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SULTAN measurement and qualification: ITER-US-LLNL-NMARTOVETSKY-092008

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LLNL

Memo

To: John Miller, Mitchell, Schultz, Minervini, Bruzzone, Ciazynski, Bessette, Michael, Wesche, Stepanov, Zanino, Della Corte, Muzzi, Zani, Nijhuis, Vostner

From: Nicolai Martovetsky

CC: Heitzenroeder

Date: 08/29/06

Re: SULTAN measurement and qualification: ITER-US-LLNL-NMARTOVETSKY-092008

Executive summary

Measuring the characteristics of full scale ITER CICC at SULTAN is the critical qualification test. If volt-ampere characteristic (VAC) or volt-temperature characteristic (VTC) are distorted, the criterion of 10 uV/m may not be a valid criterion to judge the conductor performance. Only measurements with a clear absence or low signals from the current distribution should be considered as quantitatively representative, although in some obvious circumstances one can judge if a conductor will meet or fail ITER requirements. SULTAN full scale ITER CICC testing should be done with all measures taken to ensure uniform current redistribution. A full removal of Cr plating in the joint area and complete solder filling of the joints (with provision of the central channel for helium flow) should be mandatory for DC qualification samples for ITER. Also, T and I should be increased slowly that an equilibrium could be established for accurate measurement of Tcs, Ic and N. It is also desirable to go up in down in current and/or temperature (within stable range) to make sure that the equilibrium is reached.

Introduction

ITER is in the beginning of the CICC qualification period. Recently tested full scale ITER relevant CICC with advanced strands at SULTAN created a strong concern.

The news [1, 5] is that Tcs and Ic at 10 uV/m are much lower than expected. The N is also low, much lower than in the CSMC, where N-value was in turn lower than in the strand by a factor of 2-3. The Ic and Tcs degradation is much higher than in the previous samples with “less advanced strands”. This result is not fully unexpected. The new “advanced” strand has a higher non-copper fraction in the strands, and the amount of brittle Nb_3Sn in the non copper cross section was also increased to mitigate degradation observed in the CSMC strands in CICC.

Some people suspected that higher content Nb3Sn strands in comparison with CSMC type strands may not allow full realization of its increased Nb3Sn cross section due to worsened mechanical properties of the strands, which is the main reason of degradation in the first place. In other words, if brittle filaments break in a Model Coil strands, making the matrix less ductile in the advanced strands may make things worse despite higher cross section of Nb3Sn. On the other hand, in the beginning of this project (using advanced strands) this concern did not seem very bad: a subscale CICC with the advanced strands [2] showed no degradation, some first preliminary data on the advanced strands sensitivity to the strain [3] were interpreted that it did not get worse in comparison with the CSMC-TFMC type strands.

The SULTAN TFAS 1-2 full scale samples showed unusual voltage signals. The raw signals did not look as a pure superconducting transition.

The suspicion is that the current distribution is poor due to joints, as 10 uV/m is within the noise (in other words at 10 uV/m this effect masks the performance). Therefore this criterion makes correct assessment impossible. The same conductor in the TF may behave much better.

Since there is no final report on the TFAS tests, only presentations at different meetings or papers submitted to the conference are available, it is easy to make an error when assumptions are made by an outsider trying to access the results (like myself). A possible oversight could be that some sensors are not reliable and some noise is usual or unusual.

This assessment has several purposes.

1. Is it possible to formulate quantitatively which SULTAN tests represent the true conductor behavior. In other words, what conditions should be fulfilled before we claim – this test shows a true performance of the CICC with high probability that will be reproduced in the TF coils in ITER. What do we do with the results if some conditions are unresolved?

2. How strong are the assumptions in the Tcs (Ic) assessment? Is there a significant risk that the assessment may contain significant uncertainties? Are the assessments by the test group well substantiated?

3. What could be offered to improve the situation in test procedure or sample preparation?

EAS leg

First let's look at the EAS leg (left) in the TFAS1 (Fig. 1 reproduces assessment [1]). The story is a little complicated by the fact that the temperature sensor, which is considered the most representative, are the sensors 5 a, which were installed only after the last warm up of the sample, the last campaign. The sensors 5, both RT5 and LT5 are about 0.25-0.3 higher than RT5a and LT5a. Studies on the sensor readings [4] showed that the sensors downstream from the high field region are unreliable. It is speculated that the sensor itself might be OK, but the readout of the sensor does not represent the average temperature of helium due to possible hydraulic effects (the direction of flow is from top to the bottom, opposite to natural convection), which may create local temperature no uniformity.

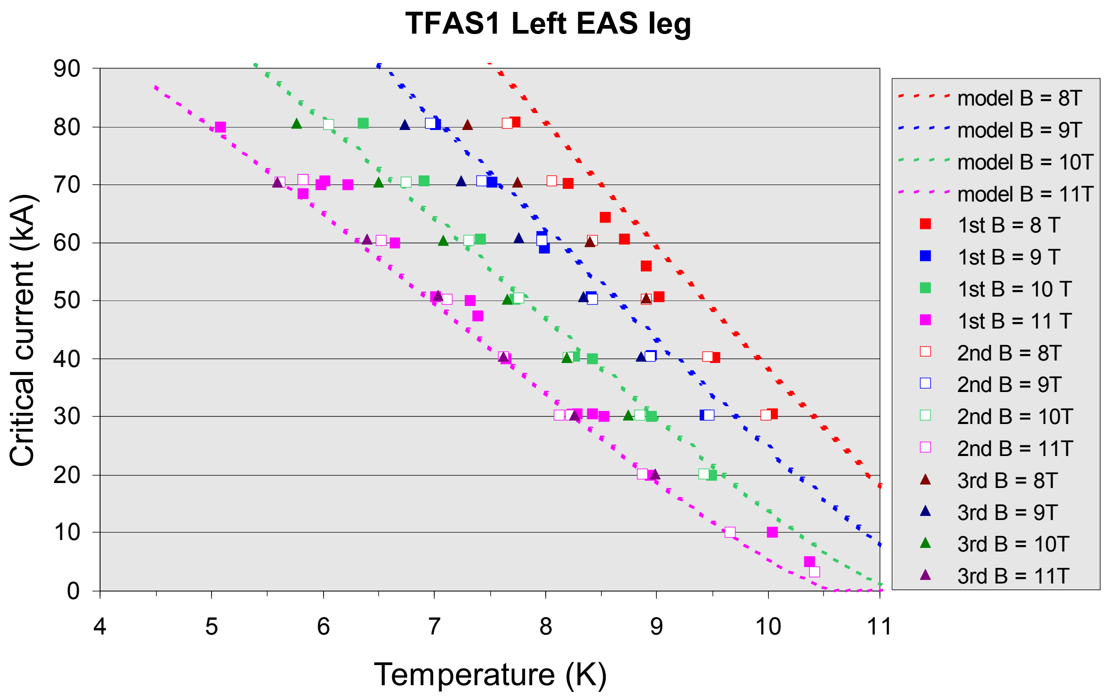


Fig. 1 Critical current vs. temperature for the EAS leg before and after cycles. Ciazynski – Cadarache, July 06 [1].

I will try to reproduce the result shown in Fig. 1 for say 50 kA at 11 T that gives 7.1-7.2 K. I am taking a file ADS310107 (Fig. 2) and see that if I just baseline it (blue diamonds), it is 7.3-7.4 K at 0.1uV/cm criterion. 2. That roughly corresponds to Fig. 1 data, to be compared to the second campaign minus 0.25 K to correct the LT5 readout for more accurate LT5a. So far I am in agreement with the SULTAN Test Group assessment.

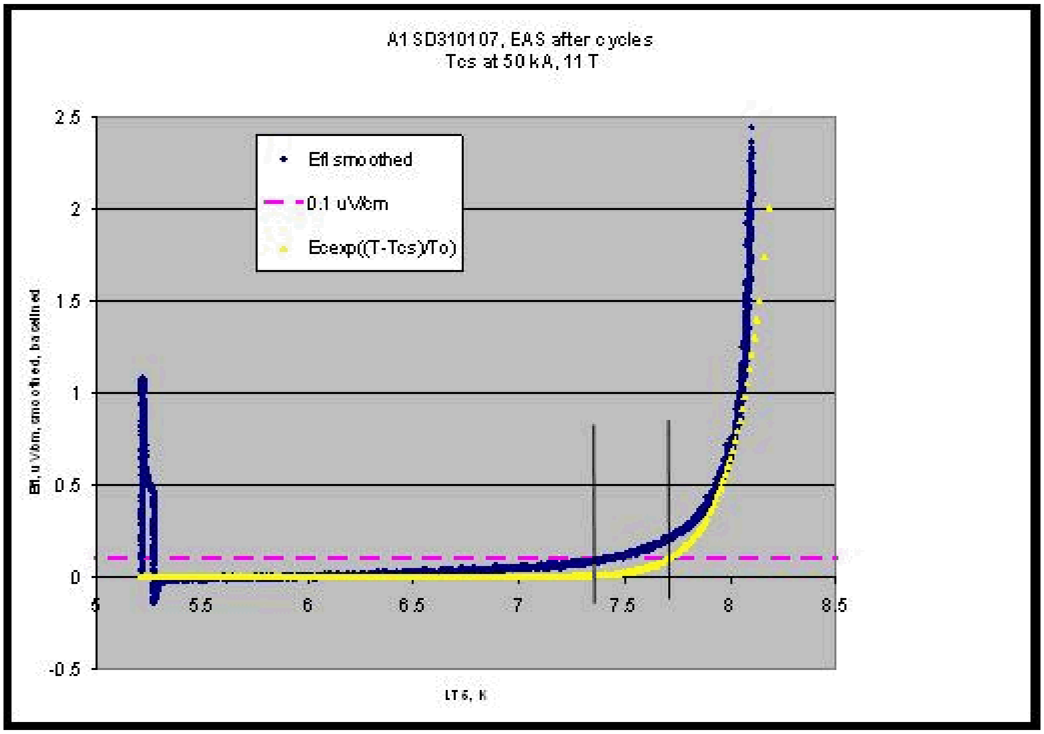


Fig. 2. EAS transition vs T. Now I am trying to see, if the VTC for EAS follows the exp (T/To) (indistinguishable from (I/Ic(T))^N in this narrow range).

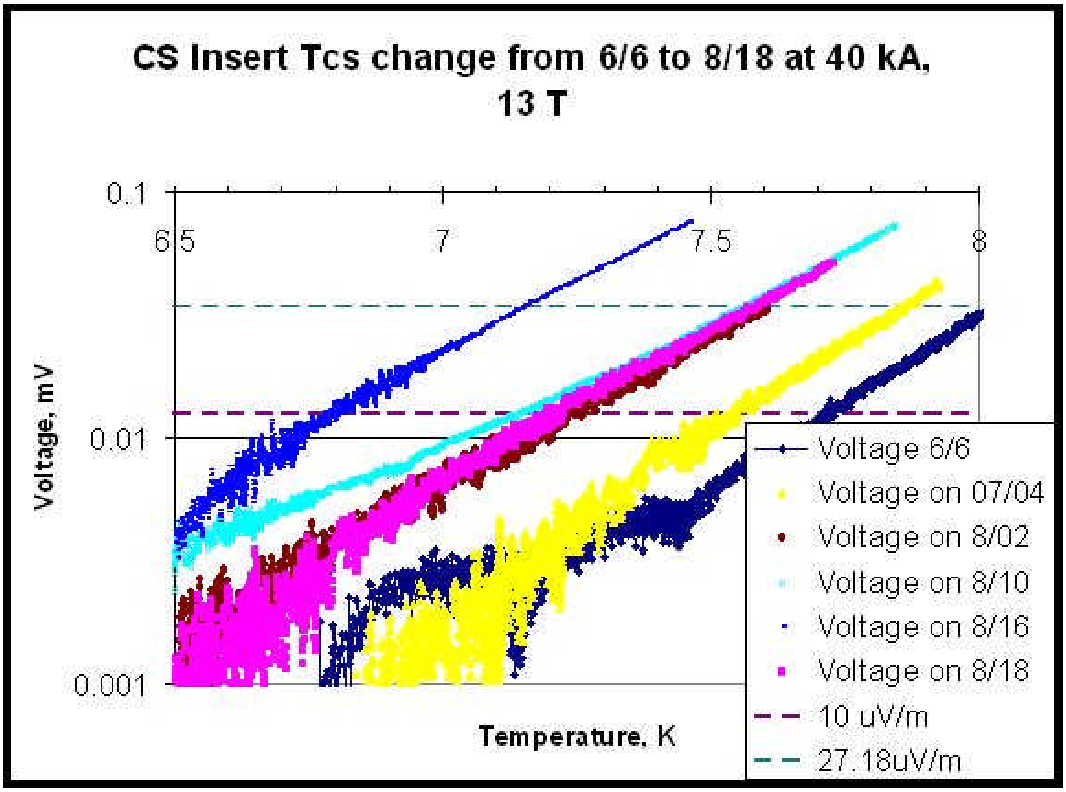
Now, let us try to approximate the transition with an exponential growth of the voltage with the temperature.

$$E = E_c \exp\left(\frac{I - I_c}{Io} + \frac{T - T_{cs}}{To}\right) \tag{1}$$

Where the Ec is the critical temperature, the Tcs is the current sharing temperature at the Ec and To is the temperature parameter of the voltage growth, Ic is the critical current at Tcs and Io is the current parameter of the voltage growth. This formula is close to another popular expression E=Ec*(I/Ic(TB))^N and to connect both I will try to indicate the N-parameter to make it easier for those who prefer this description, even when I work with (1).

All the CSMC Inserts: the CS Insert, the TF Insert, the Nb3Al insert had more or less exponential transitions with temperature and current after initial current distribution noise is overcome. The same is true for at least some SULTAN tests (CS 1.4, the SECRET sample A hairpin, both SULSAM samples) where it is known that the effect of joints was small.

Fig. 3 and 4 give couple of examples from the past experience of testing ITER conductors.



Volt-Temperature Characteristics (VTC) of the CSI before and after cycles (lower Tcs) and anticipated VTC from the witness strand data at 13.1 T at 40 kA

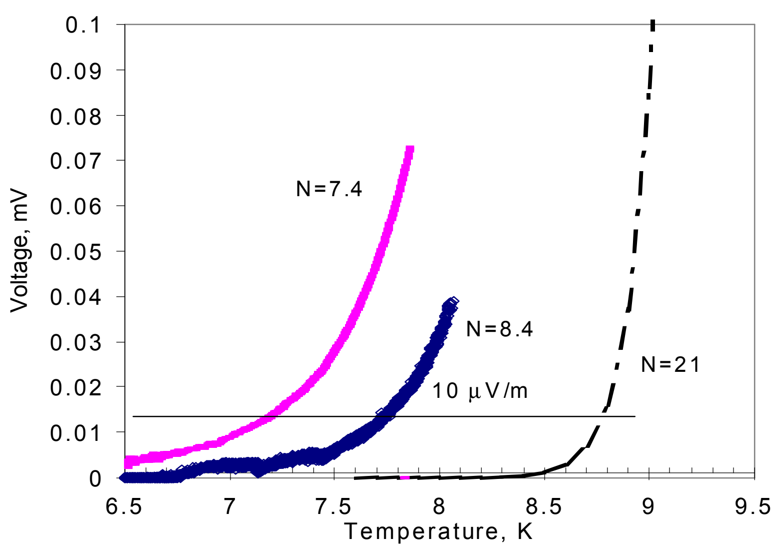


Fig. 3. The CS Insert VT traces taken before, during and after cycling tests in 2000. Voltage on 8/16 is for the reverse charge. The semi-log and linear plot for selected runs are shown. The lower plot shows a witness strand VTC, computed from the measured VAC.

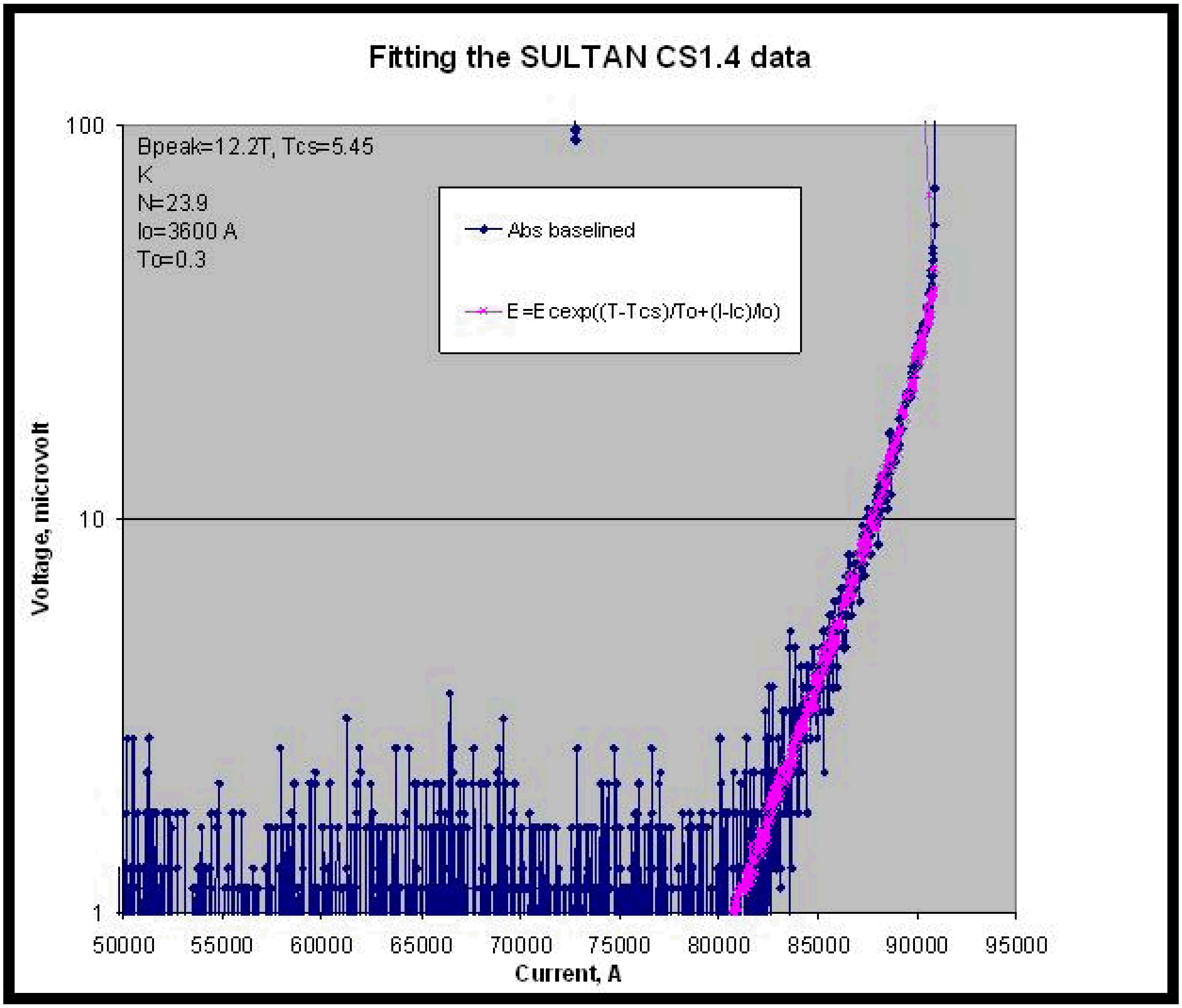


Fig. 4 SULTAN data from CS1.4 measurements. My processing of SULTAN data. The electrical field is baselined and an absolute value is taken, no Ohmic or inductive voltage correction is made. The voltage tap distance I guess was about 0.4 m, so 10 uV/s is about 4 micro Volts.

How easy is it to get rid of the nonuniform current distribution?

It was not easy even on the CS Insert. See below an example of the VTC measured slightly away from the center of the CSI (Fig. 5 a and 5 b). Negative voltage is a clear indication of the nonuniform current distribution. Two factors make distribution uniform in steady state: 1) high longitudinal voltage along the strands that pushes the current into the less loaded strands and 2) low transverse cable resistance.

The transverse resistance is determined by the transverse resistance in the joint and transverse resistance in the cable. The transverse resistance in the cable is inversely proportional to the length.

The length of the CS Insert was 120 m, 40 m in the peak field, whereas a SULTAN sample is 3.5 m long, 35 times advantage for the Insert. Normally, the transverse resistance in the joint is much lower than in the cable, but the CS Insert shows that the current distribution occurs in the cable as well. In SULTAN cable length is too short to play any role.

The maximum voltage in the CSI before quench was about 1 mV. In SULTAN, the max voltage before take off for ITER type conductors is about 40-100 microVolt at 50 kA or so. So, roughly an order of magnitude advantage for long length sample in developed voltage.

So, the uniform current distribution is much easier to achieve in the CS Insert. Despite of that, as we see, the effects of the nonuniform current distribution is seen even in the CS Insert. That sends a message that the joints in the SULTAN should have much lower transverse resistance to have a reasonable chance to be suppressed during measurements at the level 10 uV/m. To assure the lowest possible transverse resistance this we need to remove all the Cr from the joint region and fill it with the solder. The resistance between the sleeve and the facility bus is not that important for the uniform distribution, but it is important to have it low for the facility operation.

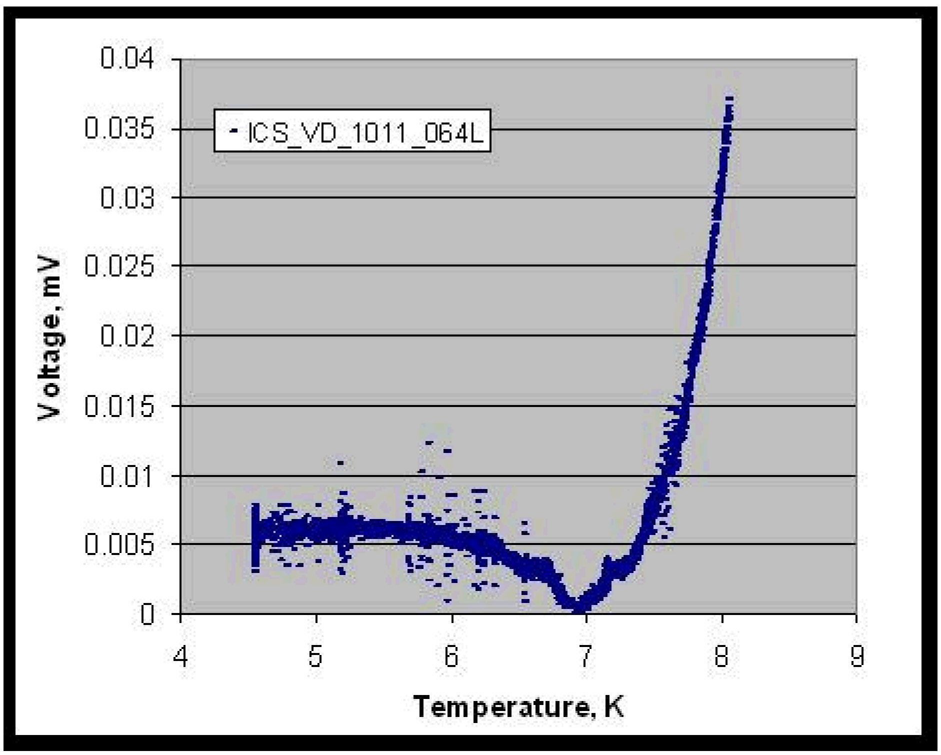


Fig. 5a. Negative voltage measured in Tcs measurements between voltage taps 10 and 11 (1.25m apart) - near the center of the CS Insert, indicating effect of global current redistributing

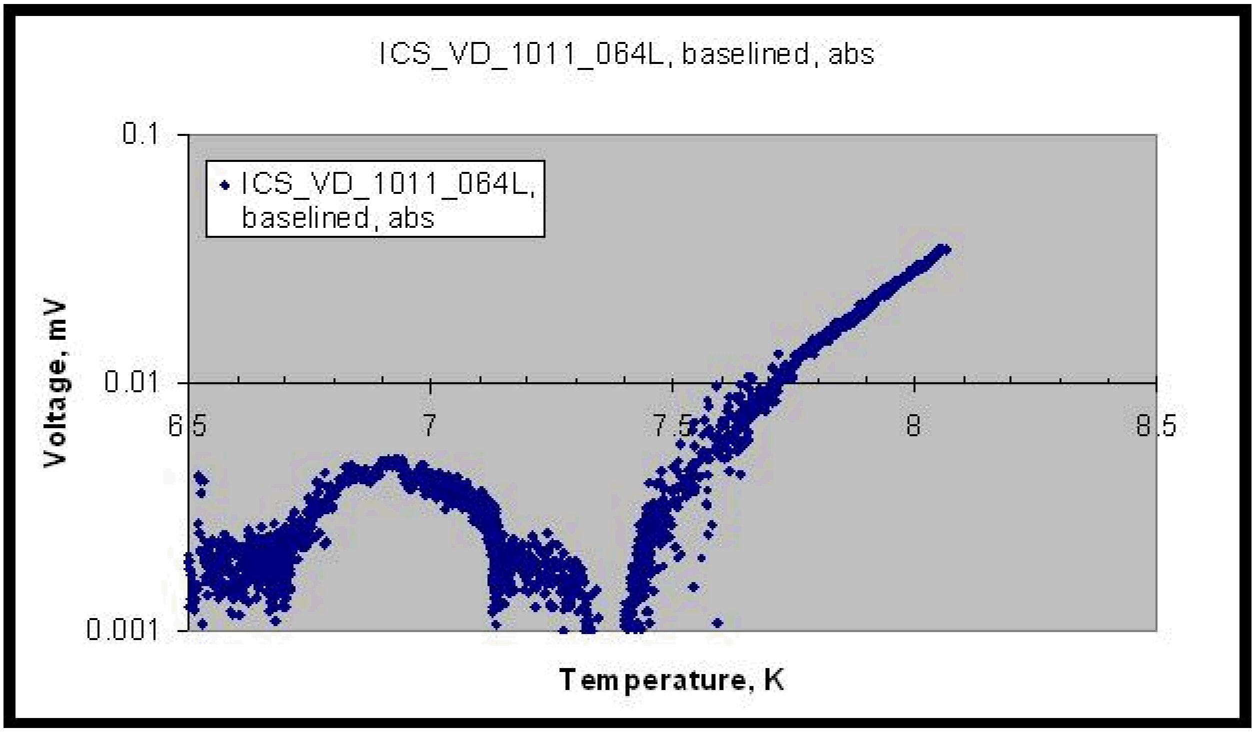


Fig. 5b. Same plot as above in semi-log coordinates. I had to take absolute value to use a semi-log coordinate. After the voltage reaches 5 microVolts, the VTC is more or less straight in the semi-log coordinates.

Although this exponential voltage growth versus temperature or current in the Nb3Sn CICC is not exactly an established law of nature, it is logical to me to expect that the TFAS should follow the pattern.

Also, in Fig. 3 one can see that the electrical field of the criterion 10 uV/m is on the “established” part of the exponential growth. The same is true for Fig. 3, the voltage tap span is about 0.4 m, so the Ic criterion is at 4 uV.

As one can see from Fig. 2, I could not fit the transition with one exponent. It is especially clearly see in semi-logarithmic coordinates (Fig. 6).

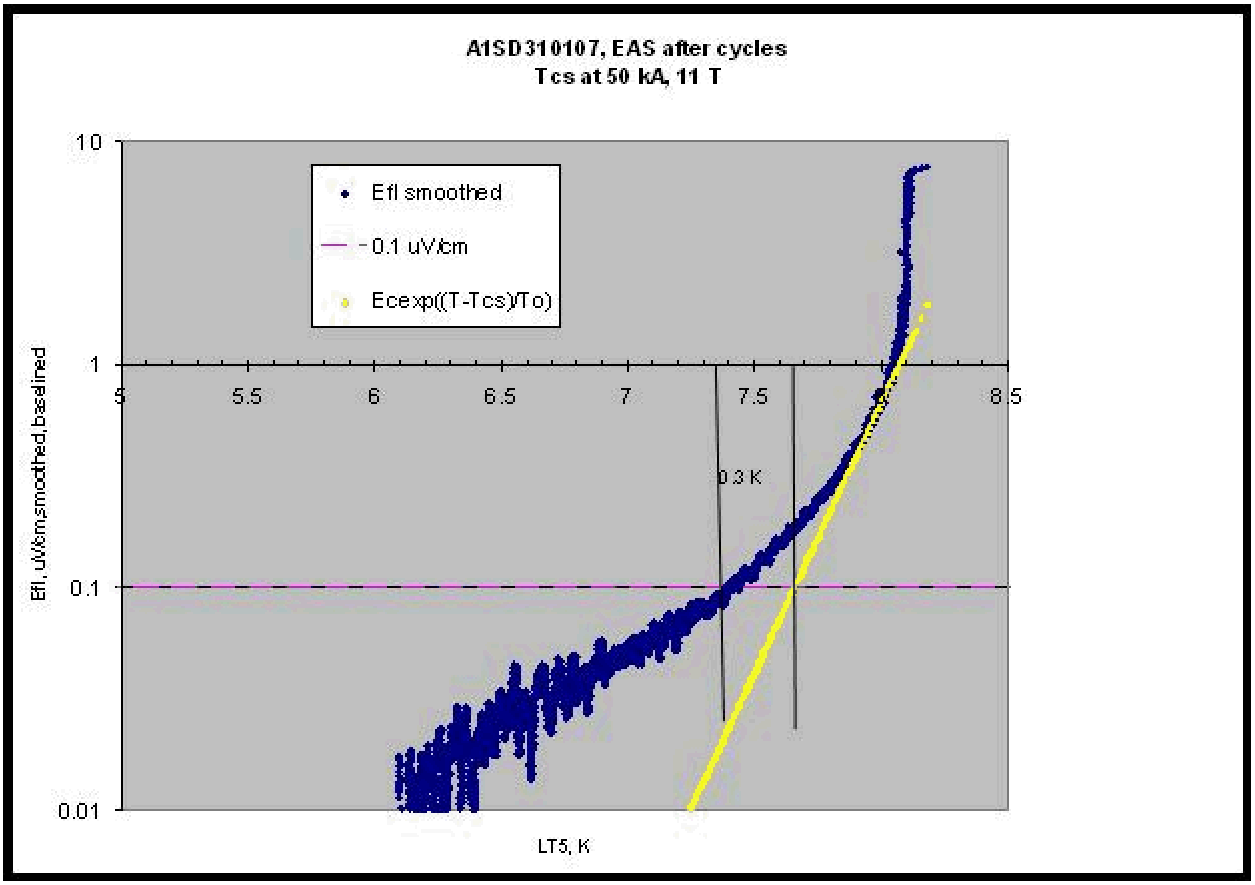


Fig. 6 a. Same as Fig.2 in semi-log scale.

As we can see, it is impossible to describe the transition with one To (or N). The part of the curve where the voltage crosses the 0.1 uV/cm criterion has a very slow growth (low N, large To figuratively speaking, since it is not exponential).

Which part of VTC do we do expect to be exponential? Close to the quench we expect heating to be small, up until say about 1-2 uV/cm if the temperature distribution is uniform over the cross section. But in reality, the voltage above 1 uV/cm is not stable (Boris Stepanov, private communication)– run off is inevitable even if there is no change in heat input anymore. Therefore somewhere below 1 uV/cm there should be a “real transition”.

So, that gives a basis for a speculation, that at a lower voltages the signal is masked by the current redistribution from the joints and when the voltage becomes large enough, this signal from the joints is overwhelmed and real transition is observed.

Following this logic, let’s see, what is the difference in the Tcs between those two approaches. Fig. 6a shows it is about 0.3 K. Not very large, but not insignificant either.

A possible explanation for the observed transition is: at low voltages it is significant distortion of the voltage due to current redistribution due to different resistances between the strands and the terminal, at high voltage, the thermal run away is taking place due to accumulated heat.

As we see the point which we consider a “real characteristic” is critical to guess which part of the curve could be used to judge the real performance.

Let’s check at what level of voltage heat generation is enough to cause runaway:

Let’s take a look at the outlet temperature, see Fig. 6 b.

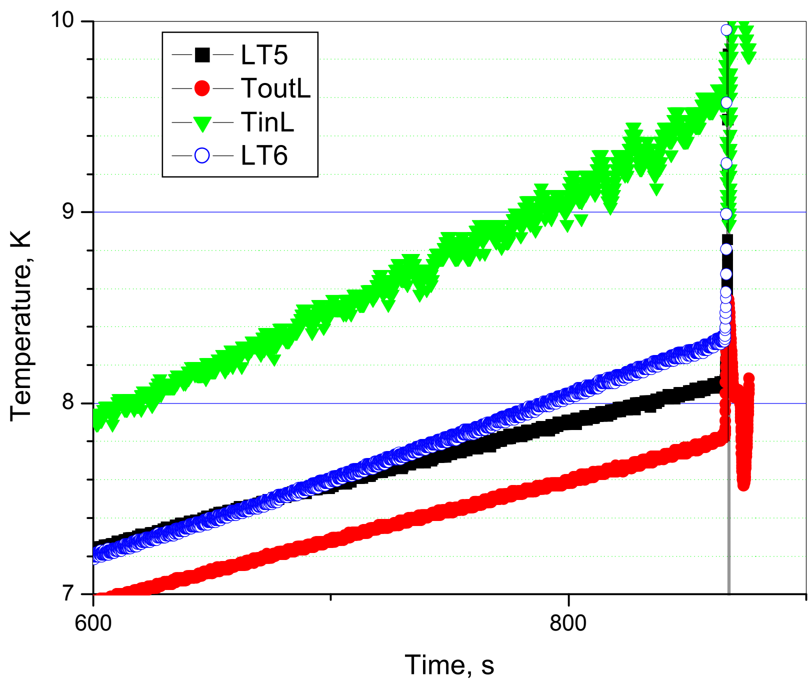


Fig. 6b. Temperature traces of the same TFAS1 shot A1SD310107.

The LT 5 is upstream from the high field region, the LT6 is downstream, very close to the joints. I see no indication of the self accumulated heat at the outlet, no indication on the LT6 until quench. The LT6 and RT6 are considered suspect, I agree with that. The slope of the LT6 is slightly different from LT5, but one would expect it to be nonlinear when the voltage is nonlinear. The calorimetry between LT5 and LT6 does not work well. As a minimum, the outgoing temperature should give an indication of the accumulated Joule heat and accelerating growth. No such sign.

Why not? Does voltage grow too fast?

At 1 uV/m AND 45 cm base the voltage is 45 uV, heat generation is W=45e-6*50e3=2.25 W.

The overheating is dT=W/mCp=2.25/(4*8.25)=0.06 K. At quench the voltage is 3 times higher, 0.2 K and that should be noticeable on the outlet.

Absence of the temperature growth at outlet may have two reasons:

1) Temperature growth at the inlet and voltage growth is too fast, that quench occurs before outlet senses the heat generation – for that dT/dt should be slow

2) Transition to normal state is really sharp but masked by current distribution effect at lower N.

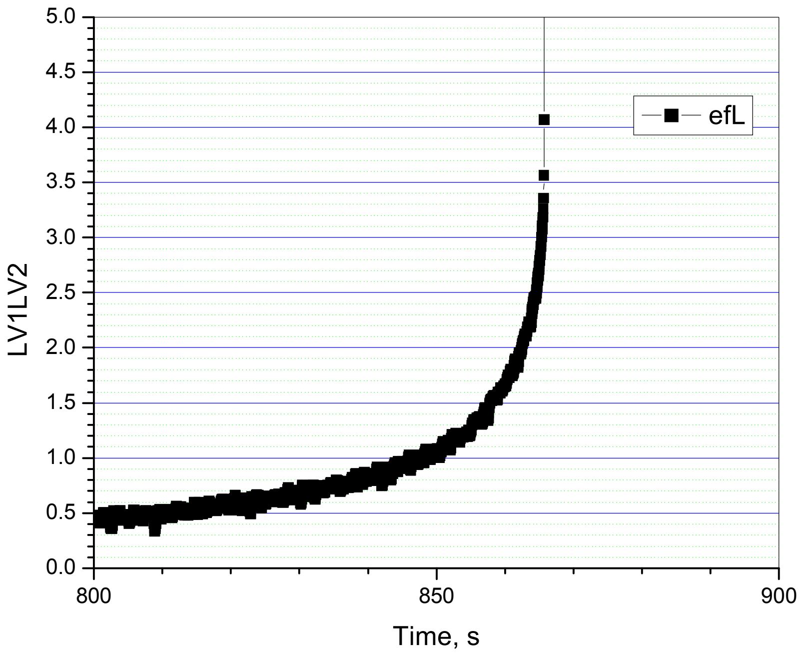


Fig. 6c Voltage growth in time. (voltage units are uV/cm), same run as in Fig. 6a and 6b.

This is how quick the voltage grew. At 1 uV/cm the overheating is small, if uniform in cross section, at 2-3 uV/m should be reliably detectable, but it is too fast – temperature sensor can not respond since helium does not have time to get to downstream sensors.

Growth from 1 uV/cm to 3 takes in the test only 15 s. Helium velocity is 0.08 m/s, for that time helium travels only 1 m and does not have time to show at outlet. Conclusion: go slowly, DC measurement should show equilibrium, it is desirable if you can go back and forth in temperature.

Compare with CS Insert measurements, shown in Fig. 7 as an example.

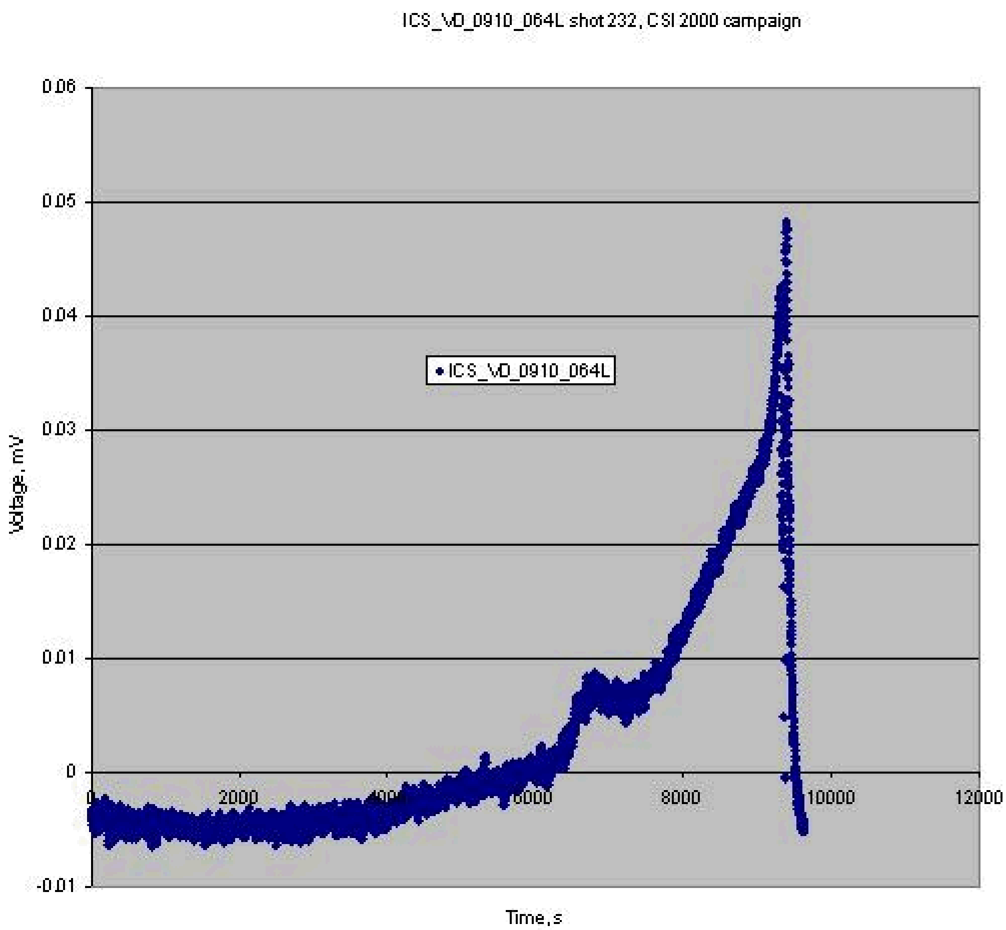


Fig. 7. An example of the VTC taken on the CS Insert. The voltage growth is going on in 3000-4000 s from detectable to quench, giving a good sense of equilibrium.

As we can see, a slow growth gives a better chance for equilibrium. Of course, SULTAN may have its own limitations, like limited flux in the transformer, but the point is to try reaching an equilibrium to make sure the VTC is real, not a dynamic trace.

Right OST leg in TFAS1

The EAS leg in the TFAS1 is considered more or less "normal". The OST leg is more "abnormal", the transition is very broad and the degradation is more pronounced.

Let us take a look at the VT transition at 50 kA. Fig. 8 and Fig. 9 shows

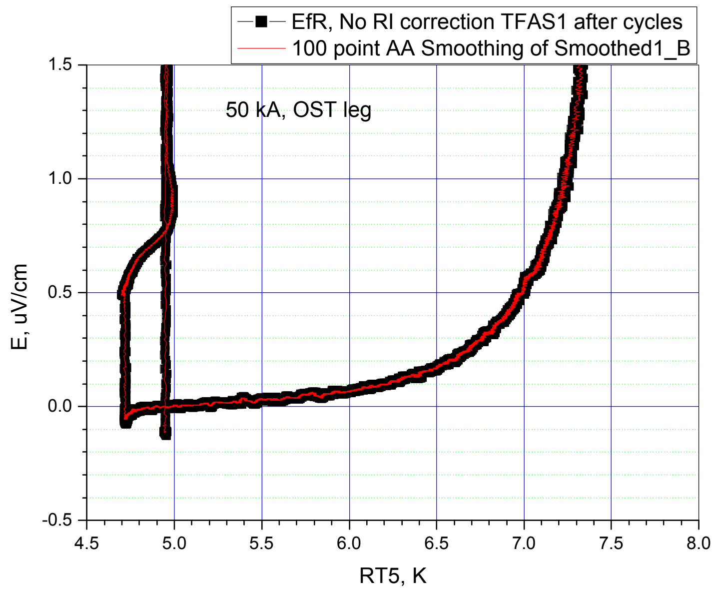


Fig. 8. VTC of the OST leg, file A!SD310106.

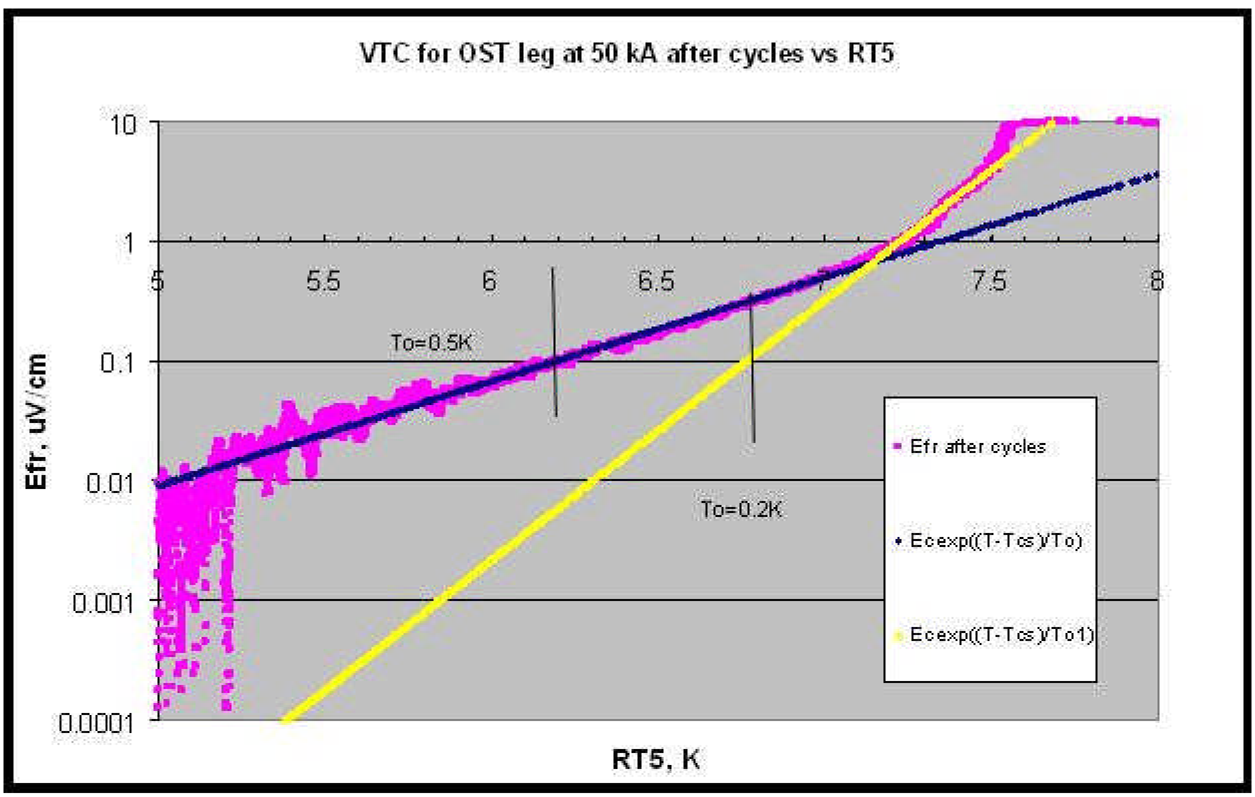


Fig. 9. The same as Fig. 8 in semi-log axes, the To for blue line is 0.5 K, fo yellow 0.2 K.

What if the real To is much smaller, what would be a correction? The yellow line in Fig. 9 is the approximation of the exponential transition in the area of higher electrical field, where typically (not guaranteed) the effect of joints and non uniform distribution is suppressed by the resistivity in the strands. In this case the correction would be about 0.6 K. That is a very large correction.

Let' take a look at the last measurement of the Tcs after cooldown and installation of the LT5a sensors.

Fig. 10 shows the VTC in semilog coordinates.

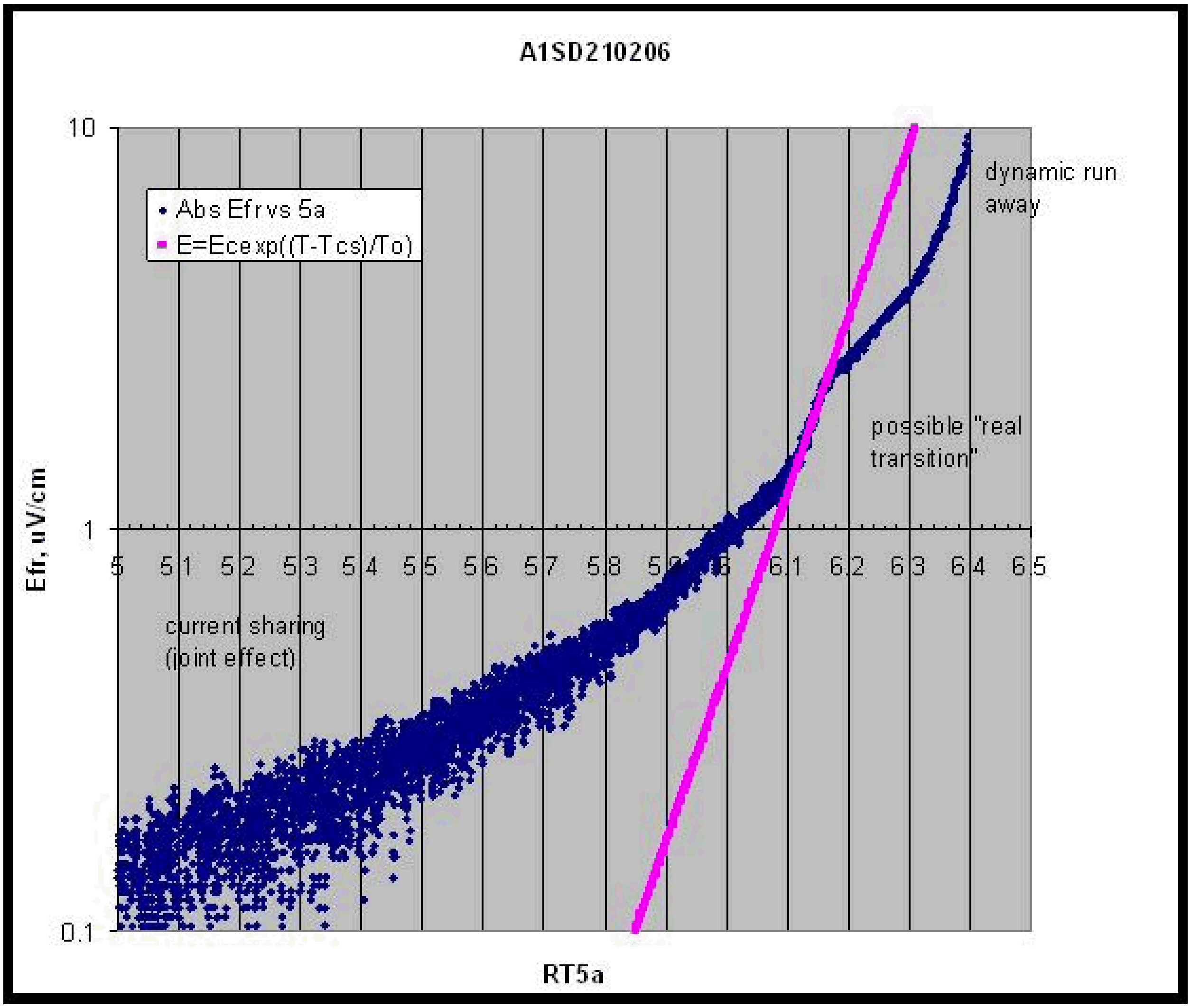


Fig. 10. Transition at the A1SD210206. My speculations of what different parts of the curve represent are shown as well.

If my speculations in Fig. 10 are correct, the "real" transition is about 5. 85 K; about 0.8 K better than what the measured transition shows if no additional processing is done (about 4.9-5 K at the face value, see Fig. 10).

Daniel showed the transition around 5.7 K at 11 T after the 3rd campaign (see Fig. 11). Pierluigi stated that after the cooldown the Tcs was 5.44 K [5].

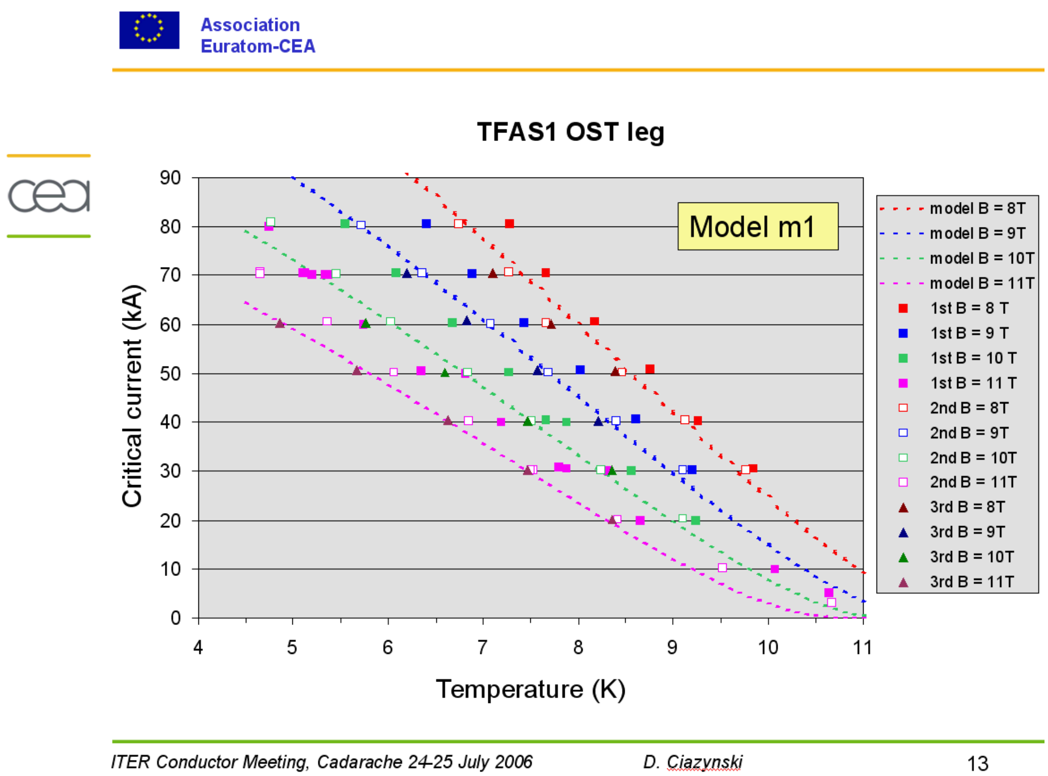


Fig. 11. D. Ciazynski summary of the OST tests

I am not sure why Daniel and Pierluigi have slightly different data, it may be the latest analysis (ASC06) refines the data since July, but looking at the raw data of Fig. 10 I can not see neither 5.44 nor 5.7 at 50 kA and 0.1 uV/cm. I'd say it extrapolates to 4.9 K or so. Daniel showed another slide for the OST and other conductors taking into account IxB and other factors to produce the following slide (Fig. 12). I can not exactly reproduce his calculation, but if my speculations are correct, the Tcs for OST would be better by may be up to 0.8 K (judging by how much the improvement is in Fig. 10) and that would make the OST acceptable for the TF operation.

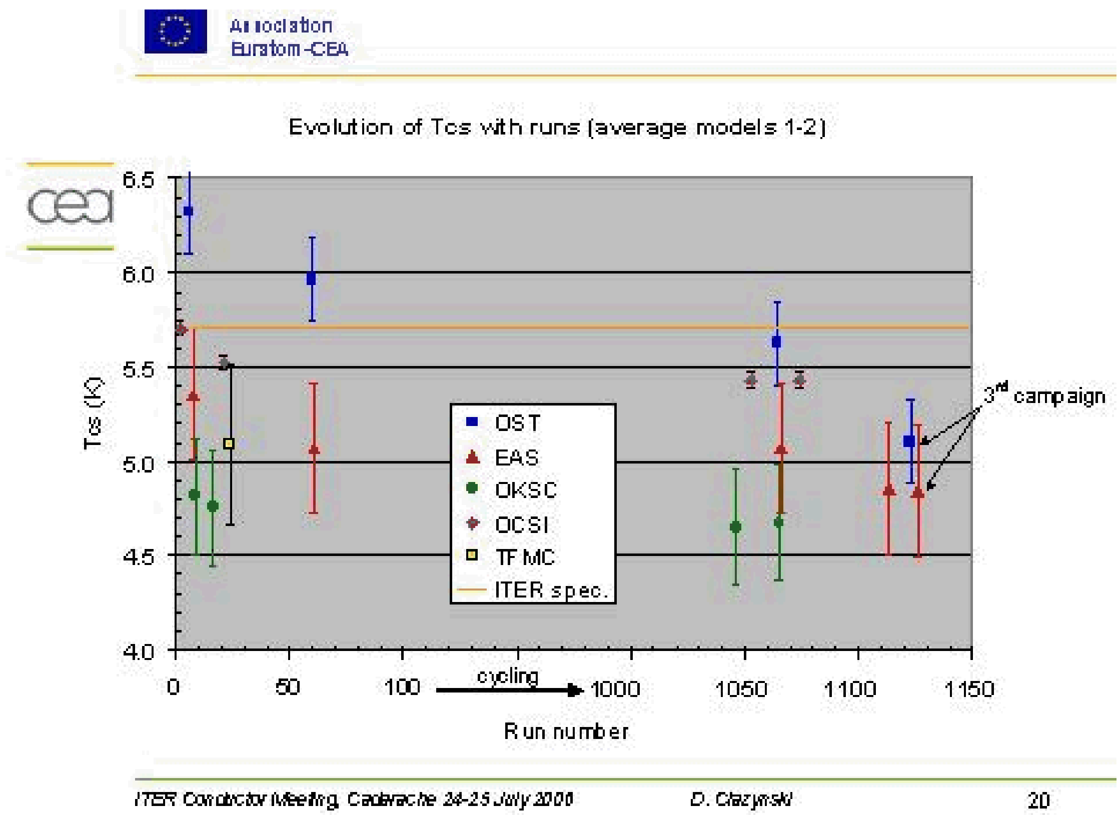
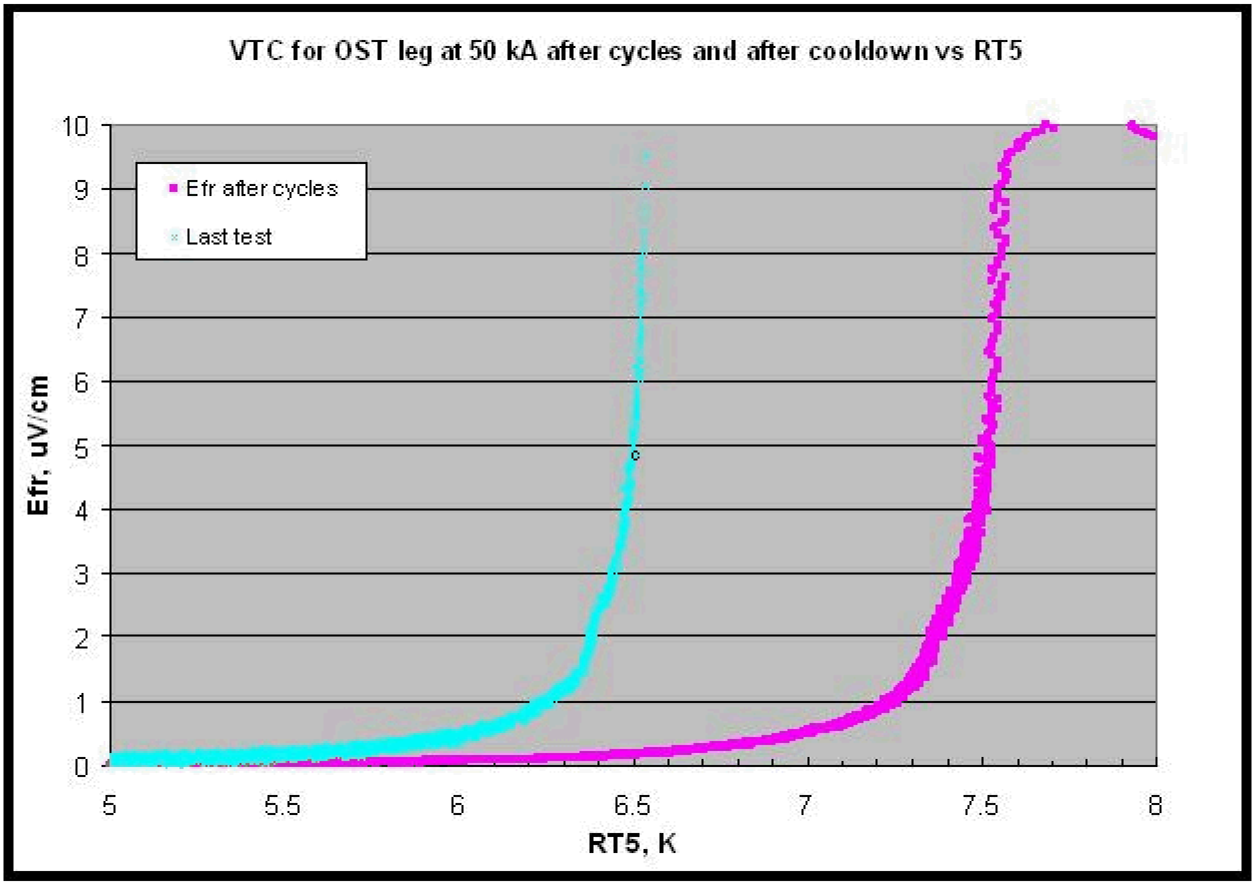


Fig. 12. Assessment of Tcs in TFAS1 and TFAS2 tests

Is there a chance that the current redistribution invalidates observed strong degradation of properties due to cycling?

Fig. 13. Shows performance of the OST conductor after the cycles on 31/01/06 and after the warm up and installation of the LT5a sensor.


VTC for OST leg at 50 kA after cycles and after cooldown vs RT5
Efr after cycles
Last test
Efr, uV/cm
RT5, K
0
1
2
3
4
5
6
7
8
9
10
5
5.5
6
6.5
7
7.5
8

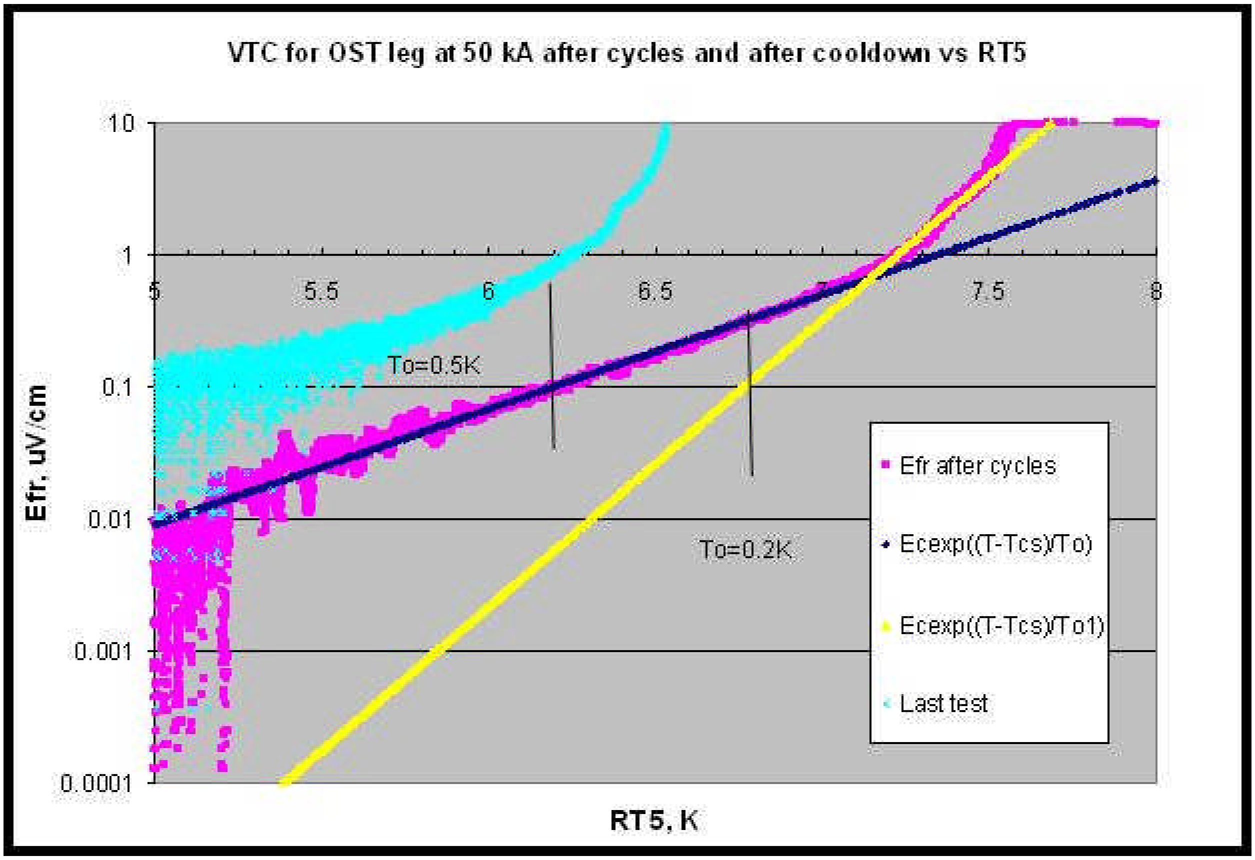


Fig. 13 a and b. VTC before and after the last cooldown for OST leg of TFAS1 (linear and semilog scale).

The previous SULTAN samples, at least some of them like SULSAM and SECRET A, showed that the critical parameters (Ic and Tcs) degraded significantly with cycles, but the quench parameters did not change that much. The reason for it was partially explained by the fact that the N-factor gradual degradation allowed higher take off voltage and that compensated partially or almost completely the fact that the quench did not change as strong as Tcs and Ic [6, 7].

In the case of TFAS1 the situation is quite different. The N-factor was not changing that much but the run away Tto or Ito dropped significantly despite quite low N. This picture is similar to what we observed on the CS Insert, but in the Insert case, the take off is limited by accumulated heat on a quite long length where the voltage is generated. At SULTAN, so far even when the degradation of Tcs (or Ic) was obvious, the take off parameters did not change.

Can it be that the cyclic degradation is caused exclusively by joints and non-uniform current distribution, without degradation of the strand properties in the high field region? I think it is unlikely, hard to imagine what could go wrong with the joints to cause such degradation, the joints do not experience much of a load. However, it is possible that the joints caused a premature runaway to begin with. In other words, even before cycles, some strands (or subcables) had too high resistance to receive the same current as the other subcables at the run away voltage. Of course, I can not prove it with the existing data, it is a pure speculation, based on the unusual behavior of run away current versus cycles in comparison with most tested samples at SULTAN where such degradation was observed.

What do we do with less than perfect transitions any way to signal process to deduce the "real performance"?

The next question I'd like to discuss is what do we do if the signals we measure on the SULTAN tests are highly unusual. Can we signal process it somehow to make sense and credibility, e.g. by subtracting noise?

I think besides the baselining of the voltage due to parasitic signal of the amplifiers and simple filtering due to AC noise, any other signal processing is not well substantiated. In the area of small voltges it is not reliable.

At the meeting in Frascati, D. Ciazynski [8] discussed proposal for compensation of a parasitic resistive and inductive signals. Although it may have sense at the first glance, it is impossible to prove that this is the right way to do it. There is no solid physical basis that the resistive signals are always linear with current and that the inductive signal is a simple. One can easily built a model of parallel strands with different resistances in between them and the terminal and obtain infinite number of possibilities of how this "resistive" component will look like, let alone how legitimate is subtracting an "Ohmic" term, especially negative. And they will not be necessarily linear. Since we do not know what exactly the noise function is, there is no way to make a correct subtraction.

The only way to proceed is to get to conditions where those signals are negligible. Since the SULTAN facility was capable of producing low noise signals on the other conductors, it suggests that the sample preparation, particularly the joints have something to do with that.

Between two alternatives – develop a smart signal processing which would give a reasonable curve or make all efforts to eliminate parasitic signals , I'd definitely choose the latter. How far we should go with that approach?

If in doubt, shall we reject the sample overall that shows unusual signal? I'd say, if after baselining the conductor shows that it passes 10 uV/m ITER criteria, it is acceptable. If at 10 uV the Tcs is too low, but the take off temperature Tto is significantly higher, there is a hope that it is a nonuniform current distribution effect, then the joints must be improved and the sample re-measured.

If the take off temperature is too low to meet the ITER criteria for the Tcs, the chances are that the conductor is deficient. Although even then there is a chance, that it was joint preparation which caused a problem, but it requires a gross problem in the joint. As we see from this, it is difficult to find a clear criteria in such a gray area. It is appealing to think that we can define acceptance criteria in the take off parameters, but it has its inconveniences. Ideally we would like to reproduce the CICC behavior in SULTAN facility, but it is not possible. At 10 uV/m the Tcs and Ic should be more or less identical for a SULTAN sample and a magnet, providing the same strain, providing uniform current distribution in both cases, which may be problematic. For take off parameters, the nonuniform distribution is suppressed. But if such a conductor were used in a magnet, the take off temperature Tto in the magnet would be different (lower) from what was measured in SULTAN if there is no hoop stress. It is lower in the magnet due to accumulated heat and translation of the SULTAN results into the TF prediction is not straightforward, but possible. Note that in this memo we always talk about temperature before the electrical field generating length, in a sense, inlet temperature, when talking about comparison of the SULTAN magnet to a TF coil.

What are the criteria of a representative conductor?

When we stated that the TF conductors qualification critical step is meeting ITER requirements in SULTAN tests we postulated that it should show that at 68 kA and 11.8 T peak field, and 5.7 K before high field zone it should have an electrical field less than 10 uV/m (my understanding).

SULTAN has not become an ITER qualifying machine yet in a sense of a stringent procedures for sample preparation and result processing, it is still an ITER R&D device where every sample has its features treated on the case-by-case basis. It is not that there is no will or ability to do it, it is just we get more frequently surprised what SULTAN tests reveal than otherwise. Also, such a qualification was not implemented by ITER anywhere before. With time, after several TF samples tested in SULTAN the acceptance criteria will become more formalized. We did not particularly standardize how exactly we process the voltage signals or how to compensate the noise or shall we assume any credit for hoop strain, which is present in the TF and especially in the CS but is absent in the SULTAN.

I read with intrest a set of comments by Phil Michael [8] and the corresponding comments by Pierluigi Bruzzone [9]. It shows to me how difficult it is to formulate quantitatively these criteria. I'll try to give my comments.

Indeed, if a SULTAN sample meets the criteria of 10 uV/m. we are happy, even if there are signs of nonuniform current distribution. I'd also say that if the sample shows a clean horizontal voltage at below 3-5 uV in the high field area, it has a good probability of a good sample and measurement, but of course it is not guaranteed. Some strands could have higher resistance, then the horizontal run is irrelevant. But if there is no horizontal baseline and the transition is unusually broad, the joints are suspect, unless the whole sample is broken from the start.

If the criteria of 10 uV/m is not met, but the Tto at the TF current (68 kA) is significantly higher than the Tcs required (5.7 K), then we are in the grey area: there is a suspicion that the sample is not prepared well and we need to decide how to improve the sample to make it qualified. Especially encouraging would be signs of a sharp exponential growth of the voltage at 0.5-1 uV/cm. But I do not think that attachment of more voltage taps and/or temperature sensors will give a quantitative answer.

I can not imagine that there is a defendable formalism in the signal processing, (other than baselining and filtering the voltage) that can compensate nonuniform current distribution or inductive noise. In other words, it is not possible to compensate bad sample with good signal processing.

I think such a sample must be rejected, fixed or redone and re-measured until signs of a bad current distribution are small. What is the criteria? An exponential voltage versus T and I at 1 uV/cm or so. In principle I'd trust the level of 1 uV/cm much better than 0.1 uV/cm for demonstration of the "real" conductor performance, but the trouble is that this high level of electrical field is not sustainable in the TF or CS, so one has to gamble how much projection from the high level of voltage to 10 uV/m is more reliable than the direct measurements in the SULTAN. I think we have all reasons to expect that, but strictly speaking, there is no hard and confirmed evidence that it is true.

Theoretically, bad joints can cause premature quench even if the joint resistance is relatively low. But there is no clear and verified way to see from the voltage traces if some of the strands or subcables have higher resistance to the terminal than the other strands. Since we saw on the successful samples that the effect of the joints in SULTAN could be low above 0.3-0.5 uV/cm, that would be a criterion for a good joint not necessarily in the sense of the low resistance, but in the sense of the uniform current distribution, which is obviously not the same.

In his discussion Pierluigi raises a good point [10] if performance of the conductor is much worse than expected why is it so hard to accept that it is just a bad conductor, not facility or joints? From what we know I do not see how to convincingly dismiss that. If feasible, I'd suggest to improve the joints with solder filling, but I am not sure if such a process is possible without significant risk of breaking the sample.

I agree with the majority that the cycles degrade the conductor, not the joints. But even then, it is arguable how bad it is. As it appears from the OST VTC, the difference may quite significant. Even 5.44 K Tcs at ITER specified non-copper jc [5] the take off temperature is 6.6 K, I'd expect that until about 6.3 K (taking into account longer length in the field in the TF in comparison with SULTAN) the ITER TF system can operate safely. Then if the peak helium temperature in the channels does not exceed 5.0 K in operation, the actual temperature margin is a healthy 1.3 K. Not bad. If the TFAS1 OST sample measured in SULTAN would be as expected, N=7 and Tcs=5.7 K at 10 uV/m, we'd expect the take off about 0.7-0.8 K above the Tcs, which makes it 6.4-6.5 K. Not that far off from what we would project for the TF based on the SULTAN TFAS1. So, in the sense, a low N in OST, regardless if caused by the joints or real strand degradation extends the quench and real temperature margin (defined as temperature of take off minus maximum operating temperature). The real temperature margin is not that different from a sharper expected transition. I do not want to bring hoop stress here at this point, although it is supposed to help.

My observations that things are may be not as bad as they look. That does not mean that we should stop our effort to find a better cabling pattern or some other innovations, like reinforced strand or something like that. If my suspicion is correct it makes the projection of the ITER TF performance a little brighter. Since we do not have much time, we better find something useful really fast.

As one can see, my speculations are based on the expectations that the VTC or VAC transitions are more or less straight lines in the semilog coordinates. Is this a proven fact? Unfortunately not. There is no hard proof that it is always a case with Nb3Sn. It is just all ITER relevant tests I analyzed so far, when there was no doubt that the nonuniform current distribution is suppressed, seem to follow this pattern. Pierluigi thinks that a broken strand may have any shape of transition (private communication). May be, I did not see an evidence, but I agree, it is conceivable to break a strand in a way that the VTC or VAC are not linear in semi-log coordinates anymore. At the ASC, I noticed in one of the poster, that one of the coated wires, which had a perfect linear transition in semi-log coordinates changed the transition to something irregular after it experienced a violent boiling in LN2 Presumably bubbles of nitrogen damaged the flakes of the superconductor. But for the Nb3Sn CICC I did not see much of a deviation from an exponent. For the individual strands that were bent by a regular array of pins [11] the UT group reported the shapes of the transitions, see Fig. 14 below. Indeed, there is some distortion, but not very dramatic, like in our case To or N varies almost by a factor of 3 on the same curve.

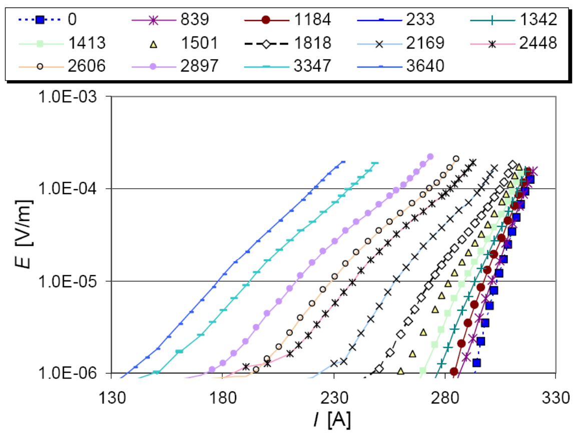


Fig. 5. Electric field versus current for a wavelength of 7.2 mm (*F* in N/m in the legend).

Fig. 14. Reproduction of the plot from [11]

Conclusions and recommendations:

1) For SULTAN sample preparation make resistance between strands in the joint low (solder). We agreed on this long ago, even for the joints, which showed good performance at the TFMC and the CSMC. Low joint resistance is not a guarantee for good current uniformity. There needs to be low resistance between the strands, not necessarily to the terminal.

2) Go slow in the voltage growth region, allow equilibrium to settle and helium to travel to the outlet sensors. If possible, go up and down with voltage measuring VTC and VAC.

3) Do not use unsubstantiated assumptions on resistive and inductive noise – these signals are not predictable. Only baselining and filtering is acceptable as signal processing. At this point it seems that smart processing can not compensate for bad joints or sample.

4) If ITER criteria are met and the safety margin is sufficient, it's OK to accept the sample even with signs of nonuniform distribution. If criteria are not met, the sample should be redone to fulfill the exponential growth and horizontal run requirement.

5) Bad joints can not explain degradation due to cycles, it is unlikely the joints themselves are affected by cycling. But bad joints can affect and decrease Ic and take off currents both before and after cycling.

Unfortunately I could not find very strict criteria upfront to say which tests are good and which are not, as was the prime purpose of this study. We still need more data on the TF samples to be measured at SULTAN to see if we can consistently lower the effect of joints below 10 uV/m level. If not, we should think about selecting a higher voltage criteria and extrapolation down to 10 uV/m in SULTAN, but than the TF Insert becomes absolutely essential.

I think there is a hope that the TF samples made by the parties may turn up acceptable for the TF use if we make good joints and go slower.

Even if my speculations are correct and some TFAS samples would pass the ITER criteria, the cyclic degradation is obvious and until too late we should not stop efforts to find a better configuration for the cable or strands in order to reduce strain sensitivity.

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