

STCC Emails with regard to the ST-FNS plasma stability assumptions, Dec09  
Copied as input to discussions in 2010

**From:** Peng, Yueng Kay Martin

**Sent:** Tuesday, December 15, 2009 9:11 PM

**To:** Steven A. Sabbagh; Majeski, Richard; Peng, Yueng Kay Martin

**Cc:** Menard, Jon; Fonck, Raymond; Baylor, Larry R.; LaHaye, Rob; Tritz, Kevin; Allain, Jean Paul; Sontag, Aaron C.; Hassanein, Ahmed; 'Fred Levinton'; Eckstrand, Steve; Podder, Nirmol; Lloyd, Brian; Nelson, Brad E.; Takase, Yuichi

**Subject:** Scientific basis for JET-level FNS plasma parameters

Dear Steve,

Let's talk about the questions I introduced today, which addresses the issues you have raised, but focusing first on the JET-level case producing 0.25 MW/m<sup>2</sup> in WL.

From there we will be ready to move on to higher performances. This approach is needed to deal with the confusion of the FNS and the CTF missions. Your earlier questions exhibited this confusion.

Your many emails appeared last week when Section 4 was being debated and finalized. We accounted for many of your suggestions, but were not able to satisfy all of everyone's differing ideas. Email exchanges with Jon last Friday provides some clarification on the FNS mission. Again please take a look at those and consider accepting the FNS mission proposed by ORNL.

This mission, included in the STCC report, already answered your earlier and present questions.

I will be pleased to detail the logical structures of the FNS mission relative to the macro MHD, if you would be willing to start with the logic presented in my long email to you today.

Let me know if you would be willing to do so.

Best regards,

Martin

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**From:** Steven A. Sabbagh <sabbagh@pppl.gov>

**Sent:** Tuesday, December 15, 2009 5:51 PM

**To:** Peng, Yueng Kay Martin <pengym@ornl.gov>; 'Dick Majeski' <rmajeski@pppl.gov>

**Cc:** jmenard@pppl.gov <jmenard@pppl.gov>; 'Raymond Fonck' <rjfonck@wisc.edu>; Baylor, Larry R. <baylorlr@ornl.gov>; 'Robert La Haye' <lahaye@fusion.gat.com>; 'Kevin Tritz' <ktritz@pppl.gov>; 'Jean Paul Allain' <allain@purdue.edu>; Sontag, Aaron C. <sontagac@ornl.gov>; 'Ahmed Hassanein' <hassanein@purdue.edu>; 'Fred Levinton' <levinton@pppl.gov>; 'Steve Eckstrand' <steve.eckstrand@science.doe.gov>; 'Nirmol Podder' <nirmol.podder@science.doe.gov>; 'Brian Lloyd' <brian.lloyd@ukaea.org.uk>; Nelson, Brad E.

<nelsonbe@ornl.gov>; 'Yuichi Takase' <takase@k.u-tokyo.ac.jp>

**Subject:** RE: the STCC document process, and exposing committee to unsupported statements

Hi Martin,

Still didn't read this entire email yet, but just a quick reply. Note that I have made more extensive comments through the revised versions, in email (repeated many times), and I don't want to do that again here to any large extent.

To summarize this one issue in four lines:

Appendix G shows FNS to cover a range of operation that should be lower  $l_i$  than NSTX typically operates (expected to have less kink stability – perhaps at any BetaN), and from other work, higher kappa and lower triangularity. BetaN = 3.8 for phase II, BetaN = 5.9 for phase III. Even in phase II, one cannot say that this is “far from all known stability limits”, as we routinely see significant instabilities (kinks, RWMs, NTMs) at these levels on NSTX even at high  $q$ . FNS  $q_{cyl} = 3.7$  or less (which is low).

This would stick out like a sore thumb to any serious review committee member – easily construed as whistling in the dark.

Whistling in the dark isn't so bad, it's the low, but finite probability of reduced professional integrity that I'm more concerned about.

Thanks again.

Steve

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**From:** Peng, Yueng Kay Martin [mailto:pengym@ornl.gov]

**Sent:** Tuesday, December 15, 2009 4:35 PM

**To:** Steven A. Sabbagh; 'Dick Majeski'

**Cc:** jmenard@pppl.gov; 'Raymond Fonck'; Baylor, Larry R.; 'Robert La Haye'; 'Kevin Tritz'; 'Jean Paul Allain'; Sontag, Aaron C.; 'Ahmed Hassanein'; 'Fred Levinton'; 'Steve Eckstrand'; 'Nirmol Podder'; 'Brian Lloyd'; Nelson, Brad E.; 'Yuichi Takase'

**Subject:** RE: the STCC document process, and exposing committee to unsupported statements

Steve,

Thanks for quick response.

We will await your answers to the questions in my email focusing on the scientific basis for your statements with regarding to whether “conservative stability conditions” and “operation far within the known stability limits...” are indeed “not supportable.”

Apologies for discussing using emails. They provide reference-able scientific information as focus of important discussions and debates, which we are sure that you also focus on. This has the side benefit and avoid using only opinions.

Have a great holiday.

- Martin

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**From:** Steven A. Sabbagh [mailto:sabbagh@pppl.gov]  
**Sent:** Tuesday, December 15, 2009 3:50 PM  
**To:** Peng, Yueng Kay Martin; 'Dick Majeski'  
**Cc:** jmenard@pppl.gov; 'Raymond Fonck'; Baylor, Larry R.; 'Robert La Haye'; 'Kevin Tritz'; 'Jean Paul Allain'; Sontag, Aaron C.; 'Ahmed Hassanein'; 'Fred Levinton'; 'Steve Eckstrand'; 'Nirmol Podder'; 'Brian Lloyd'; Nelson, Brad E.; 'Yuichi Takase'  
**Subject:** Re: the STCC document process, and exposing committee to unsupported statements

Hi Martin,

Yes – happy holidays (to come ! ).

I will read what you say below, but without me reading anything, you are making my point just by writing this email – the general process of iterations during this assignment has been to argue points by email, and not make changes in the document.

A strong statement, such as a point design being “far from known stability limits”, requires a lot of support., including present experiments. Given the range of values in the FNS appendix, the statement can’t be made. It has the potential (hopefully, with very low probability) of damaging the reputation of people that sign off on the statement. Why expose the committee to that? And, for what purpose? I can’t see why making the statement strengthens the FNS position – I think, as stated, it weakens it.

In any case, the simple deletion of the phrase would have solved that particular issue, but as I said earlier, the high majority of all suggested changes (at least mine, perhaps Dick’s?) were ignored, especially in the last iteration.

Again – I will read what you say below, think about it, and reply in time. Continued communication is good.

Thanks again for the hard work put into this effort.

Steve

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**From:** Peng, Yueng Kay Martin [mailto:pengym@ornl.gov]  
**Sent:** Tuesday, December 15, 2009 3:25 PM  
**To:** Steven A. Sabbagh; 'Dick Majeski'  
**Cc:** jmenard@pppl.gov; 'Raymond Fonck'; Baylor, Larry R.; 'Robert La Haye'; 'Kevin Tritz'; 'Jean Paul Allain'; Sontag, Aaron C.; 'Ahmed Hassanein'; 'Fred Levinton'; 'Steve Eckstrand'; Peng, Yueng Kay Martin; 'Nirmol Podder'; 'Brian Lloyd'; Nelson, Brad E.; 'Yuichi Takase'  
**Subject:** The process worked, while utilizing knowledge base of ST, Tokamak and ITER

Dear Steve,

Greetings for Happy Holidays and New Year. Please consider responding to my questions soon after the holidays. I will follow up then. Now in response to your email about the “not supportable” issue:

I am glad to note that several of the committee members expressed to me their satisfaction that a good STCC report was produced on schedule, through the challenging process of our work during the past 3 months.

I am also glad that we now have some time to sort out in more detail the scientific and technical basis for certain topics in the STCC report with majority agreement, where debates still remain. Pardon me for repeating some of the ideas already exchanged on the FNS stability and control but still not resolved at present.

Also, since you support Dick's points in his email below, please do take a look at my response to his email, and tell me where you think the logic I presented might have been flawed. Email exchanges with Jon on Friday last week remain germane to this discussion also. Please take a look at those, and tell me where my responses to Jon was in error. We am waiting for Dick and Jon to answer those questions.

This would be a good time to put all the relevant ideas on the table, and clarify the facts that have been quoted as basis for opinions. I hope to offer new opportunities for working with the STCC to resolve these standing questions, which remain unresolved at this moment. It is credible for the STCC to recognize these as unresolved. The STCC should allow time to resolve them involving the interested members.

As you well know, the credibility of the STCC in the fusion community rests on our ability to get to the bottom of important questions on the research needs. I am glad that you have been willing to engage in this level of discussion.

I am pleased with the depth and breadth of your knowledge in the ST stability control area. Our engagement and debates in this important and complex topic have not happened by accident.

As we wait for Dick and Jon to answer those standing questions, the following will expand on the questions I have already raised to you in previous emails. Since your present email focus on

the research need in macro stability driven by the FNS mission and embodiment, I will parse the questions for your considered reply.

Your email: FNS mission with “conservative stability conditions” and “operation far within the known stability limits” are not supportable.

- a. As indicated in Appendix G, the FNS mission begins to be addressed with a plasma with JET-level plasma pressure. Please tell us if this is where you think to be “not supportable.” However, if this is supportable, then,
- b. The example embodiment indicates typical parameters such as:  $q_{cyl} \sim 9$ ,  $\beta_T \sim 5\%$ ,  $\beta_N \sim 2$ ,  $T_i\text{-avg} \sim 5.4$  keV, and  $T_e\text{-avg} \sim 3.1$  keV (thus, collisionality  $\sim 0.001$ ). Same question to you. If this is supportable in your view, then,
- c. The references for this type of designs indicates the need to maintain appropriate plasma profiles, including  $q_0 > 2$  or  $> 3$ . The question to you would be: should such plasmas be considered to have “conservative stability conditions” and “operation far within the known stability limits”? We await your reply. If this turns out to be supportable in your view, then how about the following remaining physics assumptions?
- d. Assuming appropriate plasma profile control (active but slow in response time, such as using fueling, heating, torque profiles, current drive profiles, etc.) to maintain adequate margins to all known macro-MHD instabilities, and minimization of resonant error field, either by design or by additional trim coils, and success of non-inductive continuous plasma operation without active mode feedback control, then this is a plasma scenario with “conservative stability conditions” and “operation far within the known stability limits”. Is this not supportable in your view? We would like to learn the details of the scientific and technical basis that shows this kind of plasma conditions to be “not supportable”. We await your explanation.
- e. Since neither NSTX nor MAST at present plan to produce and study this type of plasmas, one has to look to the Tokamak database for actually produced examples. To be accurate about NSTX data, there was at least a HHFW-only shot in 2003 at about 200 kA that indicated nearly zero loop voltage (large  $\beta_T$  and bootstrap fraction, small  $\beta_N$  and  $\beta_T$ ) for a large fraction of 1 s in duration, with  $q_{cyl} \sim 10$  ( $q_{95} \sim 30?$ ). I do not remember whether this plasma indicated large MHD instabilities, or required active feedback MHD mode control. I am hopeful that with adequate RF, both NSTX and MAST can access this plasma regime of interest to FNS. If we have missed some other important results already obtained on NSTX, please tell us.
- f. Of course evidences for this type of plasma abound in the Tokamak database (sorry to repeat an earlier email to you). Very long pulse plasmas in Tore Supra, JET, HT-7 (400 s RF sustained plasmas with couple keV  $T_e$ ), etc., have had conservative stability conditions, and somehow did not use active mode control for the entire plasma duration. We may have missed something important in thinking such examples would support the vision of the FNS plasma. Please tell us if this is the case.

It is entirely possible that I have missed something important still, or incorrectly quoted what you mean, in the logic presented above. If so, please point them out to us, for it is important for us to learn this. Of course, there is no need to respond if there is nothing missing in the above logic.

Then we can move on to discuss other questions, with regard to other topics for FNS, and the basis for the high-gain BP and PMI assumptions.

Thanks again for your patience as the details are enumerated. With kind regards,

- Martin

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Fax: 865-576-7926 (w); E-mail: [pengym@ornl.gov](mailto:pengym@ornl.gov)

**From:** Steven A. Sabbagh [mailto:sabbagh@pppl.gov]  
**Sent:** Monday, December 14, 2009 6:51 PM  
**To:** 'Dick Majeski'; Peng, Yueng Kay Martin  
**Cc:** jmenard@pppl.gov; 'Raymond Fonck'; Baylor, Larry R.; 'Robert La Haye'; 'Kevin Tritz'; 'Jean Paul Allain'; Sontag, Aaron C.; 'Ahmed Hassanein'; 'Fred Levinton'; 'Steve Eckstrand'; 'Nirmol Podder'; 'Brian Lloyd'; Nelson, Brad E.; 'Yuichi Takase'  
**Subject:** Echoing Dick Majeski's words re: latest STCC document and process - let's tell it like it was, and do better next time

Dear Martin,

This is not an easy process, and the panel should thank you for your hard work. That said, I have to agree with Dick – not just from my reading of the significant email on the subject he mentions below, but also because I've experienced the same issue regarding specific suggestions on the recent STCC document being ignored regarding plasma stability.

It is mildly annoying when panel members, who should be trusted for their experience, are argued against via email, and even if they find support from panel members or other experts, their input is ignored where's it's important – in the written document. Colleagues who are proactive in the process then have simply wasted time (and, in some cases, travel budget when people are asked to travel to STCC meetings, as was the case this time).

Of course, it's to be expected that people will have differences of opinion, and so, at worse, if comments are not acted on, it's usually not so bad as long as the message that remains is supportable. But, in some important instances in the present document, they are not supportable.

So, like Dick, once I answered the final call for comments on a very tight schedule (this time, it was 24 hours), I thought that most would be addressed. Instead, most were not addressed (not counting too carefully – looks like about 85% are ignored, even though the number and size of requested changes were small).

Luckily, most instances did not refer to unsupportable statements, but unfortunately, some did, and those are professionally bothersome. It's folly to bring up all key points again (I think there are about 5 or 6). To be brief and simple, let me just mention two. It would be simplest to just say nothing. But I find it hard to ignore this when statements such as

(i) Page 3: “conservative stability conditions” (regarding the FNS mission – could have been simply deleted), and

(ii) Page 4: “operation far within the known stability limits...” (regarding the FNS mission – could have also been simply deleted)

are not supportable. These are over-optimistic statements about the FNSF when considering Appendix G. I don't suggest we give DOE or the community these impressions. (Separately, it's a matter of opinion whether-or-not it's wise to give such an impression if we in fact want to see such a device work).

Again, this is not an easy assignment. Perhaps at this point, the best way forward is just to do better next time. But, in order to do that, we have to acknowledge that this process was really not very efficient, panel members were rushed in many instances to review documents before meetings and conference calls (sometimes given a matter of hours to give comments), and when issuing those comments, finding that the issues, even small changes, were not addressed. Maybe others had a different experience.

I realize from my small experience that this is not easy, so thanks for your hard work.

Thanks,

Steve

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**From:** Dick Majeski [mailto:rmajeski@pppl.gov]

**Sent:** Saturday, December 12, 2009 1:01 PM

**To:** Peng, Yueng Kay Martin

**Cc:** Steve Sabbagh; jmenard@pppl.gov; 'Raymond Fonck'; Baylor, Larry R.; 'Robert La Haye'; 'Kevin Tritz'; 'Jean Paul Allain'; Sontag, Aaron C.; 'Ahmed Hassanein'; 'Fred Levinton'; 'Steve Eckstrand'; 'Nirmol Podder'; 'Brian Lloyd'; Nelson, Brad E.; 'Yuichi Takase'

**Subject:** Re: New suggestions re: appendices - STCC document - FNS section the only major issue.

Martin:

I chose to step back from this argument because of my concern that it was distracting from the main task at hand, that of completing the white paper for Steve and Nirmol.

There are three members of this committee who have backgrounds in PMI, Ahmed Hassenein, Jean-Paul Allain, and myself - although I freely admit that I only began work in this area ten years ago, and most of my experience is with liquids. The same, of course, cannot be said of J-P and especially Ahmed, who is certainly a well-established leader in the field.

I am concerned that you chose to overrule all three of us, who were unanimous in disagreeing with you, in formulating an evaluation of the potential impact of liquid metal PFCs. What is the purpose of enlisting the help of experts in a field, if their views are ignored in formulating a committee report?

I have also answered the question you posed yesterday, below.

Dick