technical editor who modified the article for reporting per their own style. The final manuscript was thus impacted by an estimated ~100 people in terms of reporting style and also in terms of the process of successfully responding to referees. For the 2004 *PRE* paper, most of the same was true with the exception that this time around, I as principal author was specifically “directed” by ORNL management at the President’s level to submit to *PRE* and not to *Science*. Was all of this to be construed as research misconduct on part of ORNL’s management? Hardly!!

One keeps an open mind and works with judgment for individual circumstances taking cues and input from fellow scientists while maintaining open acknowledgment of the key facts of the case. Seeking and using advice from mentors is a fact of life all the world over in several walks of life not just scientific research. This was indeed done for the assistance provided to Dr. Xu. My advice, guidance, and involvement in terms of helping him have been openly acknowledged all along not only in his *NED* transmittal but also for the earlier unsuccessful attempt with *PRL*. The only claim made in my published January 2006 *PRL* paper “these observations have now been independently confirmed” is indeed accurate. Therefore, the allegation of misconduct in relation to the 2005 *NED* paper by Dr. Xu is unfounded and a red-herring issue.

### **Concluding Remarks**

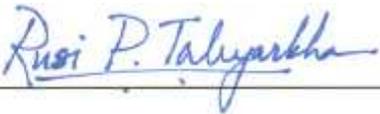
Considering the above points I draw the following conclusions:

- a) That the reported research results were obtained by Dr. Xu from experiments conducted independently, data analyzed independently and conclusions drawn independently. He has signed a testimonial to this effect dated October 27, 2006.
- b) That the guidance and assistance provided to Dr. Xu for helping him to publicize his confirmatory results were deemed appropriate and commonly acceptable, when judged through the eyes of myself and two stalwart colleagues with worldwide experience in research I consulted with before proceeding, both research stalwarts in the field of science-cum-engineering with long-standing research experience in spheres covering academia, national laboratory and

industry. As stated earlier, I myself and my team have received considerable assistance for my publications in *Science* (2002) and *Phys.Rev.E* (2004) publications during the course of reviews by journal editorial staff and referees, and more importantly, the more than hundred scientists who reviewed the manuscripts over several years.

- c) The central allegation made to the C-22 Review Committee by Dr. Tsoukalas on claims of independence as stated in my group's January, 2006 *PRL* journal paper is completely unfounded. I once again reiterate that the claim made in my 1/06 *PRL* publication was "*these observations have been independently confirmed.*" This is indeed factually correct as has been attested to in writing by Dr. Xu to the C-22 Review committee. The *PRL* publication was prepared with input and approval from all of my co-authors spanning two continents, and offered to worldwide referees and to the APS journal editors, none of whom saw anything wrong.

Sincerely,



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Rusi P. Taleyarkhan, Ph.D.

The Arden L. Bement Jr. Professor of Nuclear Engineering

Subject: Referee comments  
From: "Colin/Suzanne West" <herderwest@comcast.net>  
Date: Tue, 7 Dec 2004 12:20:09 -0500  
To: "Rusi\ (Purdue)" <Rusi@ecn.purdue.edu>

**Yes, Reviewer A's comments are so off the wall that it is difficult to see how one could respond.**

- E.G. (a) Of course the neutrons are detected indirectly (in a scintillator). How else can one detect neutrons?
- (b) Time of Flight information. This is nonsense. In these experiments, there is no time zero indicator accurate enough for ToF measurements on fast neutrons.
- (c) Certainly the tritium could be detected in other ways (e.g. mass spectrometry, optical spectrometry, even collecting macroscopic samples and measuring density or boiling point, etc, etc). But beta counting is the standard method and so it is what these authors chose. The "some other decay..." idea that this reviewer mentions is completely ruled out by the control experiments without cavitation.
- (d) I agree that the idea of "seeding" the bubbles with something other than neutrons is an obvious one - so obvious, in fact, that even we thought of it. But such is not the experiment these researchers chose to do, and so it is not the one they are reporting.

I think Referee B's questions and suggestions can, and mostly should, be accommodated. But I feel like the authors could take exception to two of his statements:

- (a) I don't think it is true that the neutron detector "would be swamped with radiation from the  $^{252}\text{Cf}$  source", and the results show that it was not.
- (b) The reviewer seems not to know that it is not the fast neutrons themselves that give the scintillation signal, but their knock-ons. Even if all the neutrons had 2.5 MeV (actually some will have lost energy by scattering on their way to the detector) their knock-ons, and therefore the scintillation signals, will range downwards from 2.5 MeV.

As I said in my earlier comments, I do think that more explanation of the "streamer" cavitation, which evidently puzzled Reviewer B, would be good. I believe that the streamers usually result from too much dissolved gas. In former times, the sharp, popping bubbles would have been called "transient cavitation" and the streamers would have been called "gaseous" cavitation, I think.

**Please feel free to share my comments with the authors if you think it appropriate.**

Colin