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UCRL-TR-220518

SUMMARY REPORT-FY2006 ITER WORK ACCOMPLISHED

N. N. Martovetsky

April 11, 2006

This work was performed under the auspices of the U.S. Department of Energy by University of California, Lawrence Livermore National Laboratory under Contract W-7405-Eng-48.

University of California
Lawrence Livermore
National Laboratory

Summary Report- FY2006 ITER Work Accomplished

Proposal # : D10583
Proposal Title : PART-TIME SECONDMENT OF NICOLAI MARTOVETSKY
Customer Name : DOE-PRINCETON PLASMA PHYSICS
Expiration Date : 31-MAR-06

SOW# 615-20050223-SCHOEN-1

April 7, 2006

This report is the deliverable for N. Martovetsky work on ITER design for the period from Oct. 1, 2005 till March 30, 2006 funded by US ITER Project Office.

During this period I, Nicolai Martovetsky, worked on several design issues of the ITER Central Solenoid, CS Procurement Package and Schedule. I worked at the ITER Joint Work Site in Naka, Japan and LLNL site.
I made two trips to the ITER International Team Joint Work Site in October-November 2005 and January-March 2006.

The progress report on the trip in October –November 2005 is given in the Attachment 1.
The progress report on the trip in January- March 2006 is given in the Attachment 2.
During my work on ITER magnets I performed also several studies and issued the following reports:

1. **Memo on the safety margin in the ITER PF coils (Attachment 3), where I showed that there is no big incompliance of the PF expected performance with ITER specifications. The ITER PF performance expectations were unreliable from the start, since the NbTi strand was not characterized yet, but correction does not cause significant problems.**
2. **Memo on the forces in bus extensions (Attachment 4), where I showed that some of the problems in structural analysis of the buses in the CS are artificial, since forces are supported by the OD of the CS.**
3. **Memo on the lower wedges in the preloading mechanism in the CS structure (Attachment 5) shows that the lower wedge is not useful, too weak to raise the coil, therefore design change is necessary.**
4. **Memo on design of the reinforcement of the tie plates (Attachment 6) which should help supporting bus extension with low stresses**
5. **Memo on investigation of where the high stresses in the flexible plates (spring leaves) are coming from (Attachment 7), showing that it comes from the vertical force on the CS, not from the internal stresses in the winding pack as it was erroneously thought before**
6. **Sketch of the modified bracket design for the CS suspension (Attachment 8)**
7. **Update on the cooling memo (Attachment 9) to my previous analysis of the CS structure cooldown with emphasize on design of the cooling tubes.**
8. **I developed a sketch for ITER designer for specification of the turn insulation for the CS conductor (Attachment 10)**
9. **In many structural analyzes plasma current generates significant forces on the magnets. Simulation of plasma current is complicated since plasma shape changes during the scenario. I compared the field generated by plasma current on Central Solenoid (given by precise plasma current configuration) and a turn 1x1m2 located at the plasma current center of gravity and showed that for structural computations such a representation is adequate (Attachment 11).**

In addition to the above documents I reworked the CS Procurement Package (two files) and gave my input to the Integrated ITER Magnet Acquisition Plan (one file). These documents are large and including them into this report is irrational. They will be submitted electronically to S. E. Schoen, PPPL ITER Project Planning & Control Officer.

This work helped to solve some of the structural problems, identified ways to solve other outstanding issues and reduced uncertainties and risk in the CS design.
This work will continue in collaboration with ITER IT until a task agreement will be signed between the ITER ILE and the US IPO and all design activity on the CS will be transferred to the US IPO. It is expected that the Joint Work Site will be relocated from Naka, Japan to Cadarache, France – ITER construction site in October 2006.

Attachment 1

Foreign Trip Report

Traveler's Name(s)
Nicolai Martovetsky

Trip Dates:
10/12/05-11/18/05
Location:
Naka, Japan

Physics & Advanced Technologies Directorate
Division/Program: Fusion Energy Program
Group Name: Fusion Technology

Lawrence Livermore National Laboratory

University of California
Livermore, CA 94550

Contract No. W-7405-Eng-48

Trip Report Date:_______November 23, 2005__________

Approved for the Associate Director
Physics and Advanced Technologies

____
Date

Administrative Contact: Judy Knecht________________

Part 1—Trip Summary

Travel to: Naka, Japan

Report Date: November 23, 2005

Travel Dates: October 12- November 18, 2005

Traveler:

Name: Nicolai Martovetsky Engineer

FE/Fusion Technology
(925) Phone #422 4269
L-641, P. O. Box 808
Livermore, CA 94550

FTMS trip number: 200516353

Destinations: Naka, Japan

Purpose:

1. Advancing design of the CS critical issues
2. Other selected ITER magnet tasks as required by International Team

People contacted:

1. K. Yoshida
2. Y. Takahashi
3. N. Mitchell

Abstract
Six parties (EU, Japan, Russia, US, Korea, China) will build ITER. The US proposed to deliver at least 4 out of 7 modules of the Central Solenoid. Phillip Michael (MIT) and I were tasked by DoE to assist ITER in development of the ITER CS and other magnet systems. We work to help Magnets and Structure division headed by Neil Mitchell. During this visit I worked on the selected items of the CS design and carried out other small tasks, like PF temperature margin assessment.

Part 2—TRIP REPORT

Purpose

1. Advancing design of the CS critical issues
2. Other selected ITER magnet tasks as required by International Team

Trip report

During my stay in Naka I worked on the following tasks.

1) Improving CS design issues
2) Developing more detailed design of the CS and improving/creating new drawings
3) Assessment of the PF NbTi conductor margins

The CS analyses showed several problematic areas, which need to be resolved; an acceptable design was not identified yet. These problematic areas include:

a) Inlet ports – all designs so far failed to produce acceptable stresses, fatigue or static problems. The major problem seems to be the large vertical force which needs to be designed out. In addition, the fatigue resistance can not be accessed only theoretically – requires experimental verification.
b) Vertical transitions at the ID – this task is also somewhat related to the inlets, since the inlets are located on the transitions. P. Titus analysis showed unacceptably high stresses at the bends of the ramp. Needs to be redesigned to relieve vertical pressure and reduced stresses at bends.
c) Bus supports – large differential movement between the tie plates, which shall support buses and the OD of the CS, where the buses are coming from. No satisfactory solution yet, several approaches are promising.
d) Leaf springs supporting the dead weight and axial EM forces – unacceptably high stresses in the leaf springs due to excessive radial movement of the gussets on the CS top.
e) Lower centering mechanism – too large potential movement under de-centering forces
f) Preloading – some questions about procedure and elements.

During my stay I worked on these issues to different extent. Only one issue is resolved with high probability of success (subject to verification by a detailed structural analysis). This is the intermodule structure, which contains adjustable keys and provides a recess for rigging the CS modules for stack up. This solution significantly reduced the cost and improves reliability. I assisted Yoshida-san in creating drawings of these interfaces.

Other problems remain to be solved, some progress is being made there are some ideas to be tried in analysis.
I wrote a memo of assessment on the inlet problem and justified fatigue testing at LHe temperature to support CEA Cadarache testing plan. The test hopefully will take place in the forthcoming months.

I did hand calculations on the vertical transitions and came to conclusion that the stresses should not be as large as some ANSYS analysis suggested. Need to resolve that with an ANSYS analyst.
I computed loads on the buses and proposed some clarifications and simplifications for analysis, which hopefully will improve the situation.
I reviewed Mr. Fu structural analysis draft report and suggested a few improvements.
I reviewed P. Titus draft report on structural analysis issues and made several suggestions.
I checked magnetic fields generated by plasma at EOB, computed by my request by plasma control group and showed that for structural analysis the representation of the plasma with a single turn is acceptable. I wrote a memo on this subject.
I discovered that the lower wedge in the preload mechanism can not be used for preloading control as intended, since the bolts, pulling the wedge are not strong enough to lift the weight of the CS modules. The top wedges do not have enough travel to do the preloading. This opened one more problem to be solved. The amount of travel in vertical direction with one wedge is not sufficient to preload the CS. A memo is in preparation.
I analyzed temperature margins for the NbTi PF conductors, which was discovered by D. Bessete to give lower than expected temperature margins. I basically confirmed Denis calculation and came to conclusion that ITER recommended NbTi correlation taken from the LHC strand can deviate significantly from the NbTi strands with different designs or supplied by different fabricators. There was not enough basis to expect that some particular strand would be described satisfactory by some particular correlation and it so happened that the LHC strand had higher predictions than NbTi strands made for the ITER PF. On the other hand ITER selection of the LHC correlation was justified by lack of knowledge about ITER strand. It should have been an iterative process of the correlation correction as material becomes available, which is developing now.
I wrote a memo with recommendations on the issue.

Next visit is planned for January 23- March 3, 2006.
The tasks to work on before and during the next visit will be:

1. Improvement of the CS design issues
2. Write description of functionality of the CS design during fabrication and operation, especially structure
3. Development of more detailed drawings, especially on the test samples (Ic, butt joint, insulation samples) for design verification

Itinerary

October 12, 2005 Departed from San Francisco

October 13, 2005	Arrived to Naka
October 13-November 17	Working at Naka JWS
November 18	Returned to San Francisco

Contacts/Facilities Visited

1. N. Mitchell
2. Y. Takahashi
3. K. Yoshida

no facilities visited

Foreign Trip Report Distribution

W. Marton
S. Milora
N. Sauthoff
W. Meier
E, Synakowski
J. Minervini
T. Antaya
P. Michael

QA File

Attachment 2

Foreign Trip Report

Traveler's Name(s)
Nicolai Martovetsky

Trip Dates:
01/24/06-03/03/06
Location:
Naka, Japan

Physics & Advanced Technologies Directorate
Division/Program: Fusion Energy Program
Group Name: Fusion Technology

Lawrence Livermore National Laboratory

University of California
Livermore, CA 94550

Contract No. W-7405-Eng-48

Trip Report Date: March 13, 2006

Approved for the Associate Director
Physics and Advanced Technologies

Date

Administrative Contact: Judy Knecht________________

Part 1—Trip Summary

Travel to: **Naka, Japan**

Report Date: March 13, 2006

Travel Dates: Jan 24-March 03, 2006

Traveler:

Name: Nicolai Martovetsky Engineer

FE/Fusion Technology
(925) Phone #422 4269
L-641, P. O. Box 808
Livermore, CA 94550

FTMS trip number: 200603465

Destinations: Naka, Japan

Purpose:

3. Advancing design of the CS critical issues
4. Other selected ITER magnet tasks as required by International Team

People contacted:

4. K. Yoshida
5. Y. Takahashi
6. N. Mitchell

Abstract
Seven parties (EU, Japan, Russia, US, Korea, China and India) will build ITER. The US proposed to deliver all 7 modules of the Central Solenoid. I was tasked by US ITER Project Office to assist ITER in development of the ITER CS and other magnet systems. We work to help Magnets and Structure division headed by Neil Mitchell.
During this visit I worked on the selected items of the CS design and procurement documentation.

Part 2—TRIP REPORT

Purpose

3. Advancing design of the CS critical issues
4. Update procurement documentation

Trip report

During my stay in Naka I worked on the following tasks:

4) Revising the CS procurement package documentation
5) Design and analysis of the butt joint tool
6) Developing more detailed design of the CS and improving/creating new drawings

In the area of the CS procurement package I revised scope of work to reflect the new ITER tasks agreed in November by the PTs, namely that now the US is responsible for all of the CS modules and not responsible for the CS conductor fabrication, development and qualification.
The US is still responsible for development and qualification of the butt and lap joints. The CS specifications still lack some details on the qualification work, because it is not finalized. Particular, design on the full scale qualification sample is not complete. I developed a design concept of the butt joint and the lap joint and had it reviewed by Chen-yu Gung, who is responsible for the lap joint development in the US PT. We had several interactions and Chen-yu plans to finalize the design in a 2 months period. Basically the specs and Task requirements reflect the latest understanding of the CS requirements and if some issues remain undefined by the time when ITER makes the Task agreement with the US PT on the fabrication of the CS, the detailed definitions of these issues shall be included in the scope of work for the US PT. So my task in supporting the CS specs is to keep updating it in accordance with progress in design and development.

Design and analysis of the butt joint tool. I finished thermal analysis of the butt joint in the process of the bonding and the analysis showed that we need an active cooling very close to the sealing point, otherwise an elastomer gasket gets damaged and we lose vacuum. The analysis also showed that we need about 1.5 kW of power to be transmitted to the joint by the inductive heater and then removed with the cooling block outside the vacuum vessel.
Design progress. During my stay in Naka I worked on design of several items.
1) We finished the butt joint design for the bus and for the butt joint in the coil. The old ITER design had a lot of features, too thick walls of the conduit, creating stress concentration, unprotected welds over the conduit, absence of jacket in areas where the

seal of the butt weld tool should be done against the jacket, etc. which made assembly or performance of the butt joint difficult or impossible.
2) I designed a ring to increase the stiffness of the attachment points on the CS brackets, according to recommendations by P. Titus.
After that I investigated the reasons for the excessive deflections of the flexible plates and came to conclusions that the problem comes from the bending moment, due to the significant distance between the force application point and the point of suspension.
I issued a memo about my findings. My study shows that the ring is not necessary. Besides, the ring in addition to cost implications, may have some problems with access to the TF preload ring.
I changed the design of the suspension and ITER designer Sato-san will be working on that after I return back. When he is done, P. Titus will run the ANSYS analysis to verify the design.
3) Bus supports. P. Titus analysis indicated that the bus bars experience high stresses due to some shear between the tie plates and the OD of the CS, where break outs of the leads take place. The problem is caused by low stiffness of the tie plates and strong deformation of them due to bus loads. In addition, buses need to have a freedom to slide vertically, since the coil height changes under electromagnetic load.
I designed the structure, which keeps the buses pressed against the OD of the CS module and which keeps the tie plates together with the system of "belts" and spacers, giving the structure necessary stiffness. That shall eliminate the problem. The concept of the supports is given to the ITER designer Sato-san. After he designs it into the 3D model, this model will be given to P. Titus for analysis.
4) I revised my memo about cooling of the structure with a modified approach, which increases the reliability of the cooling channels. I gave the necessary input to the designer.
5) I made a sketch of the CS wrap insulation with dimensions to be incorporated in ITER drawings.
6) I gave input to Yoshida-san and the designer to modify the wedges on the basis of my previous analysis. The wedges at the bottom are not strong enough to lift the dead weight of the stack and the wedges at the top do not have enough travel to preload the CS. Therefore wedges angle at the top will be doubled, while wedges at the bottom eliminated.
In the future, we need to concentrate on the remaining issues:

g) Inlet ports and vertical transitions at the ID – this task is also somewhat related to the inlets, since the inlets are located on the transitions. Needs to be designed to relieve vertical pressure on the inlets and reduced stresses at bends. The concept exists, but implementation needs work. I will focus on it in the nearest future.
h) Bus supports – need confirmatory analysis by P. Titus and then the situation will be reassessed. Hopefully already acceptably low stress.
i) Flex plates fixtures supporting the dead weight and axial EM forces I will analyze fasteners and forces of the CS and PF1, since their structures share the same bolts. Needs to make sure the bolts are not overloaded during the whole scenario.
j) Lower centering mechanism – too large potential movement under de-centering forces - access and design out.

Next visit will be preliminary end of May – end of June, but the funding is not secured yet.

The tasks to work on before and during the next visit will be:

4. Improvement of the CS design, resolving outstanding issues, working with ITER designers
5. Keeping the CS specs up to date

Itinerary

January 24, 2006	Departed from San Francisco
January 25, 2006	Arrived to Mito
January 26 –March 5, 2006	Working at Naka JWS
March 6, 2006	Returned to San Francisco

Contacts/Facilities Visited

4. N. Mitchell
5. Y. Takahashi
6. K. Yoshida

no facilities visited

Foreign Trip Report Distribution

W. Marton
N. Sauthoff
W. Meier
E, Synakowski
J. Minervini
T. Antaya
P. Michael
S. Milora
QA File

Attachment 3

LLNL

Memo

To: Neil Mitchell

From: Nicolai Martovetsky

CC: Joe Minervini, Phillip Michael, Denis Bessette, Yoshikazu Takahashi, V. Pantsyrnyi

Date: 10/19/05

Re: NbTi jc(B) : correlations and consequences

Executive summary

Correlation jc(B,T) for NbTi strands for can not be described by a universal correlation with fixed parameters valid for different strands. The requirements of jc(4.2K, 5T) =2900 A/mm2 does not guarantee jc(6.5K, 6T) =180 A/mm2, assumed by ITER 2001 design. The ITER 2001 correlation for NbTi was not checked carefully even against the LHC strand for all range of ITER operation. ITER operates at a very low current density in NbTi, where no LHC data was tried against the correlation, since it was not available. It is no surprise that other strands produced for PF R&D deviate from the ITER 2001 correlation. However, the deviations on the level of 0.2-0.3 K at the extremes (6 -6.4T, 6-6.5 K) are not very large to question robustness of the ITER PF coils.

If ITER receives strands with jc(5T, 4.2K) with the same temperature and field dependence as the PF Insert, for the PF coils 1-6, the temperature margins will be still above 1.5 K, for the PF 5 it will be 1.44 K, which seems to be acceptable. It is conceivable that other vendors will produce strands with slightly worse parameters and the margin can drop down to 1.4 K or so.

I do not see this slight deviation as very alarming and I would propose to do nothing at this point until we have qualification from the NbTi vendors and analyze results of the PF Insert test. Then we will assess the situation again.

The cryogenic system is designed to give lower inlet temperatures than assumed in DDD for the PF coils, which eliminates the problem. We need to make sure that the cryogenic system is not downscaled in overall optimization effort.

Background

ITER needs a correlation for jc(T,B) in NbTi for design and analysis. ITER have been using the latest available correlations (first by M. Green [1], then by M.F. Nishi [2], then by L. Bottura [3]) in the attempt to have a most accurate correlation for a possibly wider range of validity.

Since 2001 the DDD contains the NbTi jc (B,T) correlation obtained from the fit for the LHC accelerator magnets strand, which was proposed in [3] on the basis of measured jc(B) for 1.9 and 4.2 K.

D. Bessette recently collected the data on the commercial PF relevant NbTi strands, some of which produced by future ITER suppliers, and discovered that the ITER 2001 correlation gives better prediction than all of the recent strands by 0.2-0.3 K at 6 T at the operating current density of the PF 1&6 conductors (180 A/mm2). Therefore, there is a real prospects that the PF coils 1&6 will have margin less than expected by that amount of 0.2-0.3 K.

The question is what to do about it? The PF 1-6 specifications call for the jc (5T, 4.2 K)=2900 A/mm2. With Denis observations, it is clear that jc(6T,6K) may be slightly different than expected.

Let us follow the story and see what are the implications.

Jc(B,T) correlation background and comparison with experiment

Paper [3] shows a table (Table II) where the best fit parameters are given. In general, the proposed empirical correlation, based on observation of trends, is impressively accurate, allowing estimation of the jc(B,T) in most sensible range within several percent. Unfortunately, there is no comparison with other correlations to show that the proposed one is better, but it is likely so and ITER choice is probably justifiable. However, as it is clearly seen from the table, every strand requires its own parameters.

The proposed fits of the data in the paper [3] are within 10% of the measurements or better for most of the temperature and field range other than low fields, but also in a rare cases where the data available for higher fields and temperatures, we can see some larger deviations.

One can see that the range of fitting parameters is quite wide even for the same alloy composition. The LHC fit parameters are outside of the range of parameters found for the other strands quoted in [3]. Thus, the correlation [3] is a good tool for NbTi jc(B,T) correlation in general, but there is no credible way to predict the parameters of correlation for a particular strand.

Thus, as usual, we face a dilemma – we need to assume a correlation for jc(B,T) for the strand to carry out analysis, which would determine the requirements to jc(B,T), but we are not sure if we will get what we want, because we can not demand a particular correlation, just jc(5T, 4.2K).

Let us take the PF Insert strand which was supposedly representative to the PF coils performance (1 and 6). The measurements by Bochvar Institute were described pretty well by the Fietz [5] equation. R. Zanino found parameters for a good Bottura's correlation [3], but the parameters of the correlation are very far away from the parameters the ITER 2001 correlation uses.

Denis Bessette observed that ITER 2001 is better than any real relevant strands which he has the data about. This is no surprise, at least in retrospective.

Logically we can not even expect that if we take as ITER "official" correlation the jc(B,T) for the PFI strand, we are guaranteed from corrections in the future, when RF and China start delivering the strand, since we can not predict their correlation in advance.

What are the consequences?

Let's imagine that we received PF Insert jc(B,T) in the strands for the PF 1-6.

What would be the margins then and how does it compare to ITER 1998 and ITER 2001 expectations?

Table 1 shows the margins for the nominal and the back up modes for different correlation for the PF Insert strand: by Bochvar, using Fietz correlation, ITER 2001 (the current one) correlation, Zanino correlation for the PF Insert, based on Bottura correlation and the former ITER correlation used in 1998. Note, that the PF Insert strand gives 2800 A/mm2 in 5 T, at 4.2 K, not 2900 A/mm2, as ITER requires. And for these calculations I kept Cu:nonCu as for the PF Insert strand, 1.41 as opposed to 1.6 for ITER PF 1&6, which I will correct later. This small amount of copper change (less than 5%) may help to make up Ic if necessary, without big penalty in hot spot temperature.

As we can see, all the strands give more than 1.5 K temperature margin, although admmittingly, ITER correlation is higher by 0.2-0.28 K.

Table 1. Temperature margins in PF1&6 for different correlations, see text for explanations of assumptions.

PFInsert for 1&6 conductors, nominal		Bochvar	ITER	Zanino	ITER 98
I op per strand, A	31.25				
Bpeak, T	6.00				
Operating temperature, K	4.70				
Tcs (6T, 31.25A), K		6.30	6.51	6.24	6.52
Temp margins, K		1.60	1.81	1.54	1.82

PFInsert for 1&6 conductors, back up		Bochvar	ITER 01	Zanino	ITER 98
I op per strand, A	36.11				
Bpeak, T	6.40				
Operating temperature, K	4.40				
Tcs (6.4T, 36.11A), K		6.07	6.28	6.00	6.29
Temp margins, K		1.67	1.88	1.60	1.89

If we assume that ITER receives a strand which meets the requirement of jc(5T, 4.2 K)=2900 A/mm2 and Cu:nonCu=1.6, but the jc(B,T) scalable to the PF Insert, the margins will be like shown in Table 2.

Table 2. Temperature margins in PF1&6 for different correlations.

PF 1&6 conductors, nominal		Bochvar	ITER	Zanino	ITER 98
I op per strand, A	31.25				
Bpeak, T	6.00				
Operating temperature, K	4.70				
Tcs (6T, 31.25A), K		6.31	6.49	6.22	6.50
Temp margins, K		1.61	1.79	1.52	1.80

PFInsert for 1&6 conductors, back up		Bochvar	ITER 01	Zanino	ITER 98
I op per strand, A	36.11				
Bpeak, T	6.40				
Operating temperature, K	4.40				
Tcs (6.4T, 36.11A), K		6.07	6.26	5.99	6.29
Temp margins, K		1.67	1.86	1.59	1.89

As we see the coils 1&6 are within allowables. We see that the margins did not change much for *a la* PF Insert strand in comparison with the PF Insert strand, since growth in jc(5,4.2) compensated by slight reduction of the NbTi cross section.

Fig. 1 shows different correlation in the vicinity of the 6 T, 6 K operation. As we can see at low currents the difference is about 0.2-0.3 K.

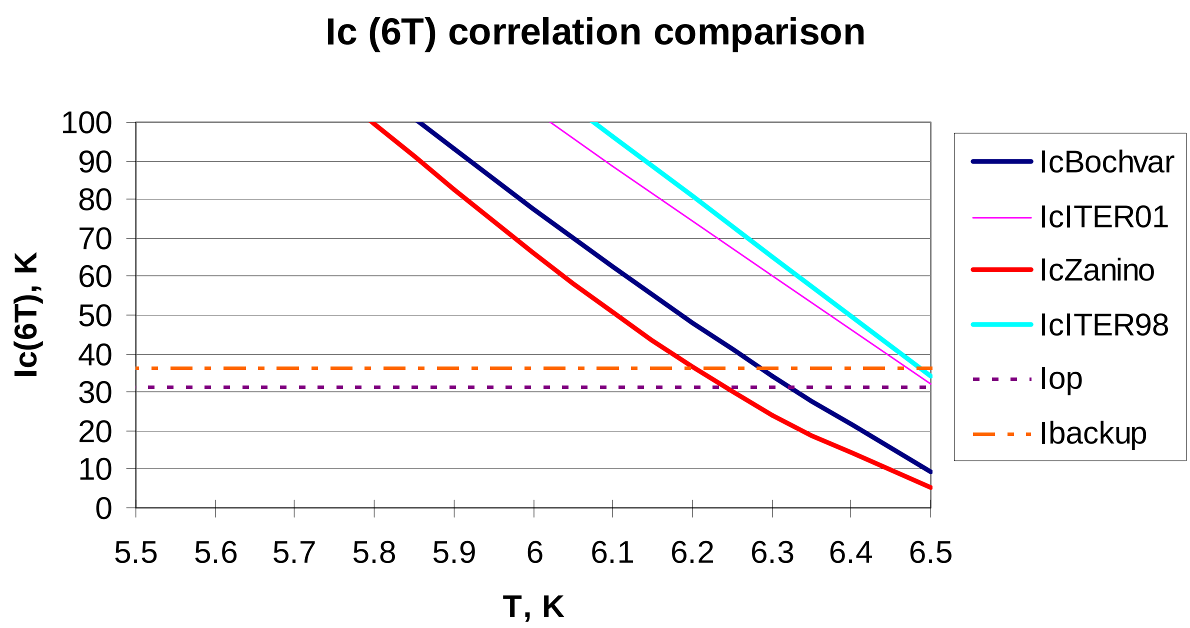


Fig. 1. jc(T) correlations for NbTi at 6 T

In Denis memo [4], it seems like he assumes that the inlet temperature will be 5.0 K for all the PF coils. I used DDD numbers that says that the PF 1-6 will have 4.7 K inlet in nominal regime and 4.4 K in the back up operation. V. Kalinin told me that the plant is designed to provide 4.3 K for all the coils, so 4.7 K assumption has some margin.

Table 3 shows the margins in the PF 5 coil. It does have 5 K inlet in the nominal mode (I need to find out why 5 k?)

Table 3. Margins in PF 5.

PF 5 conductors,nominal		Bochvar	ITER 01	Zanino	ITER 98
I op per strand, A	41.67				
Bpeak, T	5.00				
Operating temperature, K	5.00				
Tcs (5T, 41.67A), K		6.43	6.51	6.34	6.61
Temp margins, K		1.43	1.51	1.34	1.61

As we can see the PF5 does have some shortage in the desired margin - 0.07 K according to Bochvar measurements. Zanino's correlation predicts 0.16 K, but Bochvar test points in 5 T are a little higher than the Zanino's correlation, so dT=1.43 K is more credible.

This deficiency does not look very alarming, especially taking into account that 5 K inlet is very conservative, although formally we should be prepared to see that the margin will be slightly less than we expected using ITER correlations.

Possible mitigation

At the moment no qualification strands is made yet, but it is underway in Russia and China.

There are many possible alternatives to gain back the deficiency in temperature margin due to the real jc(B,T) will most likely be a little worse than ITER 2001 correlation.

1) Reduce PF operating temperature. In the current ITER cryogenic system design [6], the inlet temperature in the nominal mode is 4.3 K for all the P coils. The DDD gives 5 K for PF 2-5 and 4.7 K for the RF 1&6. Thus, the current design has enough margin to meet the criteria. The inlet for all PF coils should be assumed 4.7 K as a maximum.

2) Increase amount of superconductor, by reducing some amount of copper.

3) Increasing amount of superconductor and maintain amount of copper the same, which makes the cable a little bigger. The PF conduit cross section may be reduced a little or winding pack will grow by some small amount.

4) Specify jc (6T, whatever T). A theoretical possibility is to specify the NbTi properties at 6 T, 6.2 K does not seem very practical, there are not many places in the world to measure that and it is more expensive than the standard measurements at 4.2 K.

5) Changing the slope of the jc(B,T) in the NbTi is beyond state-of-the art. However, Bochvar Institute team plans to attempt optimization of the fabrication parameters to achieve higher jc at elevated temperatures and fields [7]. They may figure out how to improve the performance in higher field and temperatures, which might be beneficial for ITER and we should encourage such work. However if that will not be achieved the ITER PF coils are not in danger.

Summary

Although some design change in the future is possible to restore the expected Tcs at operating current, it does not seem at the moment that any corrections in the PF conductor design are necessary. The ITER 2001 correlation was chosen on a basis of very limited data and turned out to be a little more optimistic than the strands for ITER PF on the R&D stage, but the penalty is small. The cryogenic system has inlet temperatures lower than assumed in the DDD, which compensates with a margin the lower Tcs of real strands, than predicted by ITER 2001 correlation.

It seems premature to change anything in the design until qualification batches of NbTi strands will be produced and characterized by RF, China and other suppliers and the PF Insert testing completed. The PF Insert test will give valuable data on how much margin the CICC has in comparison with the prediction based on the strand characteristics and hopefully will verify the design.

For the design purposes it is possible to continue using the ITER 2001 correlation, realizing that it will be a little optimistic. An alternative – like using the PF Insert strands scaling assuming jc(B,T)=2900 A/mm2 will give probably a better expectations, but there is also no guarantee that the final strands for the PF coils will be exactly like that. However the penalty for slightly inaccurate assumptions is relatively small.

References

[1] M. Green, Calculation of the jc, B, T surface for niobium titanium using reduced-state model, IEEE Trans. Mag, vol 25(2), p. 2119, 1989.

[2] M.F. Nishi, private communication

[3] L. Bottura, A practical fit for the critical surface of NbTi, IEEE Trans, Appl. Sup., v.10, No. 1, p.1054

[4] D. Bessette, memo, unpublished

[5] W.A. Fietz and W.W. Webb, Magnetic properties of some type II alloys superconductors near the upper critical field,", Phys.Rev., vol. 178, No. 2, p. 42, 1967.

[6] V. Kalinin, private communication

[7] V. Pantsyrnyi, private communication

Attachment 4

LLNL

Memo

To: Peter Titus, Phil Michael

From: Nicolai Martovetsky

CC: Neil Mitchell

Date: 11/10/05

Re: Forces on the buses in the ITER CS

Executive summary

Buses of the CS modules experience large electromagnetic forces. P. Titus analysis showed that these forces create large deflections in the tie plates, which support them. He assumed that the tie plates must support some radial forces arising in the circumferential portion of the bus in addition to other forces. This assumption is extremely conservative, since these forces are reacted by the dummy conductor belt. In addition, most of these forces are directed towards the coil, thus they do not act on the tie plates at all. This memo analyses the direction and amplitude of the forces on the circumferential run of the conductor, where it breaks out from the coil. This fact may reduce significantly deflections on the tie plates and thus reduce or even eliminate the problem of the bus supports.

Background

Bus support is a non trivial thing due to large forces and the fact that the structure supporting it is more convenient to attach to the tie plates. The alternative is to attach it to the coil, but in this case it would require a kind of belts around the module. It is possible, but not considered so far for economic reasons. The buses experience large electromagnetic forces. The tie plates deflect under these forces. These deflections may cause large stresses in buses if not designed properly. A recent analysis by P. Titus [1] indicated that these deflections are responsible for unacceptably high stresses in the buses.

Phil Michael [2] also analyzed forces on the buses and showed that they can be significant, since in different plasma scenarios, there is a radial component of the magnetic field which generates a large circumferential force. In addition there are some vertical component and circumferential component of the force on the radial and circumferential runs.

In my previous memo [3] I showed that if the dummy belt on the last turn is holding the forces effectively, the rest of the forces are directed circumferentially, there is no component of pure radial force. For the runs where the buses are side by side the forces cancel each other, for the runs where there is only one bus, the uncompensated run may create problems.

I assumed that the moment of inertia of the tie plates is very large and these forces, even substantial (10-15 t) should not generate much of a displacement, but Peter pointed out that the circumferential force due to misalignment with the center of tie plate will have some deflection much bigger than one would obtain assuming bending around the neutral axis of the tie plate hard way.

Unfortunately, the deflection of the tie plates is more complicated than to be expressed by a simple formula, the problem is 3 D and all we know about deflection of the plates is Peter's result where both radial and tangent forces were present, so how bad is the situation where there are no pure radial forces we do not know at the moment.

In my memo [3] I pointed out that in many situations the radial force even directed inward, which means that it is reacted by the OD of the coil, not by the tie plates, let alone the dummy conductor belt. Peter in his memo said that the sign does not matter as long as tie plates carry the load, inward or outward it is the same bending. Obviously this is incorrect, even if the dummy belt is ignored, a force inward can not be transferred to the tie plate.

In this memo I looked at several points of the scenario and show that in all cases forces on the break outs in the CS1U, CS1L, CS2U, CS2L are inward. Radial forces in CS3U and L are sometimes outward, but relatively small, will be reacted by dummy belts and are close to the tie plates attachment points, which does not allow large deflections. This elimination of radial forces from the circumferential runs of the buses from the model will improve the situation with the bus stresses, but to what extent needs to be found out from a modified ANSYS model.

Forces on the terminating turns

I ran several cases, which showed a noticeable deflections in Peter's analysis, hoping that takes care of all extreme possibilities that bus bars see in operation.

I modeled plasma by a 1x1 m^2 cross section with the center of the cross section coinciding with the plasma center as given in TDD.

Coordinates of the PF and CS modules and their currents were also taken from the TDD.

I computed the magnetic fields at the points: SOF, SOB, EOB, EOC, EOP and SOD.

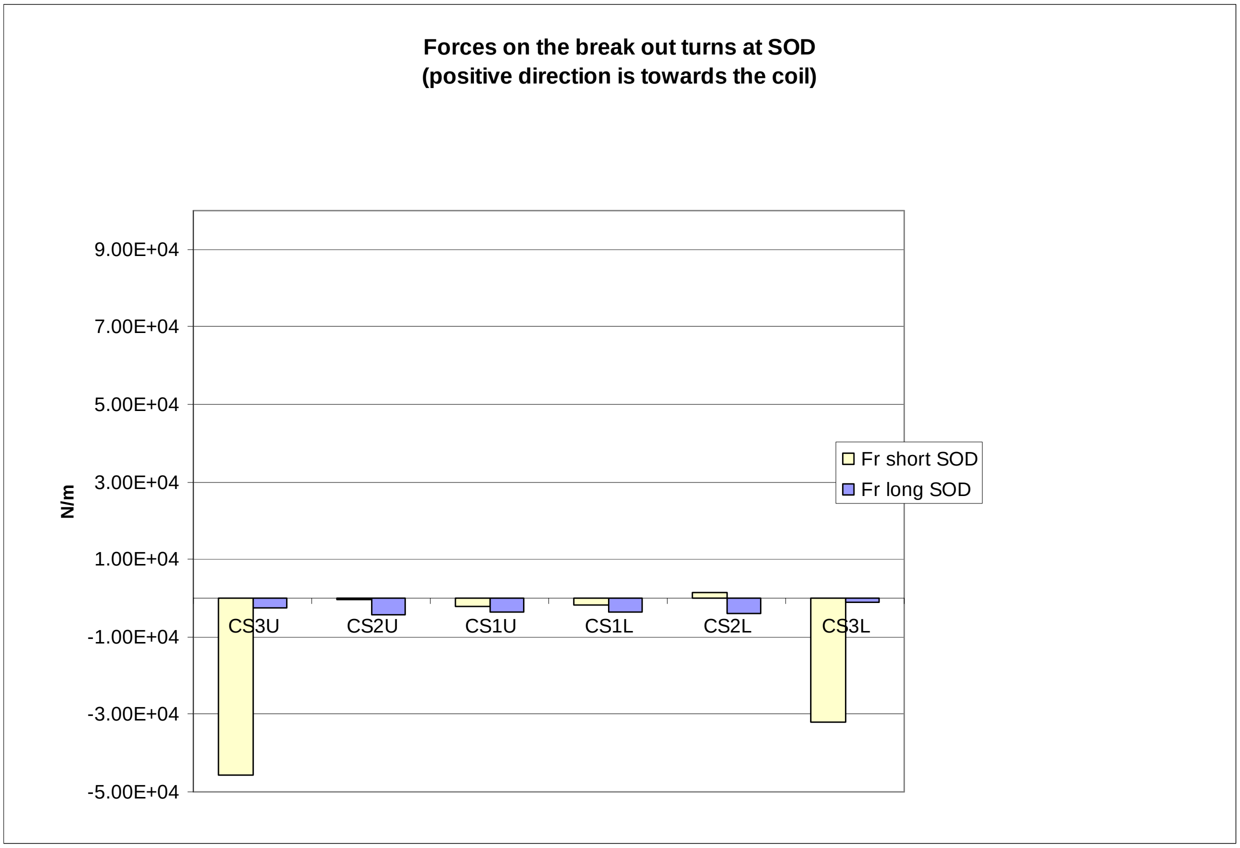
I assigned the radius 2.13 m to the location of the last toroidal run (i.e. circumferential run of the break out), which corresponds to the radius of the vertical run of the bus bar. Location of the bus break outs are given in Table 1 and Z coordinates are taken from [2]. My field tables had a 6.6 cm steps, I simply took the nearest node, so the point of the field taken for force calculations was closer than 33 mm to the Z coordinate given in Table 1.

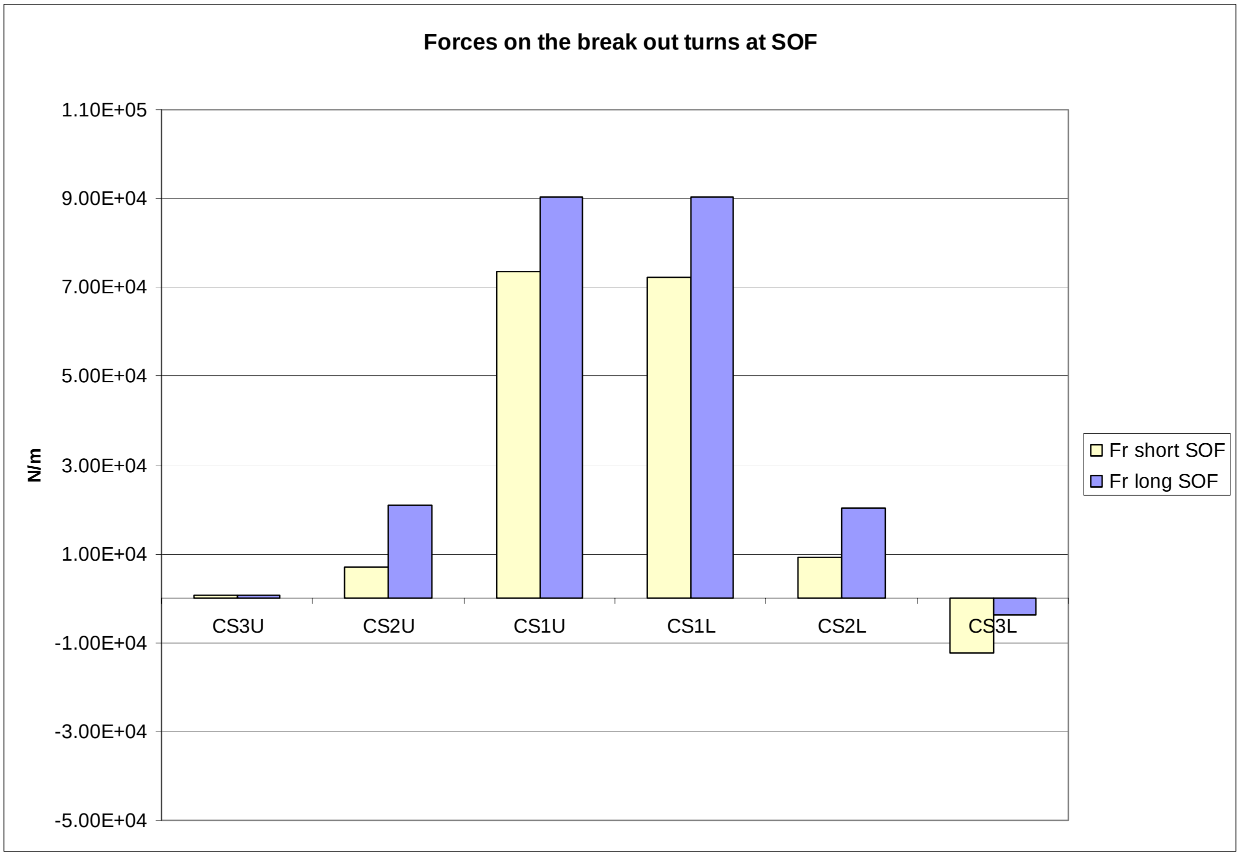
Table 1. Location of the break outs from the CS modules

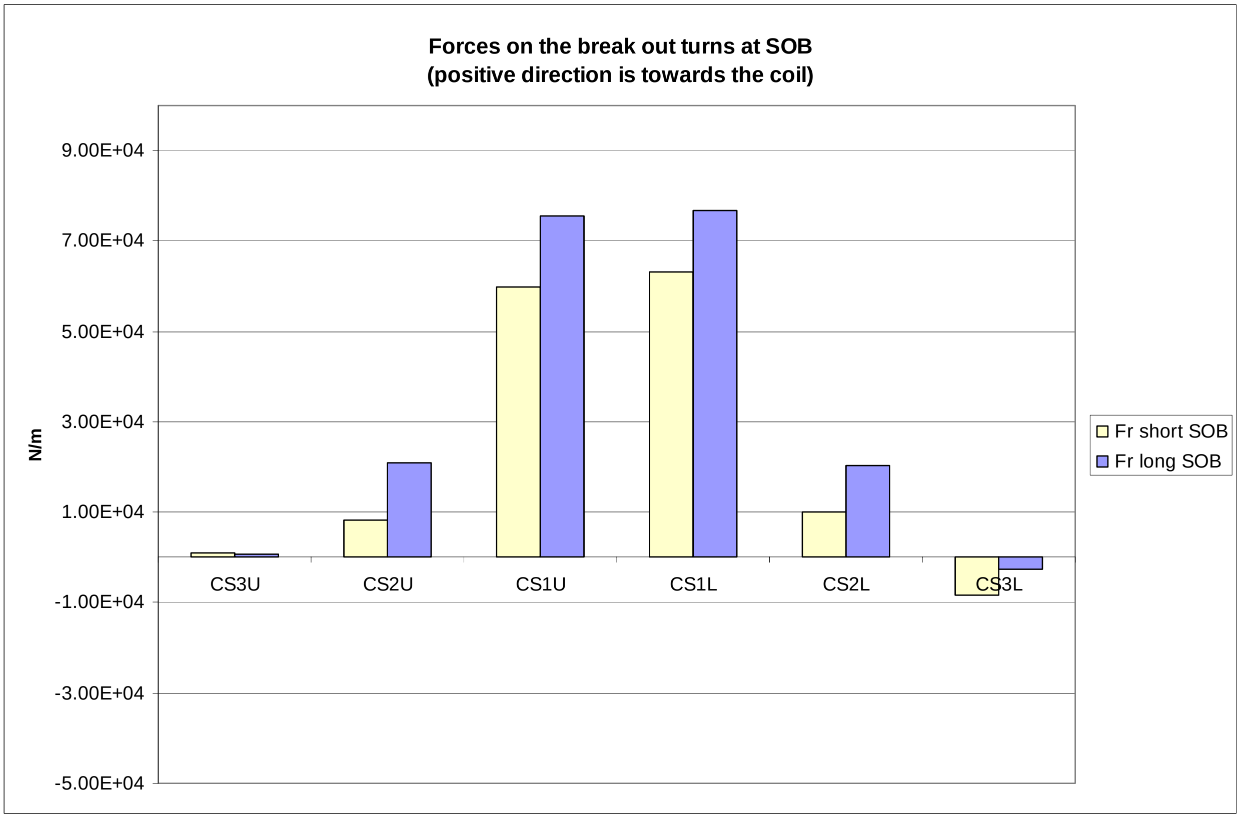
	Long lead Z	Short lead Z, m
CS1U	0.051	2.073
CS2U	2.174	4.196
CS3U	4.298	6.32
CS1L	-0.051	-2.073
CS2L	-2.174	-4.196
CS3L	-4.298	-6.32

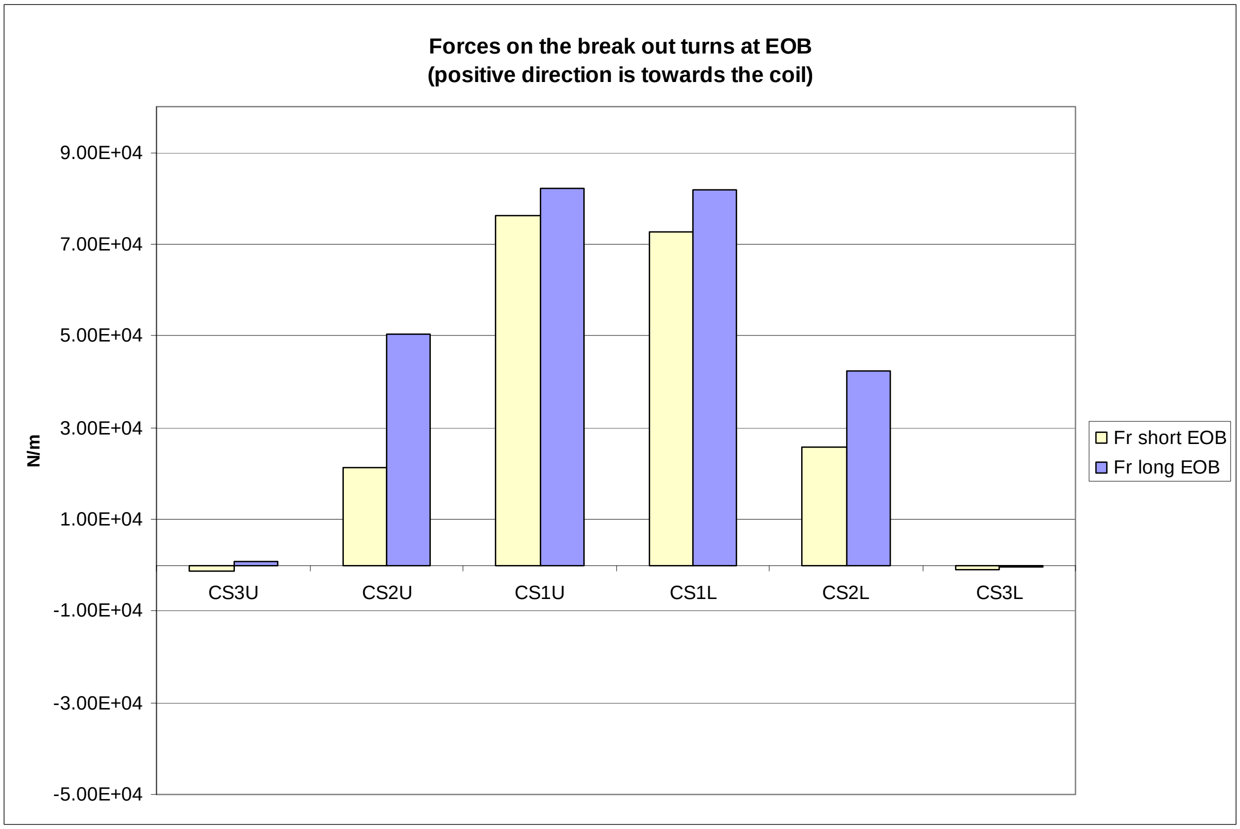
Fig. 1-6 shows forces generated in the break out circumferential run.

The positive sign of the force corresponds to the direction towards the coil, pressing to the OD. The negative sign is the radially outward. It sounds a little confusing, but that definition of signs comes from ITER definitions : the CS is wound clockwise if looked at the winding from the top and the positive current generates field directed downward, so the positive product of IxBz is directed radially inward, toward the coil center and reacted primarily through the break out bracket by the OD of the coil.


Forces on the break out turns at SOD
(positive direction is towards the coil)
9.00E+04
7.00E+04
5.00E+04
3.00E+04
1.00E+04
-1.00E+04
-3.00E+04
-5.00E+04
N/m
CS3U
CS2U
CS1U
CS1L
CS2L
CS3L
Fr short SOD
Fr long SOD


Forces on the break out turns at SOF
1.10E+05
9.00E+04
7.00E+04
5.00E+04
3.00E+04
1.00E+04
-1.00E+04
-3.00E+04
-5.00E+04
N/m
CS3U
CS2U
CS1U
CS1L
CS2L
CS3L
Fr short SOF
Fr long SOF


Forces on the break out turns at SOB
(positive direction is towards the coil)
9.00E+04
7.00E+04
5.00E+04
3.00E+04
1.00E+04
-1.00E+04
-3.00E+04
-5.00E+04
N/m
CS3U
CS2U
CS1U
CS1L
CS2L
CS3L
Fr short SOB
Fr long SOB


Forces on the break out turns at EOB
(positive direction is towards the coil)
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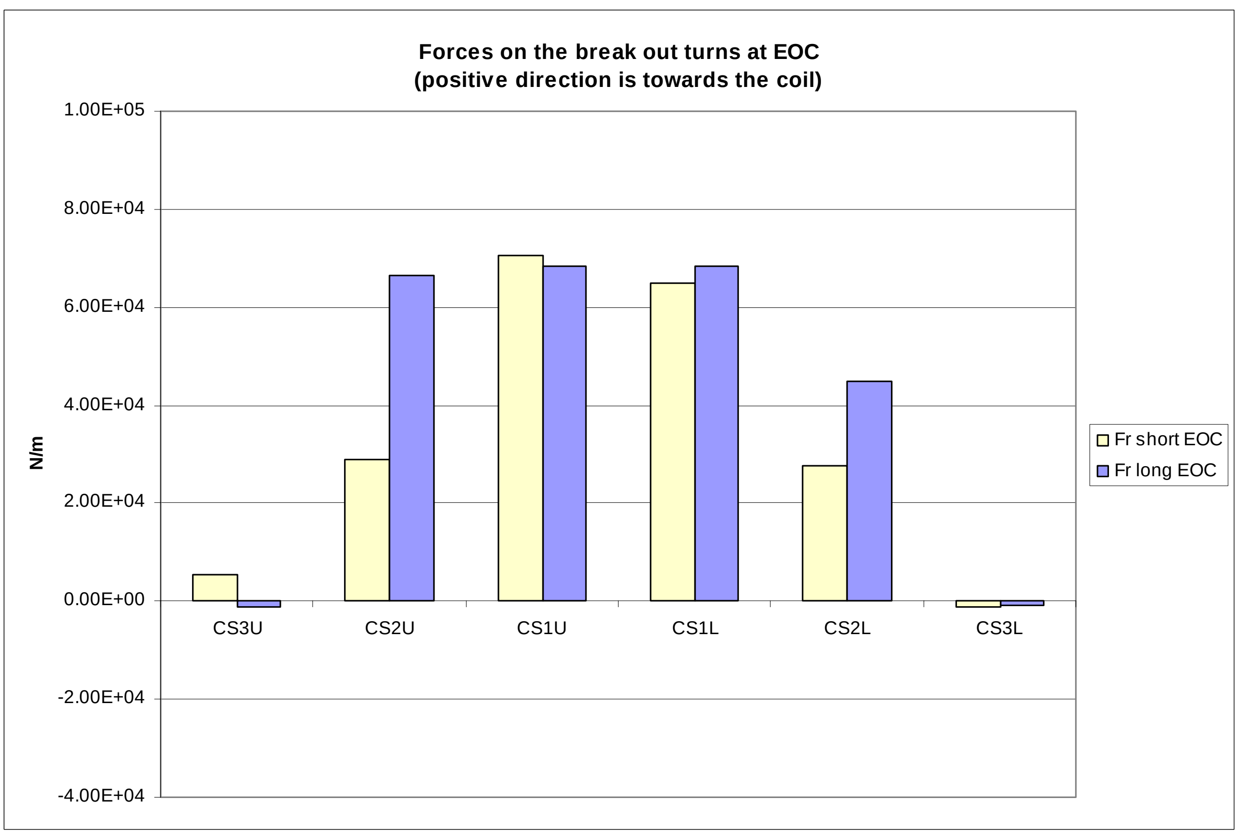
7.00E+04
5.00E+04
3.00E+04
1.00E+04
-1.00E+04
-3.00E+04
-5.00E+04
N/m
CS3U
CS2U
CS1U
CS1L
CS2L
CS3L
Fr short EOB
Fr long EOB


Forces on the break out turns at EOC
(positive direction is towards the coil)
1.00E+05
8.00E+04
6.00E+04
4.00E+04
2.00E+04
0.00E+00
-2.00E+04
-4.00E+04
N/m
CS3U
CS2U
CS1U
CS1L
CS2L
CS3L
Fr short EOC
Fr long EOC

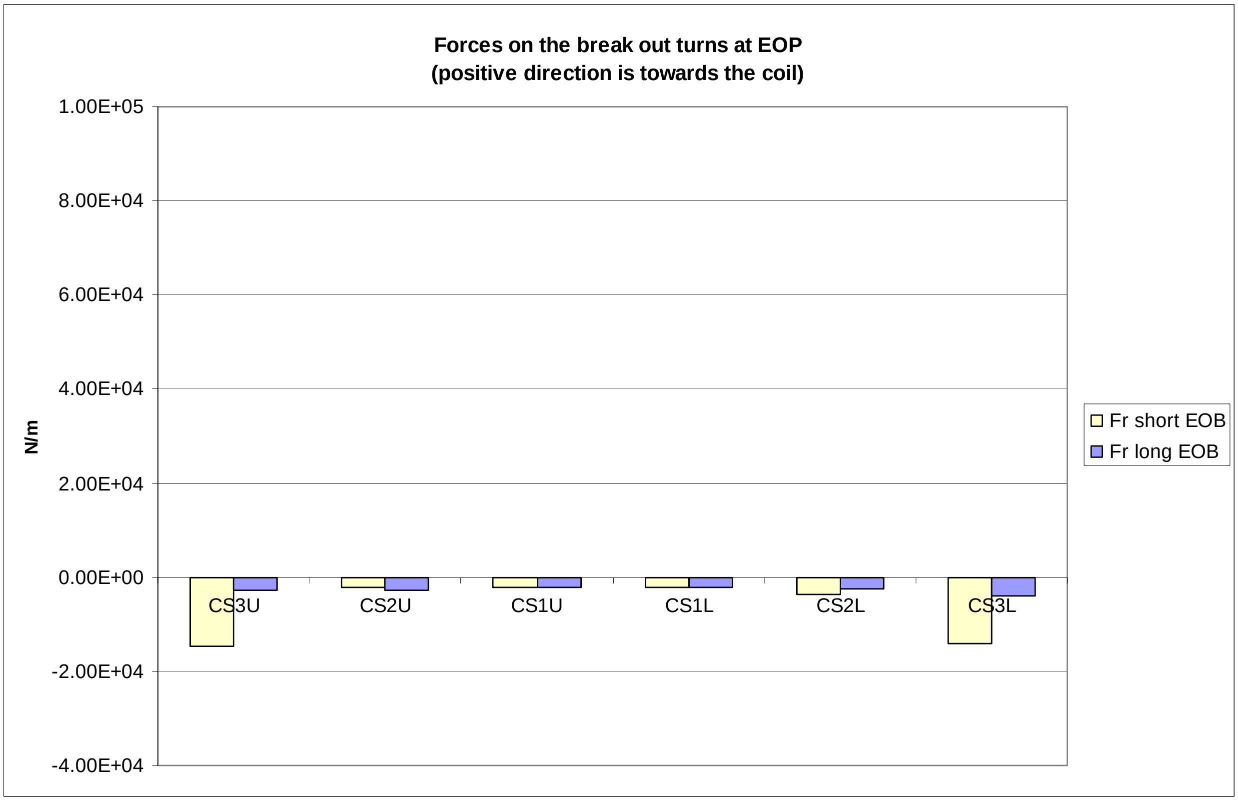


Fig. 1-6. Forces on the outer turn (break out) in different plasma scenarios. SOD and IM are the same conditions.

As we can see, in all the load cases the radial forces are directed either toward the coil or insignificant or, in some rare cases applied to the break outs located near the buffer zones, where tie plates are not that flexible and those forces do not cause much of deflection. Thus, the model for the tie plates and bus deflections should be modified, the radial forces from the break outs shall be eliminated since they are directed either toward the coil or held by the dummy conductor belts and can not be a source for tie plate deflections.

Comparison with selected runs by Phil Michael

I compared my forces with the ones calculated by P. Michael [2]. Fig. 7 shows the results. There is some difference, about 10% at high forces, and in rare cases even the sign was different, when the field is low. I define position with accuracy of 33 mm, just to make it quicker. When I calculated it exactly at the points Phil did, the results did not change much, I checked several points, not all for the SOF for example. I tried to improve the accuracy to the maximum size of the mesh and field error better than 0.2% in OPERA terms, it did not change things much.

May be Pillsbury's code that Phil used has a difference with OPERA 2D, but for the sake of structural issues the difference of 10% is not important. So the conclusion is: my forces are credible, but the accuracy of the field calculation is in question. I'd normally trust Bio-Savard integration from solenoids by elliptical integrals better than a FEM solution on a large mesh, but OPERA is too reputable to expect large errors like 10% or so. We need to check the models and assumptions with Phil.

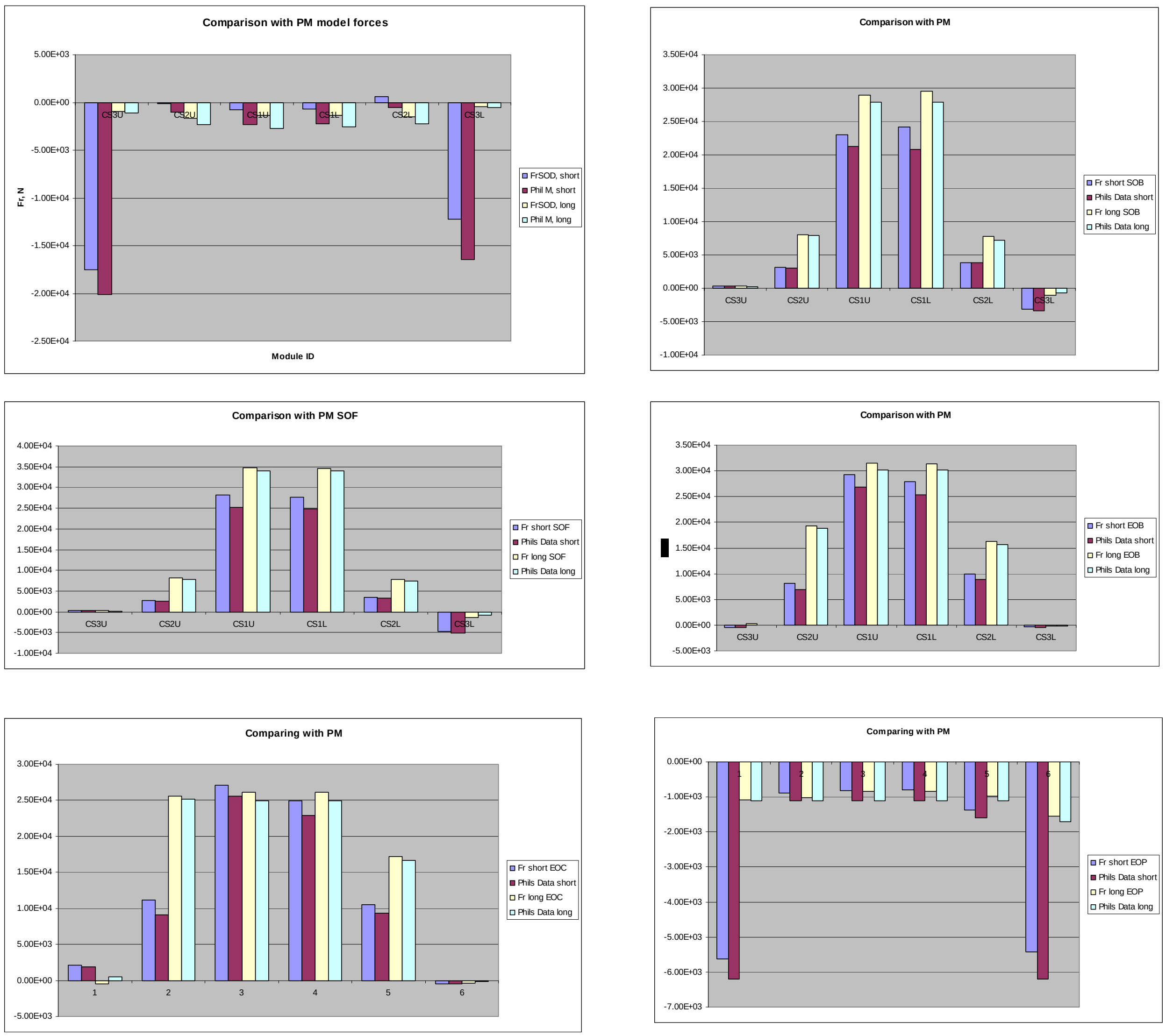


Fig. 7 Comparison my radial force calculation with P. Michael [2]

Trying to understand P. Titus analysis

Peter in his memo shows deflections of the tie plates is the major reason for high stresses in the buses. Table (unnumbered) on page 5 shows straps deflections. The largest deflection in the table is at the SOF – 20.1 mm (not clear to me relative to room temperature position or at LHe before charging, judging by small deflection at IM is probably the latter).

On the second thought Peter says that vertically compliant strap deflects 12 mm – that is shown in Fig. 3, and it shows only 4 mm relative motion, 8 mm comes from the cooldown. Total confusion about how much deflection from 20 mm comes from loading (stressing the bus) and how much comes from cooling and what is the resulting motion of the strap relative to the OD of the coil when deflection is 20 mm. Would be nice to know what toroidal compliant length is as opposed to vertical compliant length.

Unfortunately at this point, we have no results of how much deflections are caused by radial forces alone in the Peter's model (which as I showed above should be removed from the model) and how much they are caused by the circumferential (toroidal) forces acting on the unpaired bus run.

Let's assume that Peter's "strap us" in the last column of his table (Fig. 8) means net deflection of the strap due to EM loads, no cooling is involved. That disconnects it from the Fig. 23, where the radial displacement are shown too but different numbers (what is the relation between the table and the Fig. 23, both showing presumable the same thing, but different values)?

Another assumption – EOD in the Peter's table does not have such a thing in the DDD. I assume it is EOC, can't be EOP, since at EOP there are no forces on the buses, neither toroidal nor radial and Peter's EOD deflection is very significant, 14.7 mm, must be some forces there, like at EOC.

Lead and Insulation Analysis Results – With Toroidal Compliant Length - No Strap interconnection

Ld Step	Dat Set	Time Point	Lead SIGE MPa	Lead Insulation SIGE	Strap SIGE	Strap uz
1	Ble1	IM		9.26	15.9	.000731
2	Ble9	XPF		20.6	52.2	.0009
3	Ble16	SOF	1670	74.9	59.6	.0201
4	Ble22	SOB		62.6	46.9	.0163
5	Ble23	EOB		67.3	60.8	.0174
6	Ble27	EOD		61.8	56.3	.0147

Fig. 8. Load cases from [1]

The radial loads (which I argue should be removed from the model of tie plates bending) are shown in Fig. 1-6 and 7, the circumferential loads on the buses are given in [2]. From comparison of loads and deflections one can see the following.

The toroidal loads and radial loads applied to the top and bottom of the plates do not cause much of a deflection in the tie plates (compare IM and XPF), small deflections at significant forces on CS3U and L, both toroidal and radial.

That is because the forces are applied too close to the attachment points to cause large deflections. The rest of the cases are difficult to analyze just looking at the table on the subject – if radial forces are removed, whether there still be large deflections requiring mitigation.

Table 4: Electromagnetic loads (in kN) on un-paired feeder-extensions at eight time points during plasma scenario.

Module	IM	XPF	SOF	SOB	EOB	EOC	EOP	EOB + PD
CS3U	-126	-11	75	78	118	135	52	122
CS2U	-14	16	-49	-49	-126	-191	-0	-120
CS1U	-1	-1	-83	-63	-47	-19	0	-46
CS1L	1	1	91	71	63	50	0	64
CS2L	16	-27	71	66	142	153	4	135
CS3L	135	9	-104	-106	-150	-151	-51	-154

Fig. 9 Loads from [2].

Summary

Radial forces in the model Peter used, are possibly the main reason that gives large deflections and create problems in the buses. These radial forces should be removed from the model and the situation needs to be reassessed. If deflections still remain large, we should introduce stiffeners to reduce bending. Two alternatives are possible – either belts and spacers which would make the tie plate structure stiff or keys secured to the dummy belts which would prevent bending and rotation of the tie plates. Both alternatives have a variety of conceivable designs and an optimum can be found when we know the extent of the problem if it is still there after radial forces are removed.

References

[1] P. Titus, Memo “Coil to strap connection – strap stresses and displacements”, Oct 17, 2005

[2] P. Michael, Memo , Current stick model for electromagnetic loads on CS current-feeder-extensions, **Reference #:** ITER/US/MIT/PMichael/062805-1

[3] N. Martovetsky Support of the bus bars, Memo 04/28/05

Attachment 5

LLNL

Memo

To: Peter Titus, Phil Michael, Neil Mitchell, Kyoshi Yoshida

From: Nicolai Martovetsky

CC: Joe Minervini, Timothy Antaya

Date: 12/05/05

Re: Preload mechanism analysis

Executive summary

The preloading mechanism of the CS structure needs to be modified to function properly. Lower wedge is difficult to use, as currently foreseen, the hole for the pulling bolt should be modified into slot, upper wedge needs to have a bigger angle to provide more vertical travel.

Background

Preloading structure of the CS is necessary to avoid modules separation, limit axial motion of the modules and, to some extent, reduce the tensile stresses in the coils. This structure also holds the CS assembly together in operation, since at some points the CS assembly tends to separate.

Fig. 1 and Fig. 2 shows details of the preload mechanism. Current mechanism has a set of the tie plates inside and outside of the CS and two sets of wedges. Identical wedges are located at the top and the bottom of the CS assembly. To preload the CS assembly the tie plates are heated to some temperature (TBD), which cause their elongation and opens a gap. After the gap opens, the wedge moves inward to close it. After the cooldown the tie plates shrink and the CS gets the preload. Some more is gained during the cooldown 30-50% of what is obtained at RT .

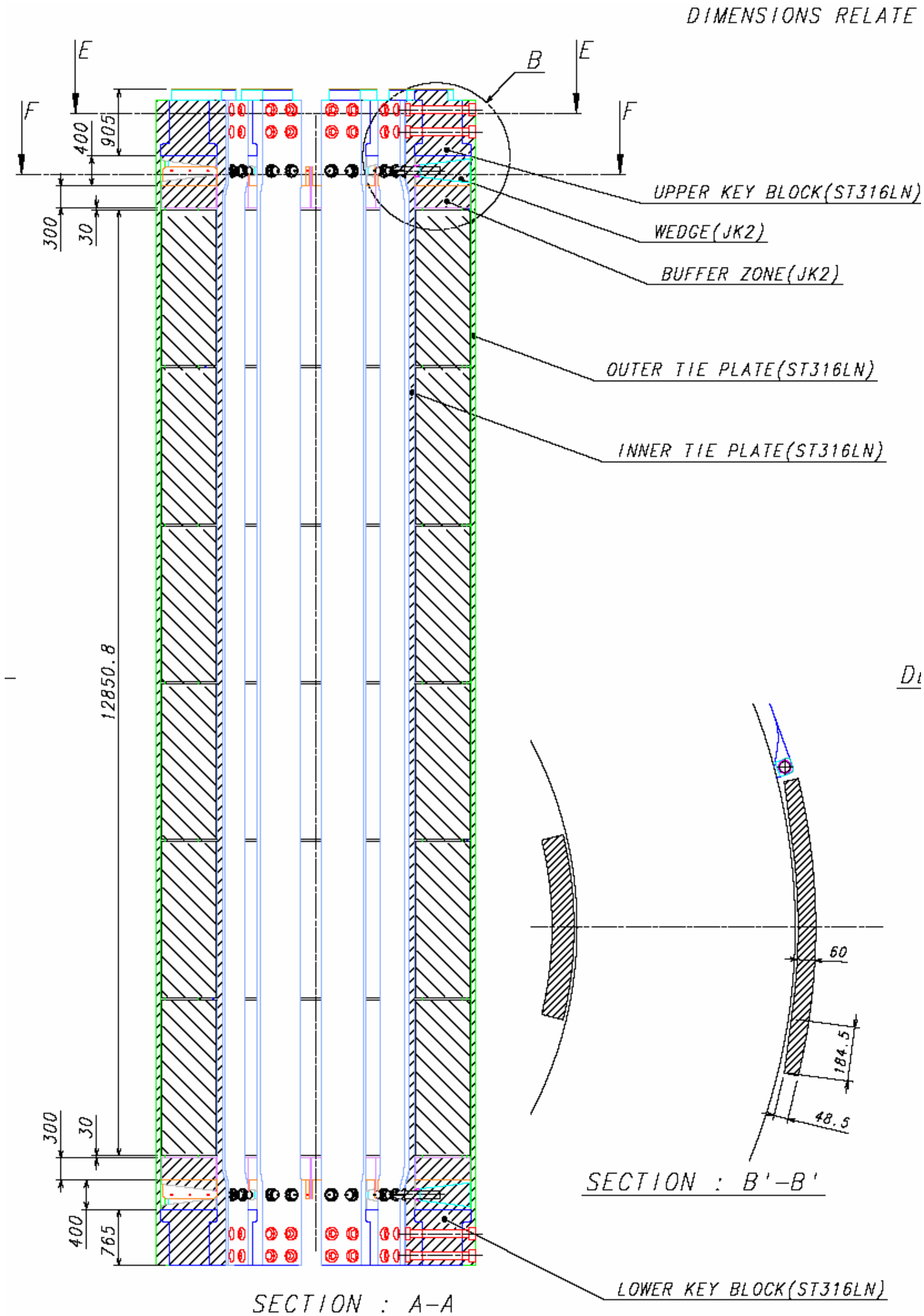


Fig. 1. Prestress structure

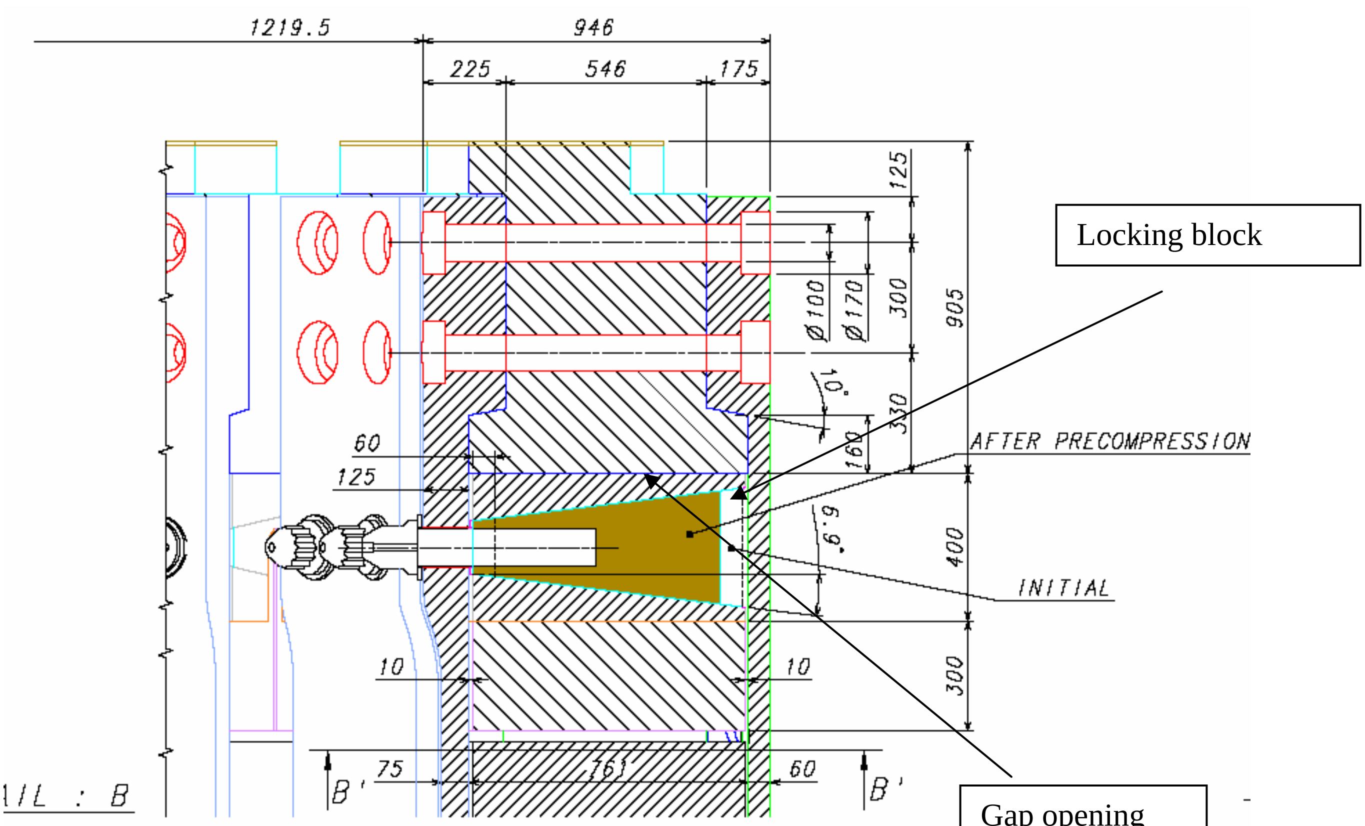


Fig. 2. Design of the wedge assembly detail

Requirements

Quite a few analyses done on the preload [P.Titus, Mr. Fu, P. Michael 1-3] show that the insulation slight tension in the global model is not very sensitive to the preload amount and say, 5 MPa is not much worse than 50 MPa preload (my interpretation of Mr. Fu 150 C dT tie plate heating [2]). Mr. Fu report [2] shows that the tensile stresses in insulation vary somewhere between 2.5 and 4.5 MPa at worst. It is not worth paying for higher prestress.

At about 70 C (which translates into 20 MPa preload) there will be gaps between the outmost modules and the rest of the assembly at some point of the scenario. Since all interfaces have keys and can not slide relative to each other, there is no reason to worry about it.

This issue remains under discussion with a clear preference toward lower prestress. So, there is no firm requirement on the amount of prestress.

But we need to know how much gap we need to fill to design our system.

Let's assume that the maximum prestress we would ever want to see is 26 MPa, which would correspond to 0.1% strain in the tie plates.

Also, we need to take into account some possible misalignment or out of flatness between the coils. These gaps provide some sponginess of the structure and will require additional travel. If we would have a hydraulic stud tensioner or something that applies force with unlimited travel, that would be relatively unimportant. In present plan, all necessary travel of tie plates occurs only as a result of heating of the tie plates.

On the one hand, the weight of the modules (100 t each roughly) will give some preload to possibly take up some slack, but on the other hand, the final load of 15000 t is so much higher than the dead weight, that the dead weight load may be insufficient for that. So, conservatively, we assume 2 mm per interface should be additional travel to take up the slack, which combined gives 10 mm per 5 interfaces of 6 modules in the CS (assume buffer zone compliance negligibly small).

Table 1 below shows required gaps and elongations for the preload mechanism.

As we can see, there is not enough travel available by the wedge move of 60 mm horizontally at its current double sided angle of 6.9 degrees on each side.

What options are available for us?

First, when the gap opens up, we can insert (theoretically) some shims in the gap and then use the wedge to apply additional prestress. But the current design prevents to insert any shims near the wedge surface due to the lips, shown in Fig. 4.

Also in the current design, the wedge can not function very well because the hole for the pulling bolt is round and has no freedom to move vertically, adjusting to the gap, even rotation is questionable. With such an arrangement, it will not work. The wedge is installed before heating in the right outmost position, no gaps. When the heating starts, the tie plates will elongate and a gap will open somewhere below the wedge, no gap appears above the wedge, it will be hanging on the bolt, the wedge slider will separate. As we start torque the bolt pulling the wedge in, the wedge will remain in contact with the upper insert wedge and will attempt to rotate around the hole, it will not give a full surface contact with both wedges it slides against. To fix the problem, the hole in the inner tie plate for the pulling bolt must be a slot allowing the bolt and the wedge vertical adjustment with a travel of 14.5 mm (make it say 25mm with margin) down from the uppermost position of the wedge. Because when the M100 bolt pulls the wedge it wants to lower the horizontal coordinate of the bolt.

The same is true for the bottom wedge if it is ever employed, again a slot shall allow 14.5 mm vertical motion with some margin, say 25 mm.

Table 1. Required travel of the tie plates

Tie plates lengh	14.25	m
Coil length	12.85	m
CTE	1.60E-05	m/mK
Strain in tie plates	0.10%	
Tie plates elongation	0.01285	m
Young modulus plates	2.07E+11	P
Young modulus coil	4.70E+10	Pa
Vertical stress in the winding pack	2.46E+07	Pa
Strain in coil	5.23E-04	m
Coil deflection	6.72E-03	m
Total deflection	1.96E-02	m
Gaps between modules and buffer zones (2 mm per interface)	1.00E-02	m
Total travel needed	2.96E-02	m
One wedge vertical travel	1.45E-02	m

So, let us suppose that we fixed the hole for the pulling bolt, turning it into a slot. Now, let's come back to the idea of inserting the shims in the gaps when the tie plates are heated and using upper wedges to eliminate the fabrication tolerances slack to increase capability of the wedge mechanism to preload. Again, the purpose of this is to see if we can still preload the CS with the existing design and seemingly inadequate vertical travel of the wedge.

The sequence of operations for the preload could be the following.

1) We assume that the preload structure is assembled without intentional gaps.

2) Before heating, we can use the wedges to see if there are some tolerance gaps which need to be filled. For that we'd torque the upper wedges to the designed torque and measure the torque versus travel. In the beginning we expect the stiffness of the CS assembly to be low and then increase as the gaps closes. The vertical travel, which takes up the gaps and low modulus preload we call slack and the amount of shimming to be done after the heating is decided on this stage.

3) After that the tie plates are heated. When the design temperature is reached the gap will open at the interface shown in Fig. 2 by a sign "Gap opening location…"

4) At this point we can insert the desired thickness shim from the side and then start torque the bolts to move the wedge to the desired position in order to gain the desired prestress after cooldown.

This measure will take care of the slack and will still leave up to 14.5 mm for the purely preloading purposes. We did not have this problem with the CSMS due to different mechanism of loading, where the force, not the displacement was controlled. However even if all the fabrication gaps are taken care of the total vertical travel of 14.5 mm is not enough, we need at least 20 mm, in reality a little more. It would be logical to use the lower wedge to increase the vertical travel.

The wedge below is intended to be used as well to provide the necessary travel. At full stroke of 60 mm horizontally it will add another 14.5 mm of vertical move, which would almost satisfy the requirements for the needed vertical travel of about 30 mm. However, the lower wedge has to lift the whole dead weight of the CS assembly.

Let us see what lifting force we can have on the existing wedges and 100 mm bolts.

Let's consider the schematic in Fig. 4 and define terminology. Technically, we have three wedges – the one which is pulled by the M100 bolt and two wedges below and above to assure horizontal interface with the buffer zone and the upper key block, which we will call wedge sliders (see Fig. 3). These wedge sliders have guides preventing circumferential misalignments of the wedge.

Fig. 4 shows the wedge with forces on it. Each wedge slider experiences the dead weight force P. This force P is supported by a vertical projections of the normal reaction force from the wedge. Thus, the wedge slider generates on each pinched surface of the wedge a normal reaction force N. In addition, there is a friction force F acting on the wedge (again on both sides), directed against the wedge moving. So, three forces are applied to the wedge – the normal force from the wedge sliders, N from each side, the pulling force by the bolt M100, T and friction, F, from both wedge sides.

The equilibrium of the wedge equations will allow us to determine how much pull we need to have to lift the coil and move the wedge.

Horizontal equilibrium (neglecting the wedge weight):

$$T = 2(F\cos\alpha + N\sin\alpha) \qquad (1)$$

Vertical equilibrium of the wedge slider:

$$N = \frac{P}{\cos\alpha} \qquad (2)$$

Taking into account that by definition the friction force is as follows:

$$F = fN \qquad (3)$$

We obtain that to lift a weight P with the wedge we need to pull with a force T as follows:

$$T = 2P(tg\alpha + f) \qquad (4)$$

Now we need to consider options, since friction can be varied.

Suppose we use a usual stainless (or JK2B) wedges and wedge sliders. If we do nothing special, the coefficient of friction steel on steel will be somewhere between 0.15 and 0.74 or even higher (see appendix). The reference [4] gives f=0.78-1.02 for dry steel 302 or 321 depending on hardness (does not specify surface conditions).

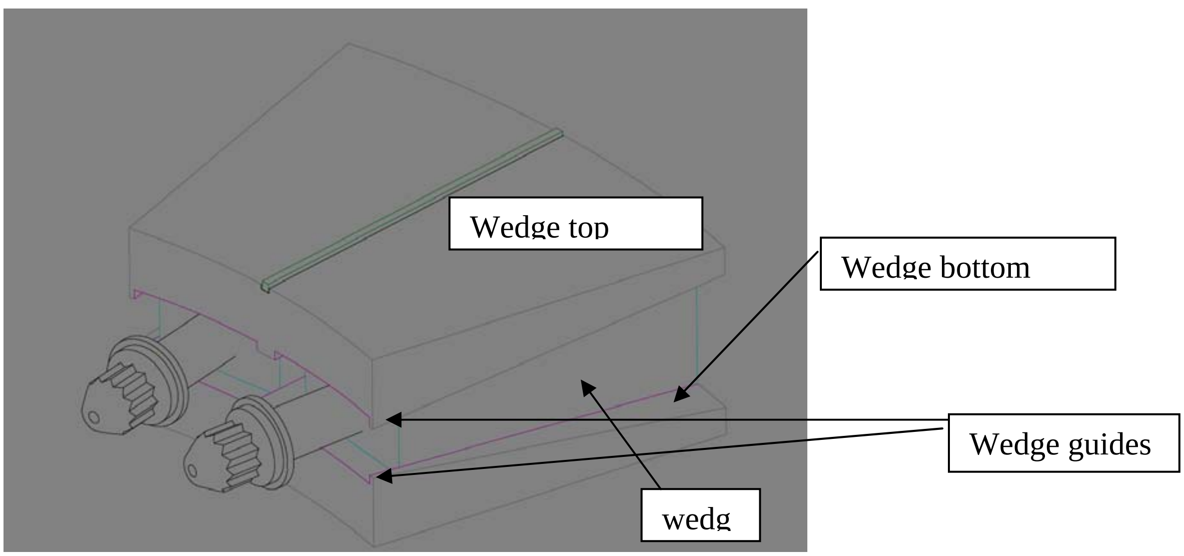


Fig. 3 Wedge details

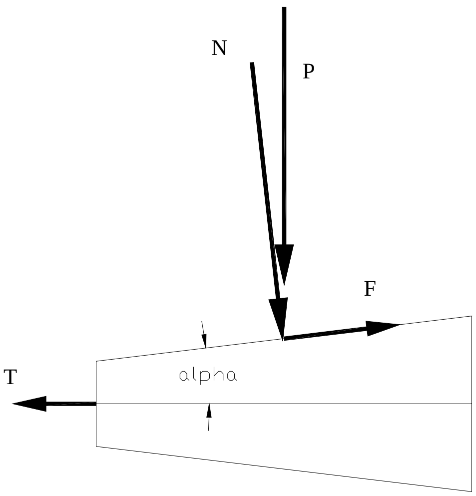


Fig.4. Forces equilibrium on the wedge

This is quite a spread.

Let's assume conservatively coefficient of friction to be 1.

Let us assume also that the allowable stress on the bolt M100 is 500 MPa at RT.

Then we can find that the pulling force on the bolt to lift the coil. For M100 the working cross section diameter is 94 mm, which has an area of 6.6e-4 m^2, gives allowable force of 3.3 e5 N, or about 33 t.

With the pulling force capacity of 33 t, the lifting capacity per bolt at f=1 is only 14.7 t. It is kind of disappointing, high friction diminishes the wedge effect. How much capacity is needed to lift the coil?

Each tie plates have two wedges, total 18 per the CS. In the unlikely event if we decide to procure 18 hydraulic torque wrenches and synchronize the wedge pulling, the total lifting capacity is 18*14.7 t=264 t, not enough to lift 600 t CS winding pack. Good chance we will break the bolts.

More realistic and convenient procedure would be to torque only two bolts at a time, in this case to lift the 600 t coil we need a lifting capacity of 300 t per pair (due to the leverage) or 150 t per one bolt. At f=1 our lifting capacity is 10 times less than needed.

If we use an oil based lubricant, the coefficient of friction goes down to 0.15-0.16 [5]

In this case, the maximum lifting force is 58.7 t, so 18 bolts theoretically can lift the coil if act synchronically, but can not do it if operated in pairs at a time.

To save the design and make it workable, we can use low friction materials, like DU, MoS_2, different type of Teflon based surfaces and coats and some graphite surfaces can go as low as 0.02-0.05 with coefficient of friction.

At f=0.05 the lifting force per bolt grows to 96 t and such a provision will allow to use the lower wedges as intended for preload.

The only problem is that the wedge is not self locking anymore. To remind, the self-locking feature is the feature when the wedge squeezed by the sliders will not require any pulling force to stay in place. That will happen only if coefficient of friction is larger than the tangent of the wedge, in other words friction is stronger than the expelling force. Since at the designed value of the wedge angle of 6.9 degrees the tangent is about 0.12 to be a self-locking the coefficient of friction must be higher than 0.12 with a margin. In the operation the forces on the wedge will be up to 12 times higher (74 MN at EOC at the top, 73 MN at SOC at the bottom wedge, versus 6 MN dead weight, see [2]) than the dead weight, therefore having too low coefficient of friction is not wise, unless there is a physical lock preventing the wedge to move.

One of the possibilities of such a lock could be insertion of a block of appropriate thickness into the empty space left behind the wedge (shown as the “Locking block location” in Fig. 2). In this event the outer plate will help to keep the wedge in place in addition to friction and the pulling bolt.

What helps also, the operating loads will take place at the cryogenic temperatures, when even low friction materials have coefficient of friction higher than 0.2, which should maintain lock in situation.

Discussion

Let’s summarize the results.

1. The current design needs to be modified to be functional.
2. The hole for the pulling bolt M100 shall be modified to a slot allowing vertical travel of say 20 mm.
3. The needed vertical travel of the wedges may be not sufficient to preload the coils even if both: lower and upper wedges are operational. To make it sufficient we may want to insert shims after the heating the tie plates into the gaps, which is supposed to open and even use a top wedge to preload the coils before cooldown if necessary to eliminate a slack in the interfaces.

4. The lower wedges require low friction materials to operate at room temperature, after that low friction may become problematic, although if friction increases at low temperatures it may become self locking.

Another and seemingly better approach seems to be not to use the lower wedge for adjustment, but use the top wedge for adjustment only. To provide an adequate vertical travel, the wedge angle of 6.9 degrees should be doubled to say 14 degrees, also double sided. In this case, the vertical travel will be about 30 mm per 60 mm of the horizontal travel, which compensates the loss of the lower wedge, but eliminates necessity for low friction materials and powerful torque wrench. To make this wedge self –locking we need coefficient of friction higher than 0.25 with a margin. Dry stainless steel, sand blasted and degreased will have f=0.55-1 and that would be quite sufficient. Just as a belt and a suspender philosophy, I would still recommend to insert a locking block to assure that the wedge is not moving under any circumstances.

Conclusions and recommendations

I recommend the following changes in the design of the CS preload structure:

1) Make a slot for the upper wedge pulling bolt M100 to accommodate the vertical displacements

2) Eliminate the lower wedge from the design

3) Increase the angle of the upper edge from 6.9 to 14 degrees

4) Use degreased sand blasted wedges to ensure self-locking feature of the wedge

5) Use locking blocks to eliminate wedge movement

Appendix 1. Friction coefficients

Contact Surfaces	slip coefficient
Steel On Steel- No treatment	0.15- 0.25
Steel On Cast Iron- No treatment	0.18 - 0.3
Steel On Steel- Machined (Degreased)	0.12- 0.18
Steel On Cast Iron- Machined (Degreased)	0.15 - 0.25
Grit -Sandblasted surfaces	0.48 - 0.55

Effect of oxide film etc on coefficient of static friction			
MATERIAL	Clean Dry	Thick Oxide Film	Sulfide Film
Steel-Steel	0.78	0.27	0.39
Copper-Copper	1.21	0.76	0.74

http://www.roymech.co.uk/Useful_Tables/Tribology/co_of_frict.htm

Coefficients of Friction

Materials	Coeff. of Static Friction μ_s	Coeff. of Kinetic Friction μ_k
Steel on Steel	0.74	0.57
Aluminum on Steel	0.61	0.47
Copper on Steel	0.53	0.36
Rubber on Concrete	1.0	0.8
Wood on Wood	0.25-0.5	0.2
Glass on Glass	0.94	0.4
Waxed wood on Wet snow	0.14	0.1
Waxed wood on Dry snow	-	0.04
Metal on Metal (lubricated)	0.15	0.06
Ice on Ice	0.1	0.03
Teflon on Teflon	0.04	0.04
Synovial joints in humans	0.01	0.003

Source:

Serway Physics for Scientists and Engineers
4th edition (p. 126.)

Friction Center Coefficient Database

The table below gives static and kinetic friction coefficients for various combinations of materials. These values are for reference only and actual values will vary depending on the particular application conditions.

Friction Couple	Conditions	static coefficient	kinetic coefficient
aluminum / aluminum	oxidizing environment	1.9	
aluminum / steel		0.61	0.47
automotive brake pad / cast iron	humid environment		0.2 - 0.5
brick / brick		0.65	
carbon composite / carbon composite	inert environment		0.5 - 1.2
carbon composite / carbon composite	humid environment		0.1 - 0.5
copper / copper	inert environment	4.0	
copper / copper	oxidizing environment	1.6	
copper / steel		0.53	0.36
cortical bone / cencellous bone	saline lubrication	0.61	
diamond / diamond	clean	0.1	
diamond / diamond	lubricated	0.05 - 0.1	
glass / glass	clean	0.94	0.40
glass / glass	lubricated	0.2 - 0.3	
glass / metal	clean	0.5 - 0.7	
gold / gold	inert environment	4.0	
gold / gold	humid environment	2.5	
ice / ice		0.1 0	0.03
iron / iron	oxidizing or humid environment	1.2	
leather / metal		0.55	
metal / metal	lubricated	0.15	0.05
mica / mica	clean, fresh cleave	1.0	
mica / mica		0.2 - 0.4	
nickel / nickel	inert environment	5.0	
nickel / nickel	oxidizing environment	3.0	
nickel / nickel	humid environment	1.6	
nylon / nylon	clean	0.20	
rubber / concrete	varying	1.00 - 4.00	0.80
sapphire / saphire	non-lubricated	0.2	

sapphire / steel	non-lubricated	0.15	
Silver / Silver	oxidizing or humid environment	1.5	
steel / steel		0.74	0.57
synovial joints (humans)		0.01	0.003
teflon / teflon		0.04	0.04
tungsten carbide / graphite		0.62	
wood / stone		0.40	
wood / wood		0.25 - 0.5	0.20

Summary

References

[1] P. Titus, draft memo on the CS structural analyses June 16th, 2005

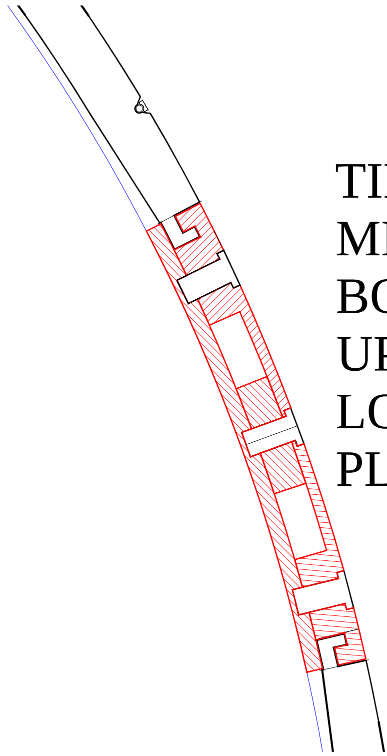
[2] Y. Fu, Global Stress Analysis and Pre-compression Requirements of CS Solenoid, ITER report, 10/30/05

[3] P. Michael, Lumped parameter's model for CS preload variation, **Date:** 17 June 2004, **Reference #:** ITER/US/MIT/PMichael/061704-1

[4] R.G. Bayer, A.T. Shalkey et al, "Designing for Zero Wear", Machine Design, 41(1): 142, January 1969

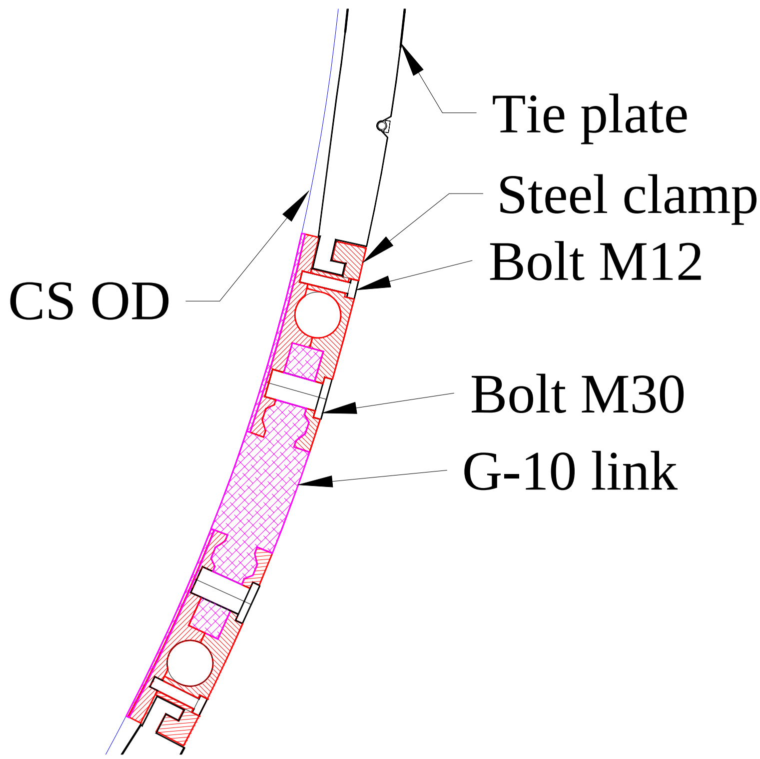
[5] R. Bolz, G. Tuve, "Handbook of tables for applied engineering science", 2nd Edition, CRC Press, 1976.

Attachment 6

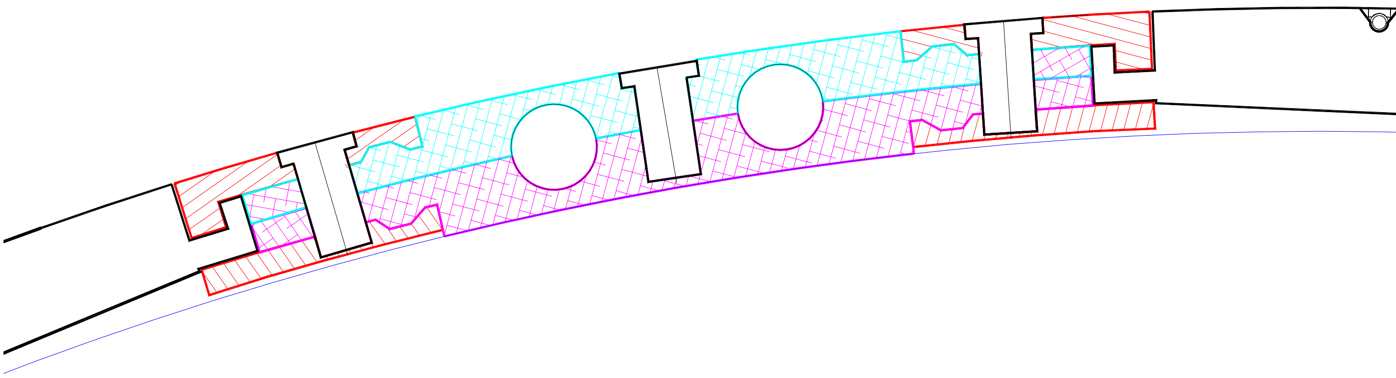


TIE PLATES LINK 100 MM TALL AT THE BOTTOM OF EACH UPPER AND TOP OF LOWER MODULES IN PLUMBING SPAN

This is where the plumbing is in between tie plates. There is 100 mm or so at the bottom of every CSU module and at the top of every CSB available. That is where we make belts. No intent to insulate between plates, unless it turns out a problem, then we can figure out how to insulate if needed.



This is the arrangement near the breakout point (when there is no other bus, there will be no hole). G10 is the insulation break for eddy currents. So far it does not have a horn from the dummy belt, as Peter proposed, since I hope it is not necessary (subject to Peter's analysis), but if need to be, we can bolt the dummy belt structure through the G-10. In that case some work on insulation will be needed, since the bolt will see 20 kV above ground. Possible, but nuisanse.



That is where the buses joggled together. Transition section will designed later.

Attachment 7

LLNL

Memo

To: Peter Titus, Phil Michael, Neil Mitchell, Kyoshi Yoshida, Youkun Fu, Cornelis Jong

From: Nicolai Martovetsky

CC: Joe Minervini, Timothy Antaya

Date: 2/27/06

Re: Deflections of the leaf springs

Executive summary

The flexible plates (leaf springs) are used to support the CS in the current design. The springs bend mostly due to the axial force, negligibly due to deformation for the winding pack. P. Titus ANSYS analysis indicates excessive stress in the springs. The main reason for deflection is the moment which the EM force creates, since current gusseted bracket design has attachment point about 1 m away from the point of the applied force. Several options are available. One, as proposed by P. Titus is to build a ring, to make the attachment stiffer and eliminate rotation of the brackets. This ring is conceptually designed and will be analyzed by Peter some time soon. Te other option is to move the spring in line with force application and thus eliminating the original problem. That will require correction of the bracket – relatively simple and less expensive option than the top ring. Both concepts are presented in the memo.

Background

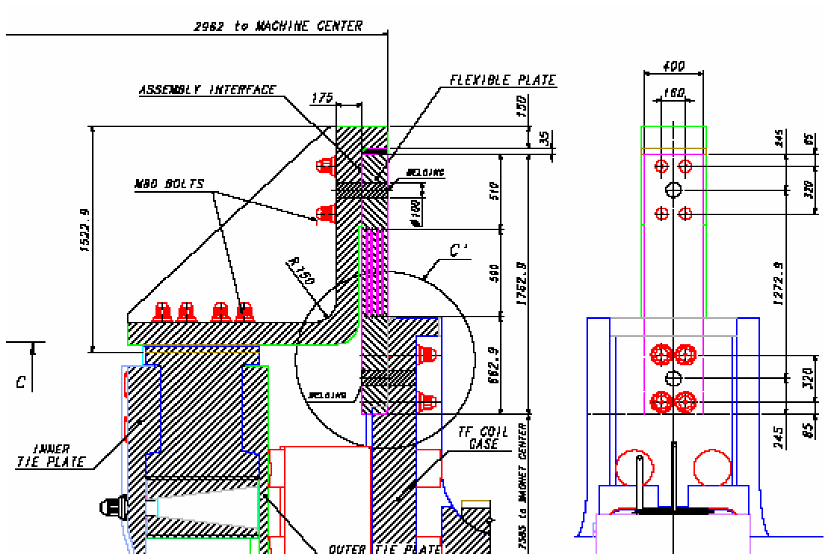


Fig. 1. Design of the support

The support structure for the CS is shown in Fig. 1. The bracket with gussets is attached to the key block holding tie plates from one side and to the flexible plate (leaf spring) with M80 bolts (M80 BOLTS marker points at it) on the other side of the corner angle.

P. Titus analyses [1,2] showed that the springs are flexed too much and stress is above allowable.

He suggested that the problem results from non-uniform deformation of the CS at the ID and OD resulting in rotation of the top buffer zone, which kind of rotate the brackets and cause excessive flexure of the flexible plates.

I used analysis by Y. Fu [3] to verify this suggestion.

Not discussed in this memo is still unaddressed issue of the initial conditions in P. Titus analysis [2]. C. Jong [private communication] pointed out that the initial conditions in P. Titus model may be too conservative to begin with. However P. Titus argues that even if true, the initial conditions will not affect maximum stresses significantly. This issue is still awaits P. Titus assessment, but in light of the current report may turn out not very important if design changes are made as required.

ASSESSMENT

Careful study of displacements in the CS does not support the explanation that the reason for rotation is the nonuniform deformation of the winding pack.

The difference in the outer and inner plates strains are too small to account for big radial displacements of the flexible plates.

Fig. 2 shows stresses in the Inner and Outer tie plates.

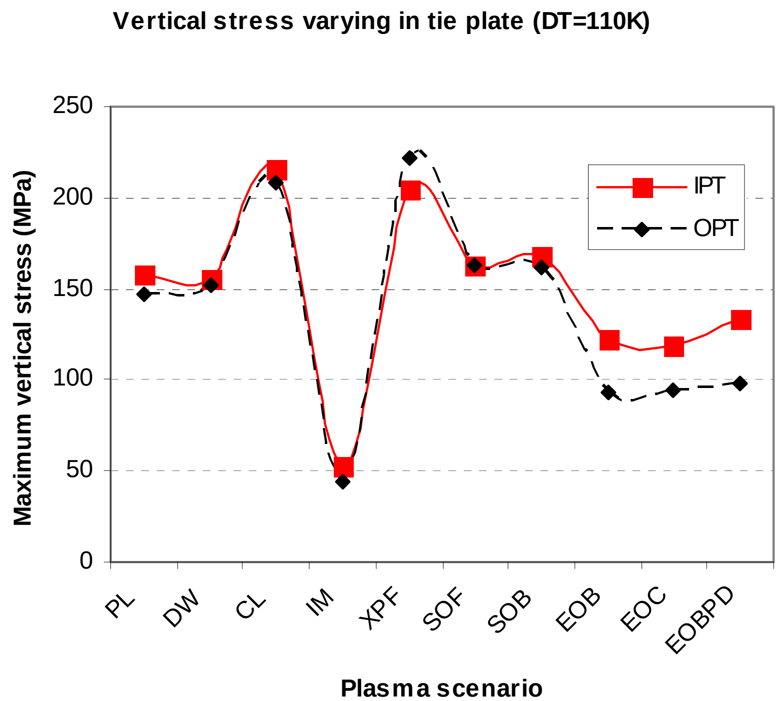


Fig. 2. Stresses in the tie plates in different points of the scenario.

First of all, the difference is small, 20 MPa or so at the most. The most loaded situation on the coil, when ID is compressed more than OD is the IM, when all the modules have the current of 40 kA. But as we can see there is no significant difference in stresses in the ID and OD tie plates at that point. So, internal loading and bending of the CS stack does not cause the rotation of the brackets on the top.

As we said, the maximum difference is at EOB-EOC (about 20 MPa). At the modules of 200 GPa, 20 MPa gives only 0.01% strain. At 14 m tie plate length, it translates in 1.4 mm elongation.

Fig. 3 shows that if 1.4 mm differential dz takes place at the base of the bracket, the radial deflection is only 2.5 mm.

The ANSYS results show that the deflection is up to 18 mm. Fig. 4 shows displacements of the coil and structure at R=2.09 m, at the OD of the CS.

One can see that the radial deflections are much bigger than anticipated 2.5 mm. Even if one takes into account that the center of rotation may be at the bottom of the buffer zone, not at the top, the 2.5 mm radial displacement become a little more, but not 10+ mm.

The explanation about rotation due to internal EM loads of the CS does not look credible.

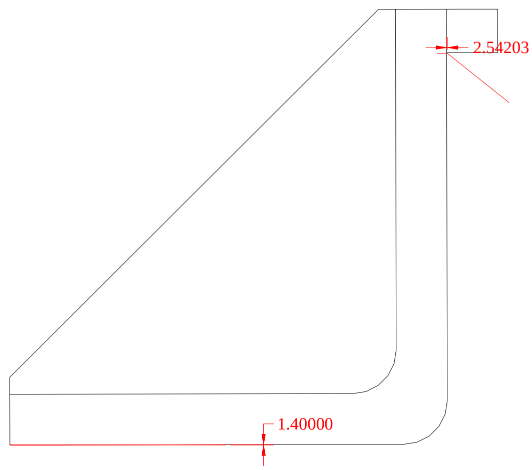


Fig. 3.1.4 mm DZ displacement of the bracket at the foundation causes 2.5 mm DR displacement at the top of the bracket.

Fig. 3 shows displacements of the coil parts at the OD of the coil R=2.09 m.

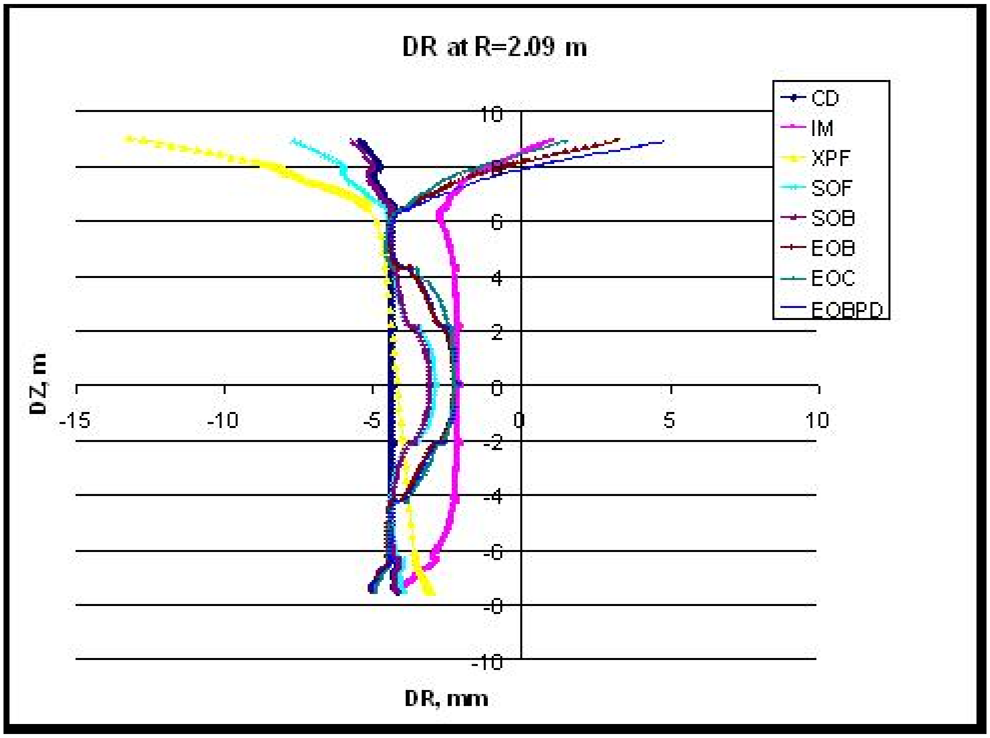


Fig. 4. Radial displacements of the CS at the OD. The winding pack is between Z= -6.45 and +6.45 m

One can also see that even when the ID and OD plates have identical stresses, the flex plate displacements do not go away. So, the internal loading of the CS is not directly relevant to the flex plates problem. What is? The net vertical force must be responsible. The flex plates support not only dead weight but also the net vertical forces generated in the CS by plasma and the PF coils.

The maximum force up is 42 MN, down is -30.5 MN, the dead weight of the CS assembly is about 10 MN.

Fig. 5 shows correlation between the vertical force and radial displacement of the top tip of the bracket at coordinates R=2.96,Z=9.42m.

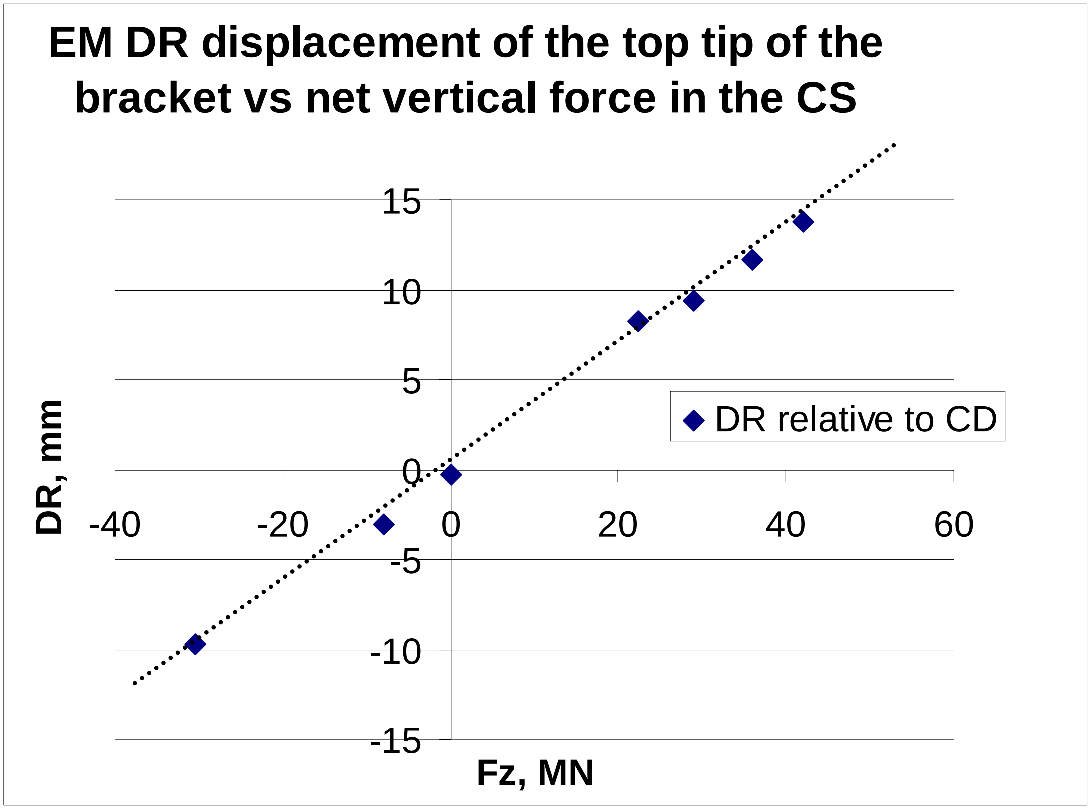


Fig. 5. Force-deflection plot of the tip of the bracket.

The points lie pretty good on the straight line, showing that it is just a trivial linear phenomenon. Slight scatter around the line indicates those other effects beyond net vertical force.

The vertical displacement of the flex plate is very small – several hundreds of a micron (10^{-4} m) or less.

The problem

The stiffness of the suspension system is not sufficient to prevent large displacements in the radial direction, that causes large deflections in the leaf springs making stresses in the plates too high.

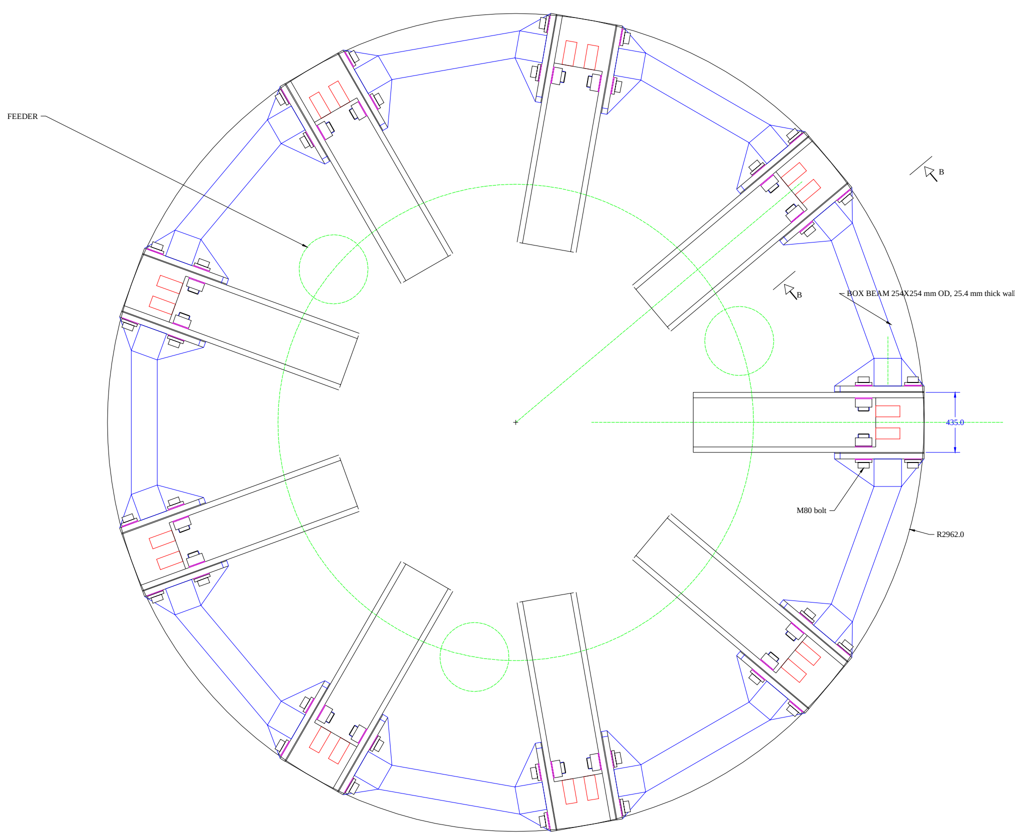
It is very obvious that the rotation happened not because of the EM forces within the CS deforming its winding pack, but the net vertical force and large lever arm, creating bending moment. Since the point of attachment of the springs is more than 1 m away from the force origin (the winding pack mean radius) it creates a large bending, causing the excessive deflection.

Mitigation

In general, we can reinforce the structure or reduce the forces. P. Titus proposed to increase the stiffness of the structure by introducing the top ring [2].

His first analysis indicated that it brings the stresses in the flex plates to normal, but the ring itself has too high stresses.

I designed a ring, shown in Fig. 6 which should eliminate the problem of high stresses in the ring and in the plates, but it needs one more ANSYS verification.



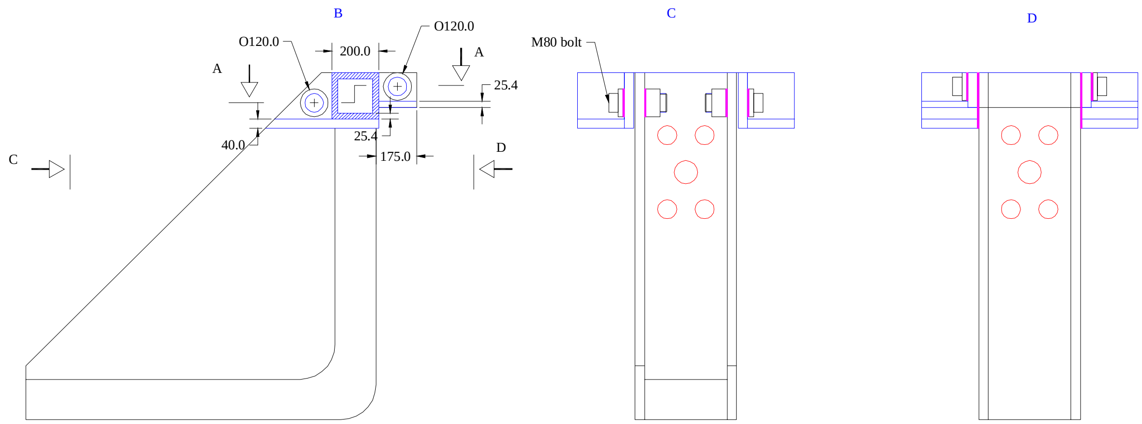


Fig. 6. A ring improving stiffness of the suspension structure and attachment details.

The design of the ring is not very difficult, but requires multiple insulating breaks, heavy weldings, a lot of machining. A similar idea, a little easier would have been to introduce spokes or a spider, that also would constrain rotation of the attachment points, but would not require insulating breaks against eddy currents.

Another way to deal with the problem is to eliminate the lever arm of the forces on the spring. Fig. 7 shows the principle of the possible change.

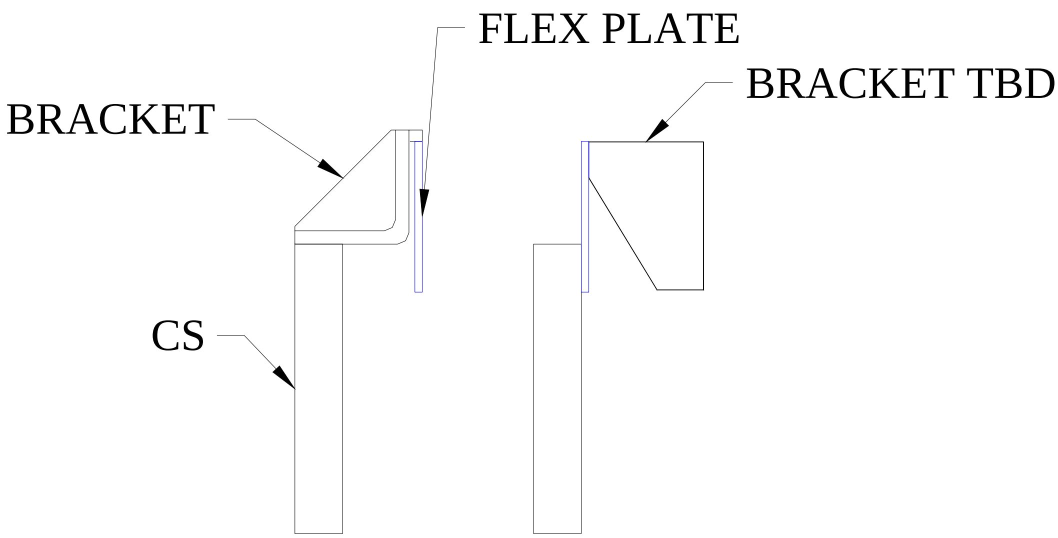


Fig. 7. The baseline schematic (left) and an alternative schematic (right) to reduce the flex plate stress problems

The alternative schematic puts the flexible plate close to the force application point, reducing the bending moment, thus dramatically reducing deflections on the flexible plates. In essence the stiffness of the ring in this proposal above is taken from the stiffness of the TF structure. On the first glance it seems that it is a better way to proceed than to introduce the ring, cheaper, less material, but it requires a redesign of the bracket and possibly the TF case in the attachment points, but do not seem like it requires a new analysis.

Summary

The reason of the flex plate problem is the vertical force and not well optimized suspension point design, creating large bending moment. Two options identified to solve the problem – to stiffen the attachment structure by a ring (or by spokes) or to eliminate the source of the problem – by moving the flex plate to the point of the force application. The latter looks more attractive but needs some design effort how to make an efficient support and think through the installation procedure. I will take on it in the nearest future.

Acknowledgement

Phil Michael and Peter Titus let me know that they started thinking in the direction of effect of the bending moment, when I showed that bending of the winding pack does not explain large radial displacements. That confirmed my suspicions about the role of the vertical force, which I initially developed from discussing the problem with C. Jong and Y. Fu couple of weeks ago. I am grateful to all of them for useful discussions and suggestions.

Mr. Fu helped a lot with his data from the ANSYS model.

References

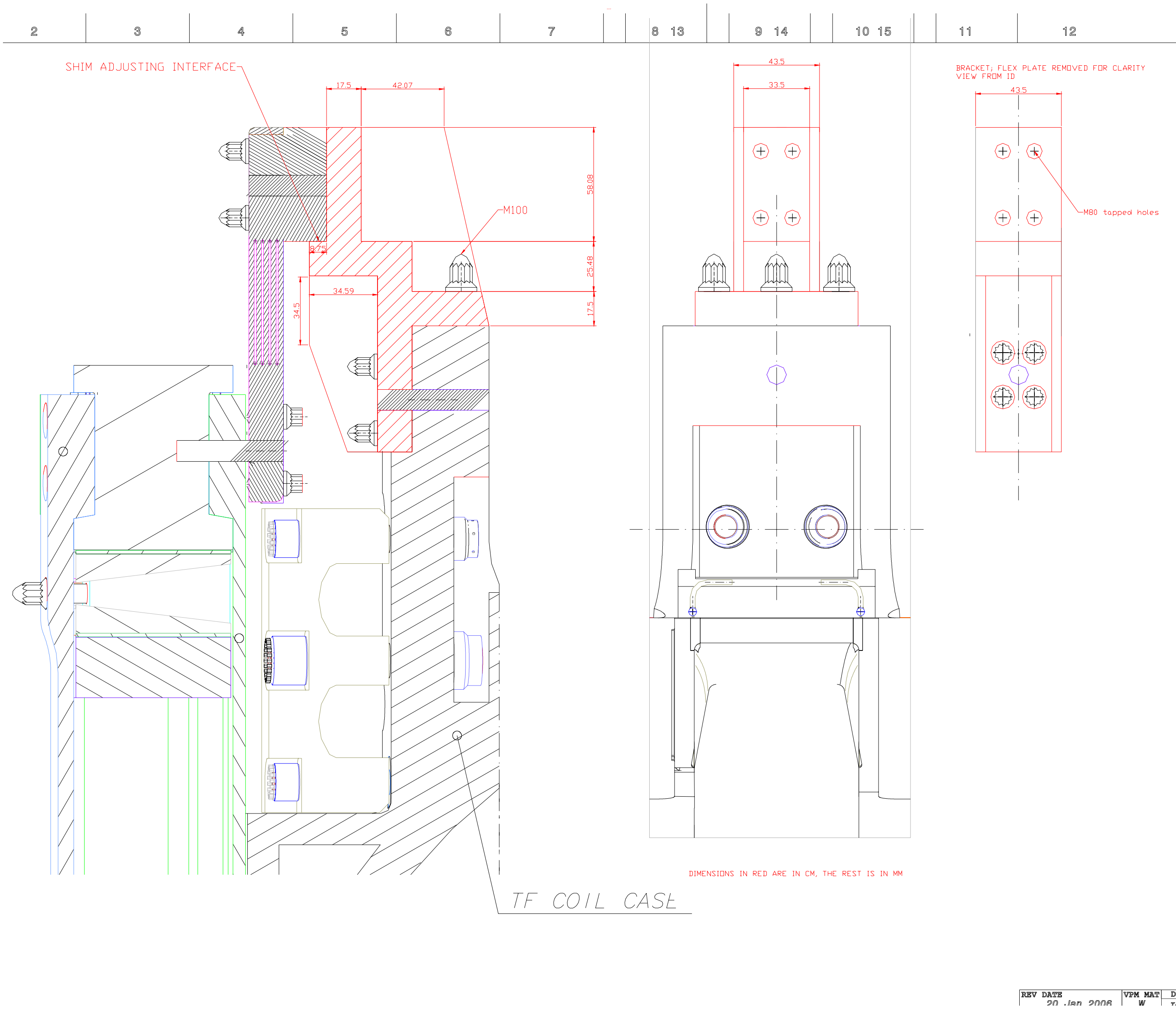
[1] P. Titus, draft memo on the CS structural analyses June 16th, 2005

[2] Memo to Neil Mitchel, Nicolai Martovetsky, Phil Michael

From: Peter Titus Subject: Flex Supports Date: October 17 2005

[3] Y. Fu, Global Stress Analysis and Pre-compression Requirements of CS Solenoid, ITER report, 10/30/05

Appendix 8



Attachment 9

TO: K. Yoshida, N. Mitchell, P. Michael, T. Antaya

FROM: Nicolai N. Martovetsky

SUBJECT: evaluation of the CS cooldown
DATE: August 1, 2004
Revised February 21, 2006

This memo discusses the concept of cooldown for ITER Central Solenoid.

Executive summary

This memo proposes a concept of cooldown for ITER Central Solenoid to show on the preliminary design drawings to give an idea what kind of plumbing should be used.
The memo proposes a method how to attach the tubing to the structure and how to verify the performance.

<u>Cooling of the structure</u>

Distance between the tubes.

Typical rate of cooling for large magnets is 1 K/hour. A table below gives a typical diffusion time. About 3 times is needed to equalize the temperatures of coolant and the most remote part. From this we select the maximum desirable distance from the tube to any part of the structure is 0.5 m. The penalty for larger distances will be a delay of the temperature equilibrium and higher potentially higher stresses (could be lower as well).

Table 1. Diffusion time in 316 LN structure

316 LN Heat diffusion					
					diff. path 1 m
Temp, K	Lambda, W/mK	Cp, J/kgK	Density, kg/m^3	a, m^2/s	hours
300	14	430	8.00E+03	4.07E-06	68.25
200	11	400	8.00E+03	3.44E-06	80.81
100	8.5	300	8.00E+03	3.54E-06	78.43
50	4.5	100	8.00E+03	5.63E-06	49.38
20	1.5	11	8.00E+03	1.70E-05	16.30

The tubes shall be running along the tie plates and then to the manifold. Since outer plate is 0.9 m wide, two tubes are desirable. The location could be in the grooves of the plates, better in the grooves of the exposed side (OD of outer plates, ID of inner plates), 0.3 m away from the edge, 0.6 m in between for the outer tie plate. Although inner tie plate could have lived with 1 tube, it is desirable to have two to provide the same flow per mass as in the outer tie plates, if the same inlet-outlet is used for the ID and OD tie plates.
The buffer zone will be cooled conduction from the conductor winding pack through insulation. Assuming insulation 12 mm (includes 10 mm ground plane and 2 mm of the pick up coil), the diffusion time through such thickness is about 1000 s at 250 K, so the temperature in the beginning of the buffer zone will be about following the coil temperature about 3*1000=1 hour later, which is fast enough. So, the buffer zone will be effectively cooled off the coil top pancake.
The massive upper key block is in contact with the tie plates through massive bolts and does not require additional tracing.
A good heat transfer is desirable. For that it is required that the tubes would have at least 50 % perimeter in good contact with structure. Machined groove and welded tube would be desirable. The CSMC showed that 1.5 mm wall was not thick enough to prevent burn through and leakage. 3 mm or thicker is advised if tubes are to be welded onto the structure. However, it would be the best if there would be no welding on the tube. We propose to use a weld shrink to maintain a good contact with the structure and copper mesh to provide a good guaranteed contact between the tube OD and the structure. The details of this concept will be presented below.

<u>How much flow is needed</u>

The mass of the structure is about 300 t.
Typical rate of cooling is 1 K/hour, as was said above between 300 K to 77 K. The duration of the cooldown is often determined to large extent by cooldown to LN2, since heat capacity drops down faster than cooling power of refrigerator in many cases, especially if LHe is accumulated to accelerate the cooldown below 77 K. The cooldown of the structure is much more difficult than that of the CICC, the heat transfer is by far worse for the structure, diffusion time to parts cooled by conduction is long.
In the case of CSMC the cooldown of the coil the 1 K/h brought the coil to 77 K in 220 hours and it became superconducting at 400 hours. However, the tie rods temperature, cooled by conduction, stabilized only after 518 hours and was at the level 16-20 K.
The other constraint imposed on the cooldown is maximum allowable temperature gradient to prevent excessive thermal stress, which is typically 50 K between the inlet and outlet. Let's calculate how much flow is needed to ensure such rate of 1` K/hour at the given gradient between inlet and outlet. The most difficult is the beginning of the cool down, so we take parameters at about 250 K and from the table below we find that if heat transfer is very efficient, then 128 g/s shall be enough.

If efficiency is less, more flow is required. Let's take safety factor of 2 and that gives us 256 g/s of He is required. We assume that the power of LN2 heat exchanger is sufficient to maintain such cooling rate.
The flow of 256 g/s roughly corresponds to the practical "rule of thumb" – 1g/s per 1 t of stainless or copper structure to cool down with the rate of 1 K/h.

Table 2. Needed flow calculated

Cooling mass		3.00E+05	kg	
CpHe		5.20E+03	J/kgK	
CSS		4.00E+02	J/kgK	
dT		50.00	K	
dT/dtau		0.000278	K/s	1K/h
Mhe		1.28E-01	kg/s	
With all inefficiencies		2.56E-01	kg/s	

Hydraulic diameter of the cooling pipe.

Let us assume that the cooling length is 30 m (manifold to manifold). Let's assume that the pressure drop shall not exceed 5 bar at 250 K.
Use hydraulic equations and assume the factor 2 for pressure drop for the plumbing, since classical formulas are good only for straight runs, for real tubing a safety factor of 2 is a reasonable compromise.

$$\Delta P = \xi \frac{m^2}{2\rho S^2} \frac{l}{d}$$

$$\xi_{turb} = \frac{0.316}{\mathrm{Re}^{0.25}}$$

$$\mathrm{Re} = \frac{md}{S\mu}$$

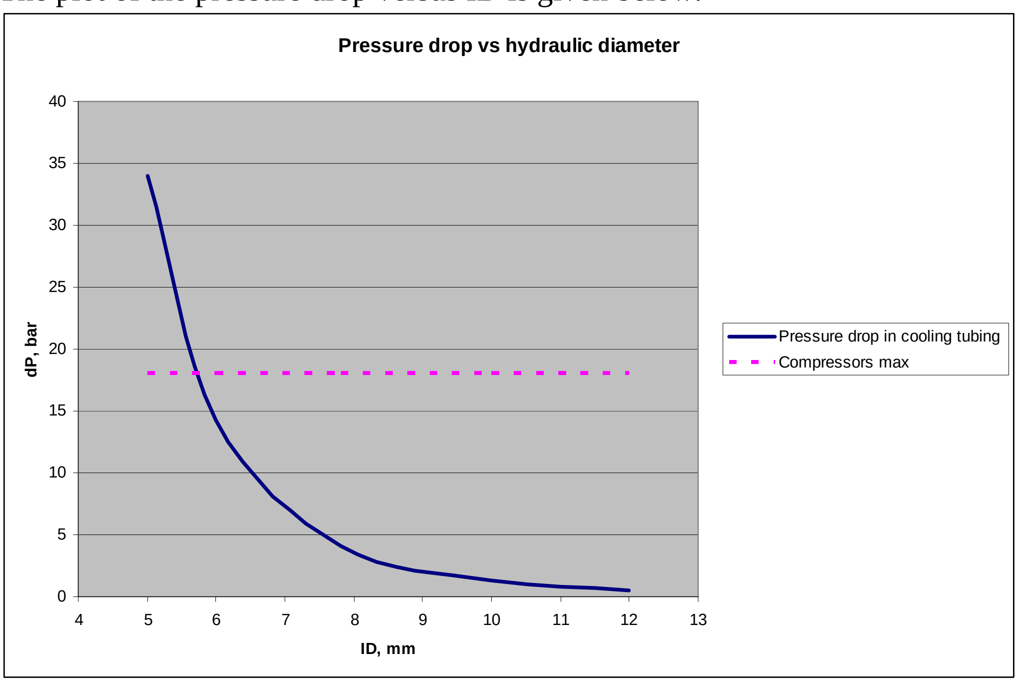
A table below shows assumed values and it follows that 10 mm hydrauylic diameter can

be used.

Table 3. Calculation of hydraulic diameter of the cooling pipes

length		30	m
multiplier for			
curved tubes		2	
hydraulic diameter		1.00E-02	m
number of parallel channels		36	
flow through channel		7.12E-03	g/s
dynamic viscosity	(225k)	0.000016	Pa*s
density	(225K)	4	kg/m3
Schannel		7.85E-05	m2
velocity		2.27E+01	m/s
Re		5.67E+04	
friction factor		2.05E-02	
friction factor with multiplier		4.10E-02	
pressure drop		1.26E+05	Pa

So, 10 mm ID is good, 1.26 bar, at 8 mm is OK – 3.6 bar, 6 mm – 14.3 bar – not acceptable, since there is an additional pressure drop between the compressors and the structure, typically several bars.
The plot of the pressure drop versus ID is given below.



Wall thickness.

Experience shows that the only reliable way to attach a tube to the structure is to weld it. Soldering is messy and unreliable, epoxy is very unreliable and inefficient. When a cold helium enters the tube it shrinks away from the structure and can break the epoxy, which would effectively thermally insulate the tube from the structure. Such an incident happened with the Nb3Al insert, in the ITER CSMC program, where the epoxy attached cooling pipe was completely inefficient in cooling the structure.
The experience with CSMC showed that 1.6 mm wall could be burned through by accident and even if pass leak test at RT it can develop a cold leak.
With the tube wall of 3 mm the appropriate cooling tube is ANSI (thanks K. Yoshida-san), who gave me the catalog data. Table 4 gives the parameters of this tube.

Pipe nominal 3/8 schedule 80				
OD	17.145	mm	0.645	in
ID	10.7442	mm	0.423	in
Wall	3.2004	mm	0.111	in

Still a little safer and hopefully still acceptable option is to develop some kind of attachment which would have a good thermal contact and eliminate the risk of burn through during installation, in other words remove welding from the tube wall.

Attachment of the tube to the plate

The details of attachments may be as shown in Fig.1. The groove shall be a little undersized. The tube shall be pressed into the groove with a tight fit with some elastic deformation and reliably held during welding, that the after welding shrinkage would not lift the tube and separate it from the cradle, since the following welding and cooldown will tend to separate the tube from the structure. Elasticity of the compressed and squeezed tube should prevent that. The exact dimensions of the groove shall be established by couple of mock-ups. Then tube is welded to the groove, 50% skipped weld with about 3 mm fill by the weld.

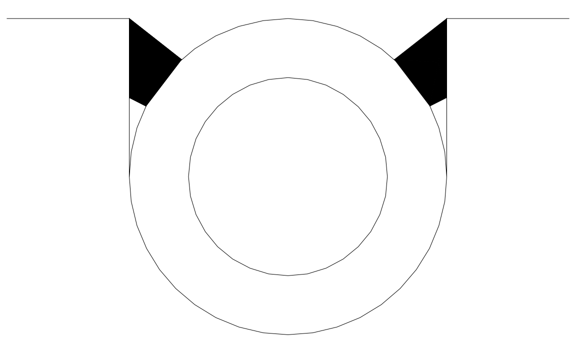


Fig. 1. Schematic of the cooling pipe arrangement in the structure.

How much weld is needed and how critical is a good contact with the groove.

If we just rely on the weld as a thermal bridge between the helium in the tube and the structure – is it sufficient for an effective cooldown? What is the criterion?

We may reason like the following. We send through the structure 256 g/s of He with inlet-outlet temperature difference of 50 K at Cp=5.2e3 J/kgK. This flow gives us about 66.7 kW of cooling power.
Let us estimate how much equivalent temperature drop will be if we assume good heat transfer through say, 70% of the surface of the tube to the structure (case when we have a good contact) and then compare with the case when we have no contact of the tube with the structure other than in the welds.
In case of good contact we have two thermal resistances – helium to the wall and through the wall thickness. Using formula:

$$Nu = 0.023 \mathrm{Re}^{0.8} \mathrm{Pr}^{0.43}$$

Knowing Re = 9.5 e4, and Pr=0.7, Nu= 189. At l=0.15, the heat transfer in the tube to the wall is h=2.8e3 W/m2K.
The area of the contact: We assume 20 m runs 36 tubes, which is 720 m. Let's assume 20 mm contact area, then the total area of contact is 14.4 m2. Then temperature gradient to transfer 66.7 kW of cooling through this surface and at this coefficient of heat transfer is dT= 66.7e3/(14.4*2.8e3)=1.65 K, which is comfortably low. The thermal resistance through the wall shall be compared with the helium heat transfer to the wall to see if thick wall contributes much. At thermal conductivity of 13 W/mK and wall thickness of 3e-3 m (3 mm) the effective heat transfer to the wall is 13/3e-3=4.3e3 W/m2K. It is a little better than helium. Taking into account this factor we come to conclusion that equivalent temperature drop will be 2.7 K, still low enough in comparison with 50 K of inlet-outlet, so the heat transfer through the wall is not a big obstacle in effective cooling.
Now, let us assume the worst case of a loose tube in the groove, that all the cooling goes through the 3 mm weld and the rest of the tube is not in a close contact. Then, roughly, the thermal pass is going through the wall from helium and then along the wall into the weld and then into the structure. Let us calculate just an equivalent thermal resistance of such a path. In 720 m and 50% skip weld from two sides we have 720 m of linear contact. The width of the contact is 3 mm, so the surface area is 3e-3*720=2.1 m^2. Let us assume that the thermal path from helium to the structure is about 15 mm along the tube and the weld. At thermal conductivity of 13 W/mK, the equivalent temperature drop between helium and the structure is 66.7e3/(13*2.1/15e-3)=36 K, which is not good, since at 50 K inlet-outlet, it appears that most of the temperature drop is happening in the tube wall due to poor thermal contact. As temperature reduces and thermal conductivity of steel drops down, it will be even worse.
Thus, just welding the tube in the loose grove in inefficient, a good contact is desirable, which can be done with a tight fit, pressing a tube into a little undersized groove.

An alternative approach

Still a little safer and hopefully still acceptable option is to develop some kind of attachment which would have a good thermal contact and eliminate the risk of burn through during installation.

As we saw above the major problem in the heat transfer between the helium and structure is the uncertain and potentially low conductance of the interface between the tube surface and the structure.
To accomplish a good thermal contact we will use a copper mesh between the tube and the structure. This principle is used for the high current contacts between aluminum and copper slabs and bars. Multiple contacts under high pressure with enough pressure create low resistance contacts.
We propose to use a copper mesh with a small diameter wires and high wire density.
An appropriate candidate could be the mesh with a high density (30% open area), with a small diameter copper which should create significant local pressure for reliable thermal contact. Small diameter copper also should keep the creep to minimum.

STG Wire Cloth Product Data Sheet

(800) 440-MESH Phone: (360) 835-8936 Fax: (360) 835-8966

Product Description:	Copper Shielding Cloth
Weave Type:	Plain Weave
Weaving Standard:	ANSI / AWCI-01, 1992
Mesh Count:	100 x 100
Wire Diameter:	0.0045" (0,1143mm)
Aperture:	0.0055" (0,1397mm)
Open Area:	30.25%
Weight:	0.136 Pounds Per Sq. Ft.
Material:	Commercially Pure Copper
Material Standard:	ANSI / ASTM A555-79

Shielding Effectiveness	Frequency
107 dB	400 kHz -
107 dB	1 MHz
105 dB	4 MHz
95 dB	10 MHz
90 dB	30 MHz
80 dB	100 MHz
65 dB	300 MHz
55 dB	1 GHz

48 dB	**2.5 GHz**
30 dB	**10 GHz**

Supply Criteria

Roll Length: 100 Feet (30,48M)
Roll Width: 36" - 48"

Fig. 2. Copper mesh 100x0.0045”

The idea to provide the prestress is based on a weld shrinkage.
The design of the cooling pipe attachment for the tie plates is proposed in Fig. 3. The guaranteed gap in the weld will result in guaranteed compression of the cover to the plate. The mesh laid between the tube and the structure shall provide sufficient heat transfer. This design needs to be tested in a simple configuration, which would measure the coefficient of heat transfer between the fluid and the stainless steel structure. The skip weld should be adequate, 50% of weld simultaneously from both sides 20-30 mm and then skip from both sides 20-30 mm. At the moment of the weld the cover must be tightly pressed to eliminate any gaps on the verge of plastic deformation of the tube.

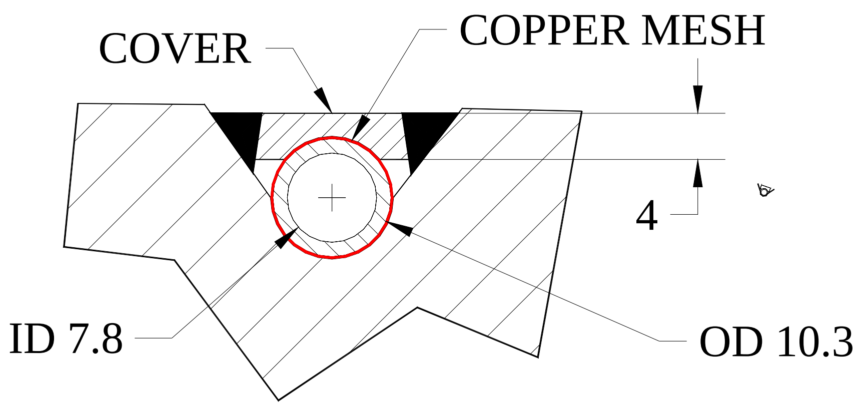


Fig. 3. Design of the cooling tube arrangement.

It would be desirable to verify high efficiency of such a design on a relevant mock up.

Verification set up

A possible simple samples to measure the thermal is shown in Fig. 4, where we will make the absolute measurements of the cooldown and comparative measurements with the ideal cooling channel, which is a drilled hole.
We propose to use LN2 for simplicity, instead of 250 K gas. We must use a colder substance, not warmer to initiate the tube shrinkage. The shrinkage of the tube will be more severe initially than in the CS structure, but if difference is not too large, it will demonstrate good quality of the thermal contact, which is the purpose of the test.
Fig. 4 shows a thick stainless steel plate 30 mm or so (could be anywhere from 20-25 mm to 40-50), where we would assemble the cooling tube in the groove. The other dimensions of 200-300 mm are a reasonable compromise between being relevant and reasonably low expenses on fabrication and cooldown, etc.
Then we would thermally insulate it with a Styrofoam and foam to eliminate radiation and convection heat losses and supply LN2 into the tube. The test set up could be as simple as shown in Fig. 5.

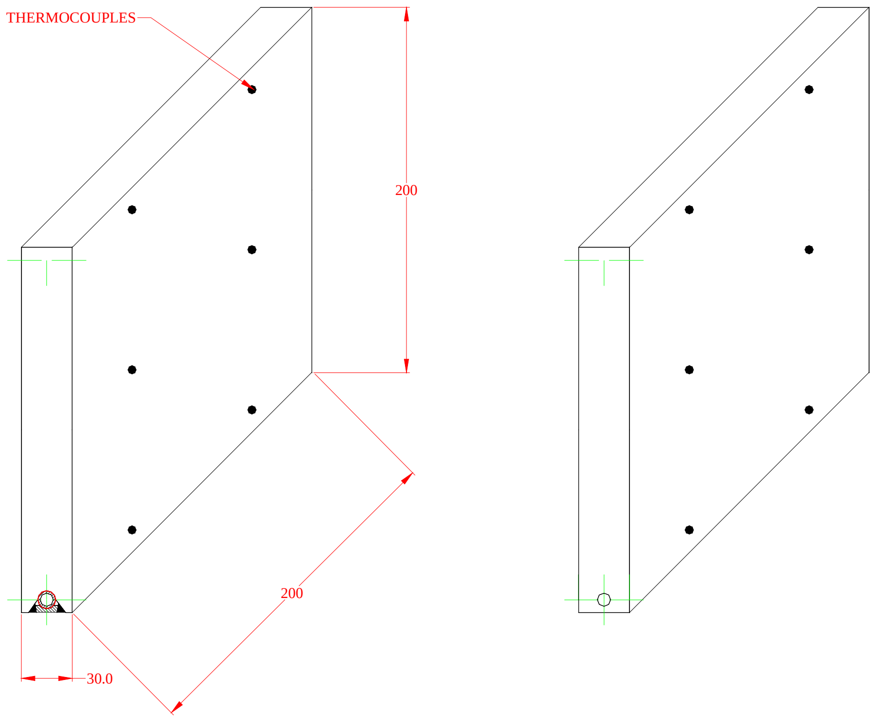


Fig. 4 Test piece for heat transfer study, with the proposed design (left) and reference piece with the drilled hole.

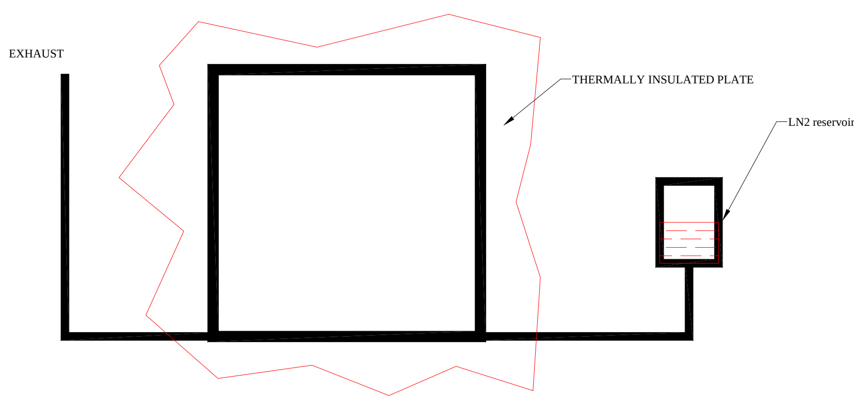
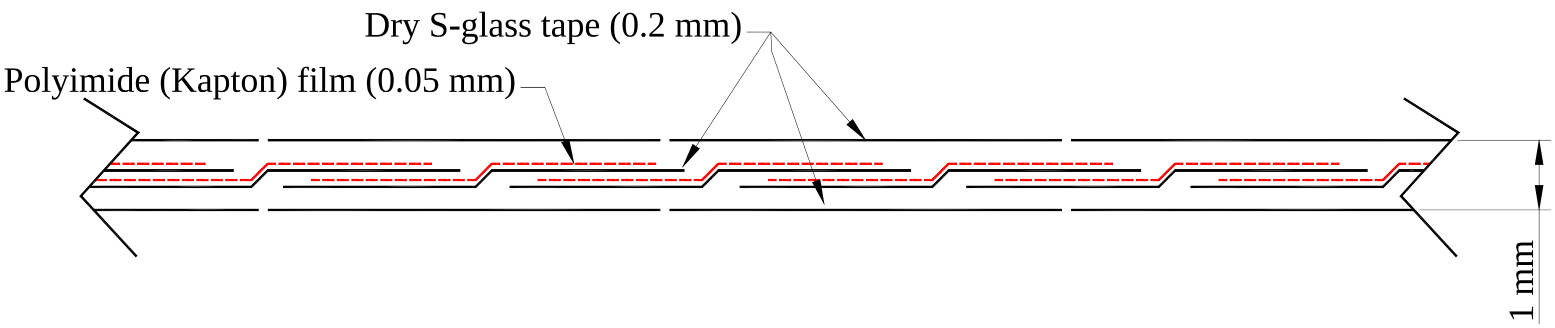


Fig. 5. Test arrangement

We should take the cooldown in 3 positions (6 thermocouples for redundancy). We should make sure that there is always LN2 in the LN2 reservoir. All lines must be insulated to minimize the thermal input from outside, which would make conditions equal for both plates.
The acceptance criterion shall be insignificant delay in cooling down the plate (say less than 15-20 % of the cooldown time).

Attachment 10


Dry S-glass tape (0.2 mm)
Polyimide (Kapton) film (0.05 mm)
1 mm

Attachment 11

LLNL

Memo

To: Peter Titus, Phil Michael

From: Nicolai Martovetsky

CC: Neil Mitchell, Yuri Gribov

Date: 11/16/05

Re: Plasma fields for structural analysis

Executive summary

For structural analyses on the CS it is acceptable to represent plasma as one turn with small cross section (like 1x1m2), which center is located at the coordinates given by DDD for different points of scenario.

Background

People computing magnetic fields for EM loads and structural analysis need to model plasma fields as well as fields from the CS and PF coils. Plasma current is distributed over a large cross section with varying current density, exact modeling is not trivial. I was not sure how to model it, but DDD, giving plasma current and coordinates of the center implies that it is OK to use just one turn with a filament current at this center with the corresponding current. The center of the turn moves around a little bit during the scenario.

I have not seen any check if such representation of plasma current is valid for structural analysis of the CS, like buses on the OD of the CS, or interaction between the CS modules. Therefore I requested Yuri Gribov to give me field components from full blown plasma current at the bus extensions location (R=2.13m) at EOB and I compared it with the fields I calculated from plasma presented by a turn 1x1 m2 at coordinates R=6.415 m and Z=0.54 m.

Results

Fig. 1 and 2 gives comparison between the real plasma simulation and a simple one used by structural analysts.

It shows that for the structural analysis the simple representation of plasma current is adequate.

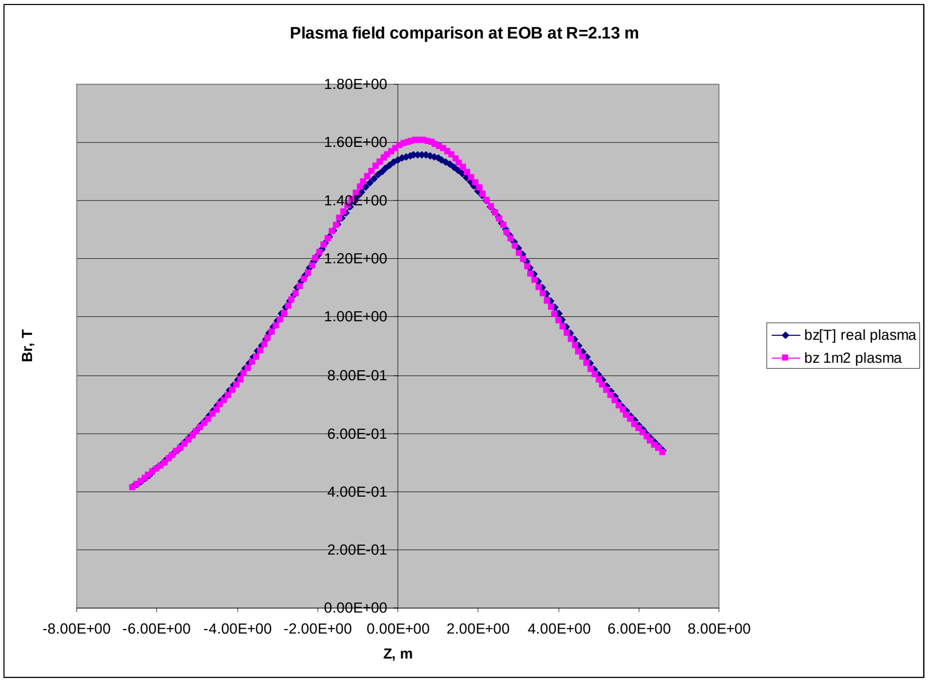


Fig. 1. Z-component of the field at R=2.13 m from plasma –simple model versus full current density distribution

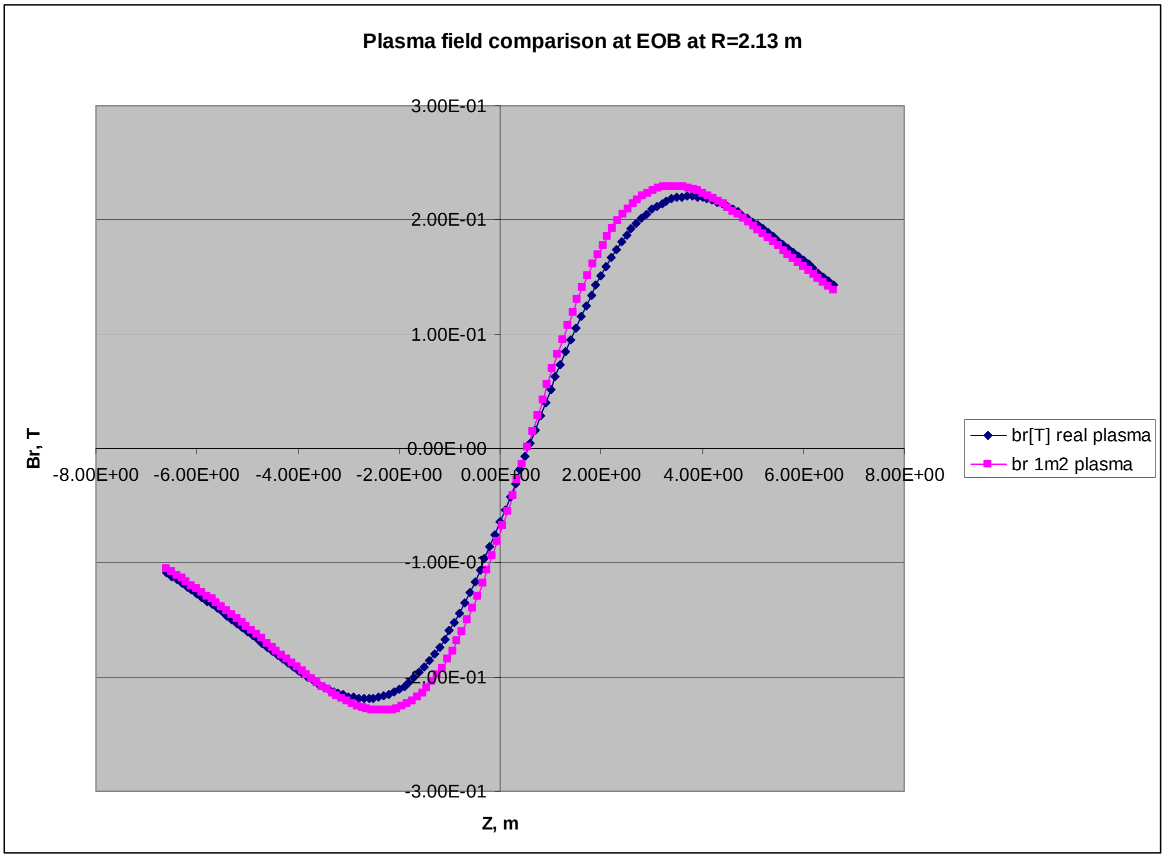


Fig. 2. R-component of the field at R=2.13 m from plasma –simple model versus full current density distribution

As we see the difference is not that large. Closer to the machine center, farther away from plasma, we should expect that the difference is even less. Since plasma dimensions are roughly the same starting with the flat top until end of burn, the plasma current is represented accurately enough by a single turn at the appropriate location.

Summary

Simulating plasma with a single turn is adequate for structural analysis of the CS.

Acknowledgment

Thanks to Yuri Gribov and Fujieda-san for responsiveness and quick analysis.