

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>

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(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.Mj, 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**

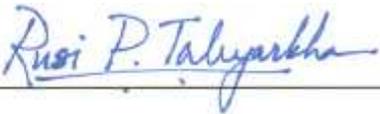
Considering the above points I draw the following conclusions:

- 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

December 8, 2006

Dr. Peter E. Dunn, Ph.D.  
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**Re: Inquiry Committee Proceedings; Taleyarkhan Response to November 30, 2006 letter**

Dear Dr. Dunn:

We write on behalf of Dr. Rusi P. Taleyarkhan (“Dr. Taleyarkhan”) in response to your letter of November 30, 2006.

First, let me say that the answer to your question is: no.

We thank you for the opportunity to respond to the question in your letter. We enclose Dr. Taleyarkhan’s response (“Response”) for your consideration. We want to make it clear that the reason previous submissions did not include, as you say in your letter, a “full disclosure” of the events and information which are reflected in the documents which were enclosed with your letter to us is because Dr. Taleyarkhan and I did not (and do not) believe that those documents are responsive or necessary to this inquiry. That being said, we want to cooperate, as we have throughout, with any and all requests of this Inquiry Committee.

Specifically, we understood the Inquiry Committee was concerned with the Nuclear and Engineering Design (“NED”) paper manuscript of 2005 and not other manuscripts submitted by Dr. Xu. Accordingly, we, as his lawyers, and Dr. Taleyarkhan did due diligence to find correspondence with Dr. Xu regarding the NED paper. There was no attempt to hide any information in any way whatsoever, and we believe Dr. Taleyarkhan’s enclosed response should put this matter to rest once and for all. Because both Dr. Taleyarkhan and his lawyers (namely, me) believed we were answering your questions completely regarding his email correspondence concerning the 2005 NED paper only, we urge the Inquiry Committee not to make any adverse inference against Dr. Taleyarkhan for not including the email information attached to your letter in his previous response to the Inquiry Committee. This was simply a situation of miscommunication or misunderstanding, and not an attempt to hide anything from the Inquiry Committee. If you want to blame anyone, please blame me.

**A Note on the History of this Inquiry**

While it may be redundant, we believe it is important to give a brief history of how this Inquiry began and what Dr. Taleyarkhan's responses were to give some context to his Response. We do this to both try to make the Response as understandable as possible, and also to obviate any feeling by this Committee that Dr. Taleyarkhan tried to hide anything from this Committee, which he did not do.

On September 22, 2006, Dr. Peter Dunn issued a letter defining the parameters of the Inquiry Committee. The letter asked questions based upon two letters from Lefteri Tsoukalas (September 5, 2006) and Martin Bertadano (September 12, 2006). Those letters allege a number of items against Dr. Taleyarkhan, including that he attempted to hide results and that he wrongfully claimed independent confirmation of Xu et al.'s 2005 NED publication for his own benefit. The main charge then from Tsoukalas is that Dr. Taleyarkhan engaged in research misconduct in reporting the independence of Xu's research in a later article, and in somehow influencing the outcome of the experiment by Xu.

In response, Dr. Taleyarkhan presented a lengthy, detailed binder to the Inquiry Committee, which showed that Xu's research was independently obtained, and that Dr. Taleyarkhan did not influence any data in what amounted to a lengthy experiment conducted by Xu himself. Dr. Taleyarkhan also answered each and every allegation raised by Tsoukalas and Bertadano. Through use of science and email evidence, Dr. Taleyarkhan showed the Inquiry Committee the detail of facts and circumstances refuting the allegations made by Tsoukalas and Bertadano. He also showed some of the alarming behavior of Tsoukalas and Bertadano for the benefit of the Inquiry Committee, recognizing that the true matter before the Inquiry Committee was and is, first and foremost, the science and the question of research misconduct.

Both Tsoukalas' and Bertadano's letters are a bit confusing, but we read them to say Dr. Taleyarkhan was being accused of research misconduct because he had claimed in his paper of independent confirmation of his sonofusion claims. In his response he tried to include everything in his possession which relates to the claim that the "*observations*" confirm his results. He went back and performed due diligence on my correspondence, emails, etc. He tried to find the materials that relate to the NED article. That is what he provided to you previously.

In those materials, Dr. Taleyarkhan tried to point out, with the simplest analogy he could think of, the situation relative to his claims. Simply put, Dr. Xu merely repeated the experiment and made observations about what he saw and measured as a result of the experiment. Dr. Taleyarkhan had no involvement in gathering or influencing that data. This is where his "airplane analogy" came from. Dr. Xu took the same (or similar) airplane, attempted to fly it, and determined that it would, in fact, fly. We know that this may be overly simplistic, but it is extremely important.

Now, after showing how Xu's research was in fact independently obtained and gathered (a fact that Xu confirmed, again, in his October 27, 2006 correspondence to the

Inquiry Committee), the question turns to the December 14, 2004 and December 17, 2004 email correspondence, now switching the inquiry to research misconduct in the reporting of two unpublished manuscripts not originally mentioned by Tsoukalas, Bertadano, or Dr. Dunn.

While we believe that this has nothing to do with the charges of research misconduct made against Dr. Taleyarkhan which was understood to be that his single claim of a confirmatory experiment was wrong or unfair because he had influenced the results of that experiment (which claim appears to now have been decided favorably for Dr. Taleyarkhan by this Committee), in an attempt to provide information to allow this Committee to completely dispel any need for further investigation, Dr. Taleyarkhan's detailed Response is enclosed.

In simple summary, Dr. Taleyarkhan's actions in writing this email to Dr. Xu (based in large measure on comments made to him by others whom the scientific world trusts and respects):

1. had nothing to do with the independence of Xu's observations, calculations, etc.;
2. were comments about facts concerning the set-up and measurements of the experiment which was already completed
3. were made in connection with a different paper that was not published;
4. are of the type customarily and repeatedly made by colleagues when they are asked questions of this type. Dr. Taleyarkhan certainly had help from others in writing most of his papers which, as we would suggest is true of the members of this Committee, is the most natural thing and is what we are supposed to do as colleagues and professionals; and
5. were recognized by the authors in the paper that was actually published when they reference all of the help that Dr. Taleyarkhan gave them, which did not interfere with the paper being published, after review by whomever reviewed it (we don't know who that was) as being confirmatory in nature, despite the fact the credit was given to Dr. Taleyarkhan for his involvement with the experiment.

To reiterate, the December 14, 2004 and December 17, 2004 emails, do *not* indicate research misconduct in any way whatsoever.

We believe Dr. Taleyarkhan's Response should put this matter to rest so his reputation can be cleared and he can continue his extraordinarily important research. While we appreciate the necessity for the Committee to take its time completing its work, we know you will understand the pressing need for finalization. If this Committee is going to decide, as we think is clearly required by the evidence presented, that there is no need for further investigation, it would be *very* important for that to happen this Friday.

We realize that the Committee has taken no oral testimony and understand its decision not to do so. Please accept this offer on Dr. Taleyarkhan's behalf that if the Committee has any further questions or needs any clarification of any points, Dr. Taleyarkhan is ready, willing and able to answer those questions in person on Friday. Please let me and/or him know if his appearance might be helpful.

Very truly yours,

Larry Selander

LZS/rcb  
Enclosure

LARRY SELANDER  
DIRECT DIAL (312) 499-0147  
E-MAIL: lselander@duanemorris.com

www.duanemorris.com

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December 20, 2006

Dr. Peter E. Dunn, Ph.D.  
Associate Vice President for Research  
Research Integrity Officer  
Office of the Vice President for Research  
Hovde Hall of Administration  
Third Floor  
610 Purdue Mall  
West Lafayette, IN 47907-2040

**PRIVILEGED AND CONFIDENTIAL**  
**ATTORNEY WORK PRODUCT**

**Re: Facts-Correction Regarding Memo from Dr. Dunn Dated December 15, 2006**

Dear Dr. Dunn:

Upon review of your letter dated 12/15/2006, Professor Taleyarkhan has noted several factual errors made by the Inquiry Committee ("IC") – perhaps due to their lack of familiarity with the specifics of technology associated with acoustic inertial confinement (bubble) fusion or perhaps due to our inability to explain things completely.

This letter is, therefore, submitted as a respectful request to correct some facts that appear to have been overlooked or misconstrued during the course of deliberations by the IC. It is submitted that these factual errors led to the several admonitions related to misjudgment. This mainly concerns the issue of what constitutes independent confirmation.

Per my meeting yesterday with Mr. Kealey, I understand it may be possible, if you agree with the contents of this letter, to strike and reissue your 12/15/2006 letter or issue an addendum to it.

**On page 3 (item 3)** of the letter the IC offers the following as needed characteristics for independent confirmation:

- 3a. The experimental apparatus with which the experiment was performed would be significantly different from that used in the initial experiments. In particular, the critical gamma-ray detector (Beckman Instruments) would not be the very same one (with the same calibration curve) used in the earlier experiment.
- 3b. The experiment would be performed by scientists who were not associated with Purdue University, and certainly not by individuals having close relationships to Dr. Taleyarkhan.

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3c. The involvement by Dr. Taleyarkhan would be minimal.

1) **Facts related to the 2005 Confirmation Experiments by Xu et al. reported in NED journal**

In relation to item 3a above, the reading of the IC is factually wrong when applied to this situation. The IC deemed this to be a "critical" detector system used by Xu et al for their 2005 NED paper, which should not have been the same as the one used by Taleyarkhan et al. With due respect, the IC was wrong in making the assumption that it was the same detector. In fact, it was *not*. As an aside, the LS6500 Beckman spectrometer is not a gamma-ray detector as quoted in the 12/15/06 Dunn memo; it is a beta spectrometer. Tritium emits beta rays, not gamma rays, during decay.

I attach hereto documented evidence that the LS6500 Beckman spectrometer used by Xu was *not the same*, and *neither was the calibration curve*, as the one used by Taleyarkhan et al. (Science, 2002). The instrument used by Xu et al. for their studies was *different* and was provided to Purdue University on loan by DOE's Dr. Michael Murray (a recognized expert with over 20y experience in tritium detection) who, at Taleyarkhan's request, personally traveled to Purdue to not only deliver it, set it up, and conduct the checks needed, but then he (Murray) performed the calibrations to develop the calibration curve used later at Purdue. Thus, the "critical" machine-instrument was totally different as is attested to by Dr. Murray. Furthermore, in Xu's experiments, the detection was performed with the organic liquid-based Ultima-Gold scintillation cocktail versus the use of an aqueous-based Ecolite cocktail by Taleyarkhan et al.

Further, the test reactor cells undergoing sonofusion used in Oak Ridge and at Purdue were distinct and different. The electronic components and data acquisition systems used at these two places were different. The location of experiments was obviously different. The experimental design and approach utilized by Xu et al at Purdue for their 2005 NED paper were also *not the same*, but very different in a fundamental way. Whereas, Taleyarkhan et al. used an accelerator based 14 MeV neutron microsecond-pulsed neutron source, Xu et al. at Purdue used a continuous spectrum and randomly emitting neutron sources, both Cf-252 and Pu-Be. Their experimental geometry was also radically different and Taleyarkhan had nothing to do with this experimental design which, basically, was dictated as a means to get around the fact that Purdue's Nuclear Engineering School simply did not have the apparatus available to Taleyarkhan et al. at Oak Ridge National Laboratory.

Finally, as the IC has itself confirmed during it's review of evidence, Xu et al. conducted the experiments, acquired the relevant data and analyzed the data independently *with no participation* by Taleyarkhan et al. We submit that these changed facts would also lead to a different conclusion relative to Dr. Taleyarkhan's "lack of judgment."

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**2. Confirmations by Forringer et al., and Bugg et al. in 2006**

The IC's conclusions relative to requirements of the 3a,b,c characteristics to the Forringer and Bugg confirmation experiments are also factually wrong.

For the record, the May, 2006 visit by Professor Forringer and his two students was one in which the group has gone on public record in their university's November 17, 2006 Press Release as stating "Two students and I went to Purdue University in May to conduct our own research, collecting, analyzing, and interpreting our own data that substantiated his previous work." Indeed, Forringer and his students used randomly selected detectors manufactured by a private company Landauer, Inc. that were *not previously used* at Purdue. These were *new and different* detectors.

Furthermore, regarding the independence-related criterion for personal associations, the fact is that Dr. Forringer's University approached Taleyarkhan not the other way around. Forringer et al. from Texas University requested to use Taleyarkhan's laboratory and system as a user-facility (as is common practice in the world today) during early 2006 after the 3/2006 nature articles came into being. Prior to this Taleyarkhan had never met, nor known of Forringer or his students. Forringer et al. were *independently sponsored* by LeTourneau University under a grant they procured from the Welch foundation.

Even more importantly, Forringer was completely new to the sonofusion field and came without preconceived notions and, therefore, to Taleyarkhan he and his institution represented a request that came without prejudice, unlike others (detractors and supporters) already in the field. Forringer et al. performed their *own* calibrations, conducted their tests with their *own* randomly selected detectors, read their *own* data including from the crucial neutron track detectors for unambiguous tell-tale signs of neutron emissions, then wrote their *own* paper with *no* input from Taleyarkhan and submitted and defended their own paper. Furthermore, they performed their own due diligence checks/surveys to negate the charges of contamination from Cf-252 made by Taleyarkhan's detractors as cited in 3/2006 Nature articles. Therefore, it is submitted that, the IC's three characteristics for independent confirmation were satisfied in the Forringer case.

The same statements can be made for Professor William Bugg (past Head of Physics at University of Tennessee and currently research professor at Stanford University's SLAC facility) who also attempted to confirm Taleyarkhan's experiments conducted at Purdue.

Therefore, in addition to the 2005 confirmation report by Xu et al., in 2006 alone there are an additional 2, not 1, set of scientists/physicists groups who reported confirmation separately and

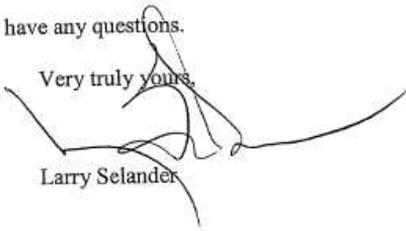
Duane Morris

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meet the spirit of the 3a,3b,3c characteristics needed for independent confirmation. We believe all of this information was submitted in the binder in detail to the IC by Taleyarkhan as part of this inquiry. Perhaps we did not explain it properly.

I look forward to discussing this with you if you have any questions.

Very truly yours,



Larry Selander

cc: R. P. Taleyarkhan, Purdue University  
W. Kealey, Stuart and Branigan

Selander, Larry

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From: Selander, Larry  
Sent: Wednesday, December 20, 2006 3:44 PM  
To: Selander, Larry  
Subject: FW: LSC memo

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From: "Murray, Michael E (ANN)" <murrayme2@y12.doe.gov>  
To: "Rusi Taleyarkhan" <rusitaleyarkhan@msn.com>  
Subject: LSC memo  
Date: Mon, 18 Dec 2006 12:49:18 -0500  
>Here it is.

>  
>Michael Murray  
>Radiological Assistance Program  
>865-574-5838 pager: 865-873-4122  
>

>-----  
>  
>December 18, 2006

>  
>Dear Professor Taleyarkhan,

>  
>Per your request I am writing to confirm that during December, 2003:

>  
>- As a goodwill gesture per your request to me and to assist Purdue's  
>efforts in the area of tritium spectrometry, I personally transported  
>the Beckman LS6500 spectrometer to Purdue on loan from the Department of  
>Energy's Oak Ridge National Laboratory,

>  
>- This particular spectrometer that I arranged for loan to Purdue is  
>different from the instrument utilized by the ORNL Biology staff in  
>their facility at DOE's Y-12 plant where your group's bubble fusion  
>samples (reported in Science, 2002) were analyzed for tritium,

>  
>- The above-mentioned spectrometer was an instrument in my custody as an  
>excess item from the many purchased by ORNL in the past (that at the  
>time in 12/2003 was not needed by the then active research groups in Oak  
>Ridge) and hence by procedure, ORNL could loan it to educational  
>institutions for approved activities,

>  
>- I have over 25 years experience in beta-and other nuclear emission  
>related spectroscopy at various laboratories. I personally set up this  
>instrument at Purdue University and conducted the needed checks for  
>functionality and developed the calibration curve using NIST traceable  
>standards, and provided guidance on proper usage for low-level tritium  
>counting before departing,

>  
>Beckman is a premier instrument manufacturer and has a reputation for  
>producing high-quality liquid scintillation instruments. Regardless,  
>instruments manufactured by the same manufacturer will have their own  
>measurement capabilities and detection efficiencies which can be  
>affected due to high-gain PMTs, scintillation cocktails, and the  
>environments in which they are utilized. However, by knowing the  
>specifics of an instrument and using established analytical techniques,  
>a sample's tritium concentration can be consistently determined using  
>various instruments. Please contact me if any additional clarification  
>is needed regarding the loaned instrument or liquid scintillation in  
>general.  
>

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12/20/2006

>Sincerely,  
>  
>  
>Michael Murray  
>Senior Scientist  
>DOE Radiological Assistance Program  
>