

Date: November 11, 1999

From: Hubert Becker <Hubert.Becker@cern.ch>

To: Mike Tyndel <m.tyndel@rl.ac.uk>

CC: atlas sct<atlas-sct@atlas-lb.cern.ch>, Peter Jenni<Peter.Jenni@cern.ch>, Volker Soergel<soergel@mppmu.mpg.de>, Steinar Stapnes<stapnes@fys.uio.no>, Mike Price<Mike.Price@cern.ch>, Leonardo Rossi<Leonardo.Rossi@cern.ch>, Daniel Froidevaux<Daniel.Froidevaux@cern.ch>

Subject: Forward Cooling Review

Dear Mike,

THE ARROGANCE OF SOME SCT RESPONSIBLES HAS GOT A NEW DIMENSION!

In the past, outcome of reviews, even of LHCC, were just ignored and corresponding proposals were not considered or even not allowed to be presented.

You probably remember my former complaints. I did not understand why you never checked the content, you only responded once: "You failed to convince your colleagues".

Now it became much worse!

Reviews are organised to manipulate the outcome. To enforce selfish interests people are not even frightened to cheat.

I was now waiting for nearly four weeks since the Forward Cooling Review was held. My anger about that farce of a review has not cooled down since, but you can be sure my letter is not a 'stupid outburst' as you like to call it, but a well overthought reaction.

Clear words are needed now!

I am aware that this is a serious reproach and I have to deliver detailed facts for verification.

THE HISTORICAL BACKGROUND:

I was asked by H.G. Moser at May 1997 (I was on a sabbatical) to review the Forward Tracker, because MPI was at that time still considering to join SCT. Maybe I should mention, that I designed modules and mechanics incl. cooling for the ALEPH Vertex Detector.

Especially the LHCC request needed attention: "The referees especially emphasise, that all areas of the detector, both in the barrel and in the forward region be scrutinised for possible material savings".

At first I proposed (presented at Santa Cruz July 97) a module design with separation of the heat arising at the electronics (=hybrid) and Si-sensors by a complete gap, and to replace the BeO-spine, which should give mechanical support and enable to cool the sensors, by a tongue of TPG (heat transfer eight times better!). By that the thermal properties are improved and material is saved, because the stability is given by the rigid Silicon itself, similar to the ALEPH and DELPHI module designs.

Another LHCC request was: "Evaporative cooling to be pushed more aggressively, as this option offers potential material savings".

No serious attempt was made to follow this. Only when it became clear for other -simple!- reasons, that 'binary ice' could not be taken, was evaporative cooling considered together with some even more massive monophasic coolants. Again not the most powerful candidate for evaporative, C3F8, was considered,

but several others. C3F8 was proposed by T.Niinikoski already in 1997, but his detailed calculations were ignored or not believed. Therefore no sufficient measurements were done by the ID cooling group. They concentrated on mixtures up to July 99, not admitting at Inner Detector Cooling Review May 99, that

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this mixtures do not work. The outcome of this review was manipulated by that! So the recommendation for the choice of coolant was: "C3F8, C4F10 mixtures look promising". I pointed that out at June 99 SCT week -no reaction.

In summer 98 I was working together with TN on a proposal to adapt his predicted C3F8 properties (low coolant temperature available and high heat transfer coefficient HTC) to reach essential material savings.

At those low temperatures the sensors will deliver only a very low heat power, even after ten years exposure to high radiation.

So our basic concept is to keep the temperature of the sensors low and to cool the hybrid independently with a 'fully split' cooling block. A second cooling block with corresponding pipework for the sensors could then be avoided, not only reducing material but also complexity (sliding grease contacts, cabling etc).

In addition, with this proposal the hermiticity problem, which was brought up at that time could be solved.

Unfortunately I was not allowed to talk about that at the SCT week in Nov 98. Instead of that it was decided to add another inner module ring, which meant increasing material in a sensitive region by 2% with additional costs of about 1 Mio SFr. A.Clark and A.Poppleton proved the nonsense of that decision in detail, which was finally -after long fights- annulled.

I also found out, and demonstrated with simple calculations, that an additional cooling block for the sensors even deteriorates the cooling if the other block is not fully split as foreseen in the 'baseline', because then the sensors are continuously heated up by the hybrid.

Moreover, when in February 99, it was announced that the hybrid power could increase to 7.2W (before 4.5W), I again proved, that with the baseline design only the outer but not the middle and inner ring modules could work.

This was admitted and led to the conclusion, to consider C3F8 as a possible coolant and by lowering the baseline coolant temperature of -15C to a range of -15C to -25C. (Now it is again changed from -17C to -25C!)

However the proposed material savings were still not considered. On the contrary, to adapt the old baseline (from TDR on binary ice), more mass was added to the blocks to compensate for poor thermal features. So the "mass" (XO) of the latest RAL design was more than 3 times higher than at our proposal! This corresponds to an additional 'ghost-disc' of 4mm thick Aluminium. On the other hand one attempts to reduce thin glue layers by microns to save material!

All trials to convince the responsables of RAL of the advantages of our concept just failed: they insisted on keeping the baseline. As a consequence we were not allowed to do thorough measurements in the cooling lab to prove calculations and FEA. They even prevented that! You, as a member of RAL, were fully aware of it.

Being aware, that our intelligent proposal has benefits to SCT in general, we did not give up. So instead of finding the best solution by working together it came to competition.

To bring this Situation to an end, a Forward Cooling Review was organised. But the date, the reviewers -especially the chair- and the given boundary

conditions indicated clearly, that no real evaluation was intended. I expressed my complaints to M.Turala, the letter is attached.

WHAT HAPPENED THEN?

1. No helpful reactions came up; my letter to you is attached, too.

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2. At Sept 99 SCT week only 15 min were allowed for a presentation. This was just enough time to point out that an immense work was made to envisage a material saving of more than 20% in the foreword region; details could not be given.

3. D.Greenfield (RAL) insisted to split our proposal in single items. I was foreseen to talk at the review about such an item. I pointed out in a letter that this is nonsense (copies to SCT).

4. The consequence of these complaints was that I was cancelled completely from the speakers list.

5. So speakers defending the baseline were guessing, what could be our proposal. S. Temple explained the RAL baseline design with new blocks having sufficient thermal properties, verified by FEA, but being quite massive. He also delivered mass calculations for an old block design, done for the 4.5W scenario, with insufficient thermal properties but less massive. Comparison of this with the "guess" of our design showed only small mass differences.

In his written summary the chair, HG. Moser, declared this design simply to be the baseline! I would call that manipulation.

6. S. Snow presented FEA calculations showing that one cooling block is not sufficient, just in contradiction to our calculations and measurements. This FEA calculations were given to him and other speakers by the chair, HG. Moser. But, as I found out meanwhile, this was based on completely wrong assumptions: our proposal is based on C3F8, but HTC of C4F10 was taken, being three times smaller than the measured values of C3F8. For the baseline, which was for a long time based on C4F10, numbers of C3F8 were taken! HGM was asked at the presentation about this and he answered: "latest measured values taken". He and some of the reviewers are aware of the correct values; one had reported about measurements with HTC much higher than ours.

Due to that, falsified information was presented to the reviewers and most of them had no objections. As a consequence, the chair, being the expert, came to "his" result and he began the summary:

"There was an interesting alternative, very good to help us to overthink the present design". He continued, how excellent the baseline is. This arrogance cannot be surpassed!

A procedure, to deliver falsified information for the purpose to get a project passed with immense consequences in cost and production time would be called in normal life "cheat". I do not know whether there exists another expression for that in the scientific world.

I was right with my suspicions expressed in my letter to M. Turala, which you declared to be a "stupid outburst"! But I could not imagine such an insolence.

Now the question must come up:

WHAT IS THE REASON FOR SUCH A BEHAVIOUR?

HGM in his summary: "The alternative is not engineered in sufficient details to be considered. Concern about time to do that. Recommendation to work on the baseline".

I claim, this is completely wrong:

The baseline is much further behind because of its complexity. Many items are not yet studied:

- at "wiggly" pipes an inhomogeneous coolant flux is expected with different (poor) cooling properties
- thin Al-pipes are not reliable (leaks, corrosion, bending, connections)
- cooling of middle and inner ring modules is not solved yet.

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This could lead again to show-stoppers, which will undoubtedly be overcome by adding more and more material.

Furthermore, production time will be much longer.

Clearly our proposal needs some R&D too, but its simplicity allows to mark off the problems. No serious argument was brought up till now.

What is then the real reason?

Our proposal implicates small modifications to the module design. A boundary condition for the review was: no changes allowed (SSG decision!). But it turned out that the baseline also needs changes now.

What is meant by "small changes"?

Modifications cannot be done any more to the sensor layout, because sensors are already procured. Serious ones should not be done to the hybrid layout, because large effort was put in that. We do not need to do that.

But there is the "spine", a relic of the TDR. All the forward assembly sites complain about its fragility and everybody admits its bad thermal properties. As I pointed out already, with a so-called "box design", which I developed at MPI as far back as in 1997, this spine is not necessary at all. It can be replaced by a TPG-tongue and small spacers.

Several modules were built to prove its performance.

At a module design review at MPI in May 98 it came to a shoot-out and this design was the clear winner. But of course that did not mean that it should be taken: the UK groups insisted on a spine.

HGM retracted the "MPI-design", as he did some time before with a superb assembly method, matching perfectly with that box-design.

The reason for both actions was that MPI did not want to push other proposals than their sensors with implanted resistors. This structure is patented privately and in case of being accepted by the community would offer enormous profit to some people.

After those decisions I left MPI and joined the CERN-Geneva group.

Another module in box design was built there with improved assembly jigs, demonstrating that this design is even simpler to build.

The spine was retained by the majority of the community, only modified from BeO to a lamination of TPG and AlN.

But this spine has another feature: it is very expensive!

It seems that this is the real reason to be kept: following the intention of HGM this spine will be fabricated "for less than 100US\$ per piece" in a special company. This means costs of about 375 kSFr compared to our proposal

of about 75 kSFr (mainly TPG).

Milling down AlN is expensive, but many firms will do this much more cheaply - if they were asked! There is no reason to waste so much money, even if there were 1070 kSFr foreseen for the TDR design with the BeO-spine.

The only conclusion can be, that here again selfish interests are playing a role. So the aim of the cooling review obviously was, highly forced by the chair, finally to get rid of a superior alternative.

But by that a great potential improvement of the Forward SCT, a reduction of more than 20% material would be prevented. This is an item of ATLAS in total and cannot be decided by SCT alone.

All of us have the duty to do the best for the huge amount of money being involved. The LHCC recommendation of 1997: "Push p+n work. Money saved could be invested to save material", is just converted into a parody.

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Therefore I would call that behaviour much more than cheating. It sabotages the excellent work of many people and therefore it is uncolleagual! Some of the SCT management protecting this behaviour are also guilty - their behaviour is shameful too and irresponsible!

There are more scandals similar to that in the SCT!

HOW TO PROCEED?

Hopefully before the next SCT week we will present an ATLAS note with a detailed description of our cooling concept. Furthermore, we will put a request for measurements at the cooling lab. At the present time we are not even "back-up" and therefore not allowed to do measurements. Our setup was dismantled at the day of the cooling review!

More than a year has gone since we presented our proposal and because of that cooling review farce we are losing additional months. This has to be changed immediately!

Mike, I expect a detailed comment to this letter before Nov 24th. I am prepared to prove all my criticisms to suitable independent persons by documents. If your response does not fully satisfy me, I will insist that Peter Jenni transfers my complaints officially to the LHCC and also to highest CERN authorities. Even other actions are thinkable!

I will not accept that my complaints are played down again.

Hubert Becker

From: Hubert Becker [<mailto:Hubert.Becker@cern.ch>]

Sent: 21 September 1999 23:04

To: Michal Turala

Cc: Mike Tyndel; Steinar Stapnes; Peter Weilhammer; Allan Clark; Shaun

Roe; Tapio Niinikoski; Bob VanEijk; Mike Price; Peter Jenni; LHCC

Chairman J. Engelen

Subject: Re: SCT review on the End-cap cooling structures

Dear Michal,

One of the unsolved problems of SCT is certainly the cooling system for the forward modules. Neither the 'baseline' system developed by RAL nor the 'circum-split' we propose are in a satisfying state. This is a bottleneck for many other activities, and everybody agrees that we must have progress.

Unfortunately the attempt to reach some quick progress by this Cooling Review is illusory, because

1. Cooling the modules is not a big challenge, it only depends on the amount of material to be installed. A main concern of the LHCC was the overall mass, and they saw the only chance to reduce it with a better cooling system. We were looking for such a possibility and we believe that we found a very good solution which could lead to a reduction of total forward material up to about 20%! We further think that this would also be advantageous in many other respects.

2. To find a solution in general, a competition may help. But there will be winners and losers, and they have to collaborate afterwards - maybe then there are only losers. I by myself will not build the cooling system, but I believe I had some good ideas and I wanted to transfer them to the responsables.

Therefore I presented my ideas several times to the RAL people; the result was a useless pseudo-split block on a wiggly pipe. At SCT-weeks it was either not allowed to present my proposals to the community or they were just ignored. I was even accused in public that: "you always try to hijack the community!" For meetings concerning this matter, I was excluded. So I did some separate research together with the CERN/Geneva groups and also with NIKHEF resulting in the proposal which is now finally discussed. That's why we have this unfortunate situation now, and that's why I became annoyed.

3. Because the proposals I presented were not considered to be worth getting assistance to do measurements in the cooling lab, we could not prove our calculations for a long time. To work in this lab was necessary, because the proposal is based on the properties of C3F8. Only last week we got a few hours parasitic to the (non-working!) RAL system. Our cooling blocks have been available for a long time! To present a consistent cooling device we have to do more measurements, not to be done within two weeks.

4. I can not agree with the agenda of the review. As you know, Tapio is the father of our new cooling layout and he should first explain to the committee the fundamental principles when C3F8 is used. Not only the blocks and the pipe have to be reviewed, but the whole cooling path, which starts at chips and detectors. It is not enough to optimize single items. The module design is one of the weak parts! So the given boundary conditions should first be reviewed.

5. I can not agree that HG Moser is member of the review board and even has the chair! If necessary, I can tell you details why. We have many 'outstanding physicists' with good knowledge in this field but without prejudice. I have in my mind; N. Unno, G. Lutz, J. Ludwig and especially

Geoff Taylor.

I just asked HG, whether he would be willing to retract his participation, but he refused!

6. With the foreseen review board, three being close to RAL (DG, RN, GH soon- and UK internally always well organized!), the outcome is already clear to me:

"Some good ideas, but it's too late".

But the same was told to Bob van Eijk and Geoff Taylor two years ago, when they proposed cooling systems highly superior to the baseline. They both resigned finally and so did Tapio!

Sorry, that I am too impertinent not to do the same.

Therefore I would now strongly intercede, that it should be Geoff Taylor leading such a review board.

You are right, we are late, but to fight against a wrong decision coming from that board, would even delay it more!

In that context I have to remind you how difficult it was to revise the SSG decision of last year, to add another ring to disc 2 (80 modules+20 spares!), when I insisted that costs of about one million SFr and an increased material budget can be avoided just by using an improved cooling/fixation block.

7. If the SSC is not willing, to change the board and the agenda, I will propose to my colleagues not to lose more time for useless discussions, but continue doing good work and present this in an ATLAS Note.

Sorry again, Michal, to cause so much trouble.

We have been working together very close for many years and I claim we are good friends. But I would like to remind you, that we have a big responsibility to do our best for the huge amount of money we spend. It worries me, that we are in a situation now, where people rather look for majorities to support second class proposals than fighting for the best solution.

This is politics of the farmers in the EU, and it is always claimed, that we also have a function as a model for correct decisions.

Best regards

Hubert

Date: Thu, 23 Sep 1999 22:27:18 +0200 (METDST)

From: Hubert Becker <Hubert.Becker@cern.ch>

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Cc: Hubert Becker <hubert.becker@cern.ch>

Subject: My complaints about ATLAS-SCT procedure

Dear colleagues,

I would like to close the discussion on this subject by the following notes:

First of all I had some doubts to direct my message also to the uppermost authorities. But the two responses I got confirmed me, that this was the right thing to do:

1. Peter Jenni asked me immediately to show up.

If SCT responsables would have reacted in a similar way on my former complaints, maybe such an action could have been avoided.

2. Mike Tyndel responded as to see.

At first it was not a "stupid outburst", but the consequence of a long lasting bad situation.

Moreover he is wrong in that this will have an "extremly negative effect" on me: I am not obligated to work at CERN. I had some great time during the last 27 years, which I partially spend here. Anyhow I would not continue to work at SCT, if there are not essential changes.

He is hopefully wrong also in that this has "extremely negative effects on the SCT". Maybe it can help avoiding similar problems in the future.

Regards,

Hubert Becker