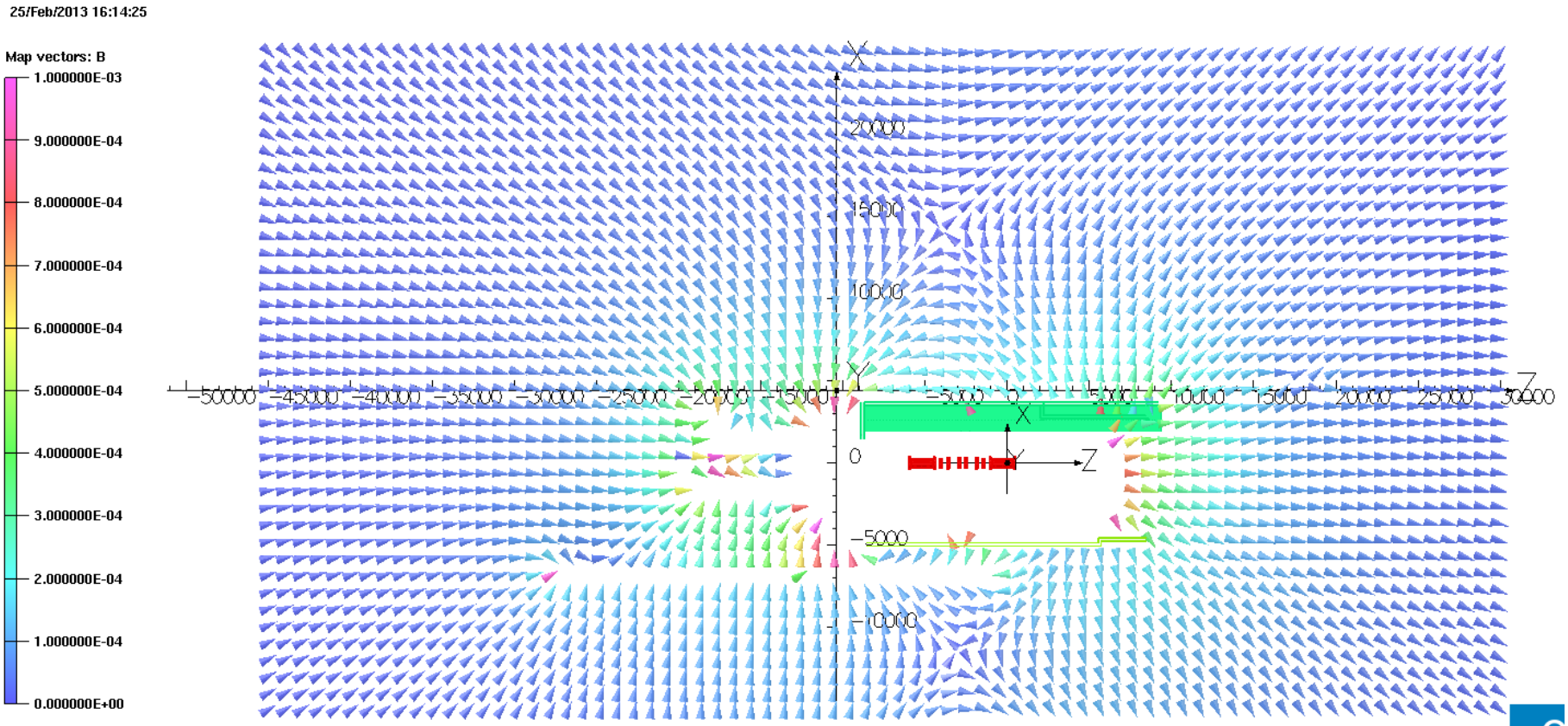
Model Update – 27/02/2013

This week I have been undertaking some investigations of the Hall Model at around the ~1 gauss scale. The reason is that I have uncovered a number of what I would call 'inconsistencies' in some of the vector/Bmod plots.

- The vector plot on the next slide runs almost from boundary to boundary.
- The boundaries are set to TANGMAGN, which means that the field should run parallel to the boundary(I checked this with Vector Fields), but they don't in these plots.
- There are some strange artefacts to the left and right of the solenoids.
- I'm pretty sure the vectors point in the wrong direction in some places…

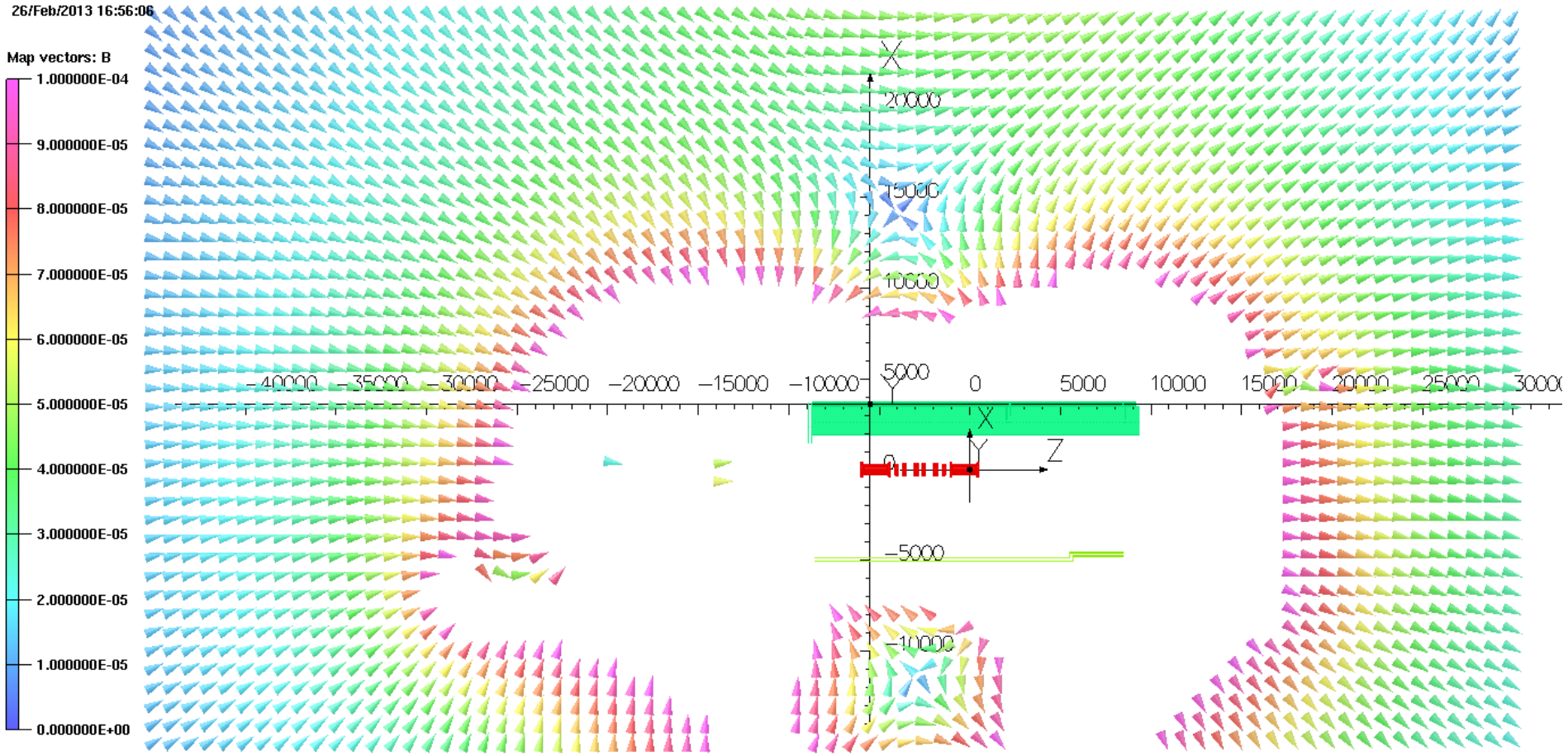
South



North

Model 51 - Step IV 240Mev/c
No return yoke

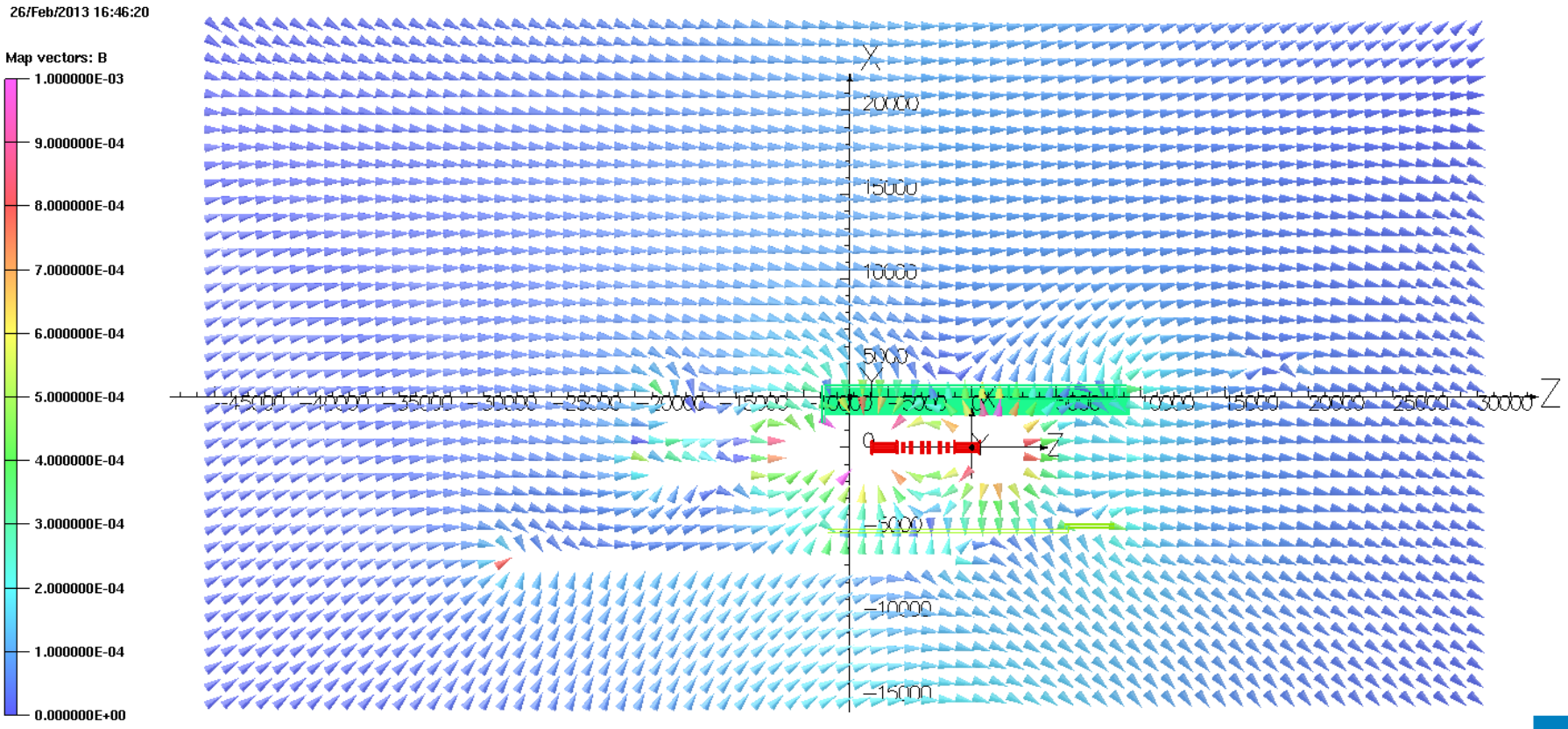
South



North

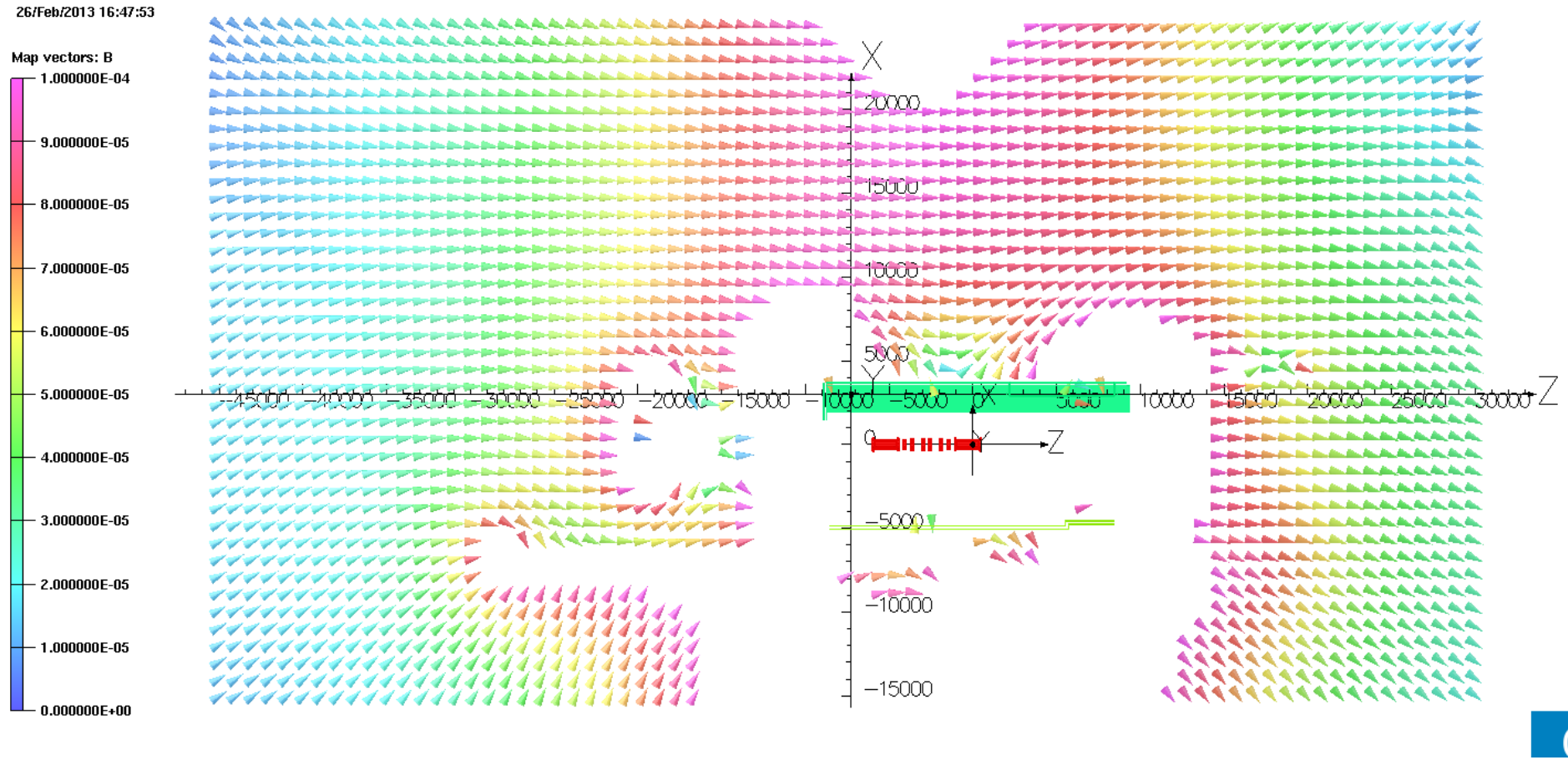
Model 51 - Step IV 240Mev/c
No return yoke

This is the same plot as on the previous slide but on the 1 gauss scale. The fields are small but they are not zero.



Model 52- Step IV 240Mev/c
With return yoke

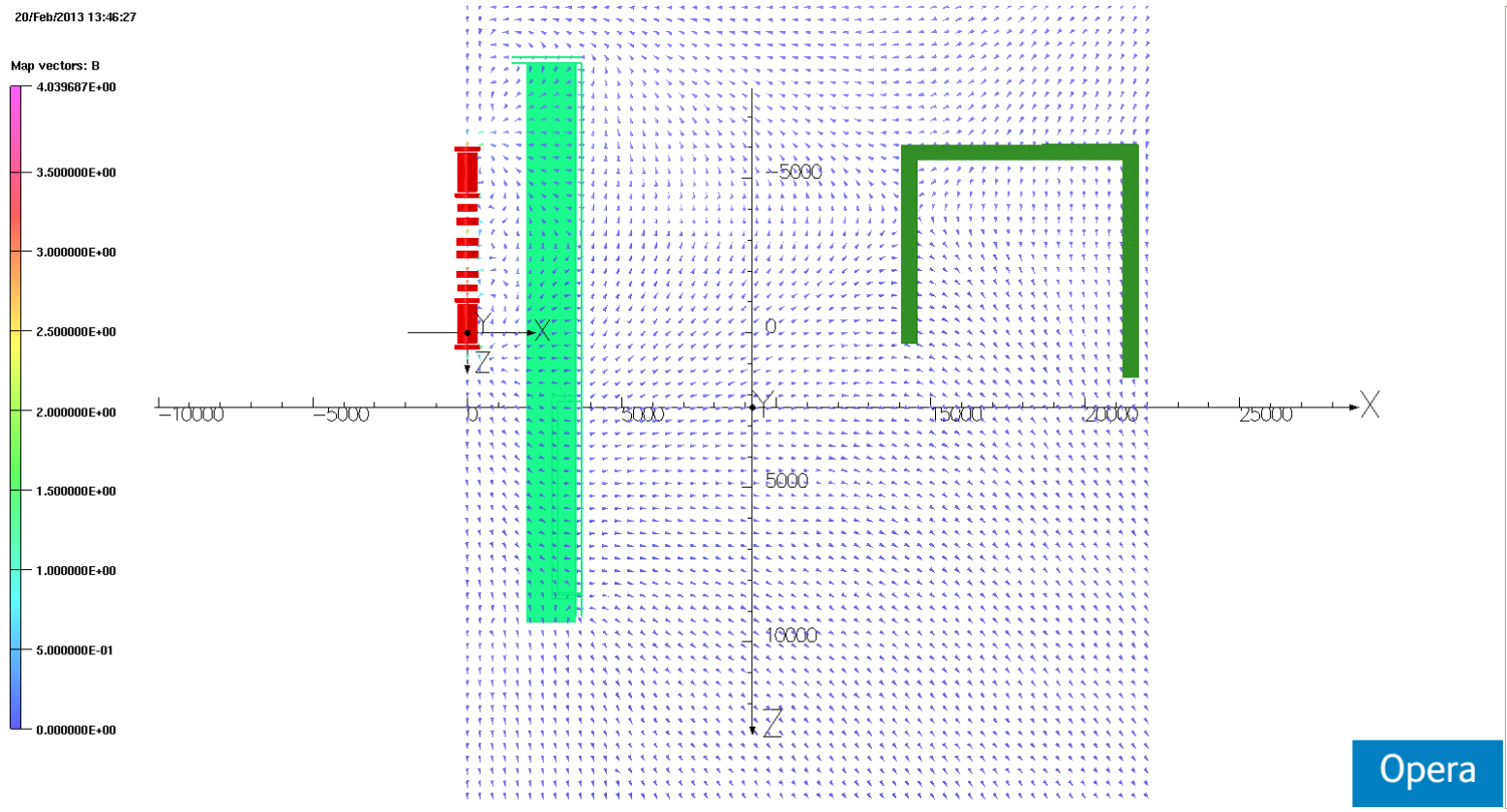
Artefacts disappear but is the background field real? –It seems to be going the wrong way to me….



Model 52- Step IV 240Mev/c
With return yoke

This is the same plot as on the previous slide but on the 1 gauss scale. The fields are small but they are not zero.

One of those artefacts is where RR2 is proposed to be placed.
Do these artefacts have an effect on the field in RR2?
This is from Model 51.



UNITS

Length	mm
Magn Flux Density	T
Magnetic Field	A/m
Magn Scalar Pot	A
Current Density	A/mm²
Power	W
Force	N

MODEL DATA

Hall_Test_51.op3
TOSCA Magnetostatic
Nonlinear materials
Simulation No 1 of 1
13777571 elements
19285041 nodes
12 conductors
Nodally interpolated fields
Activated in global coordinates

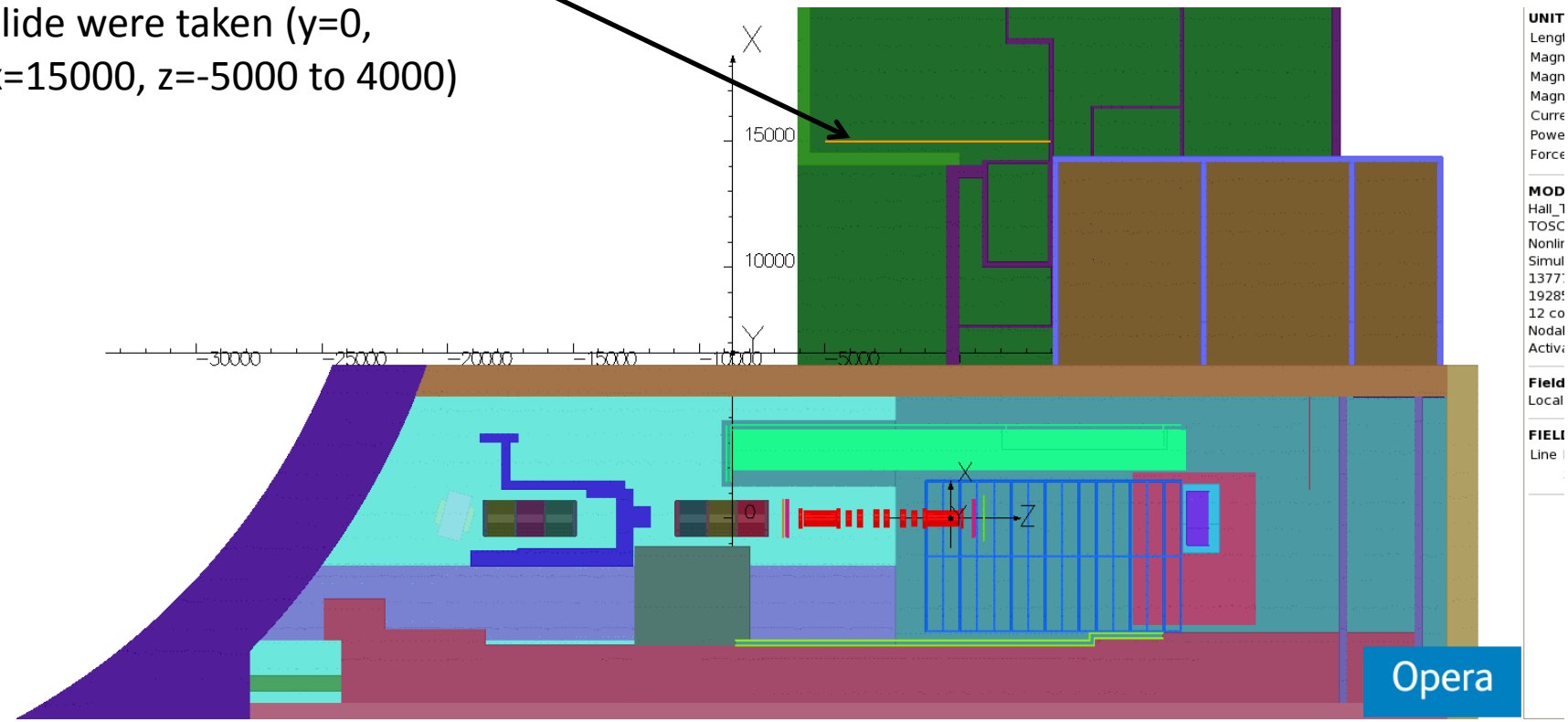
Field Point Local Coordinates

Local = Global

FIELD EVALUATIONS

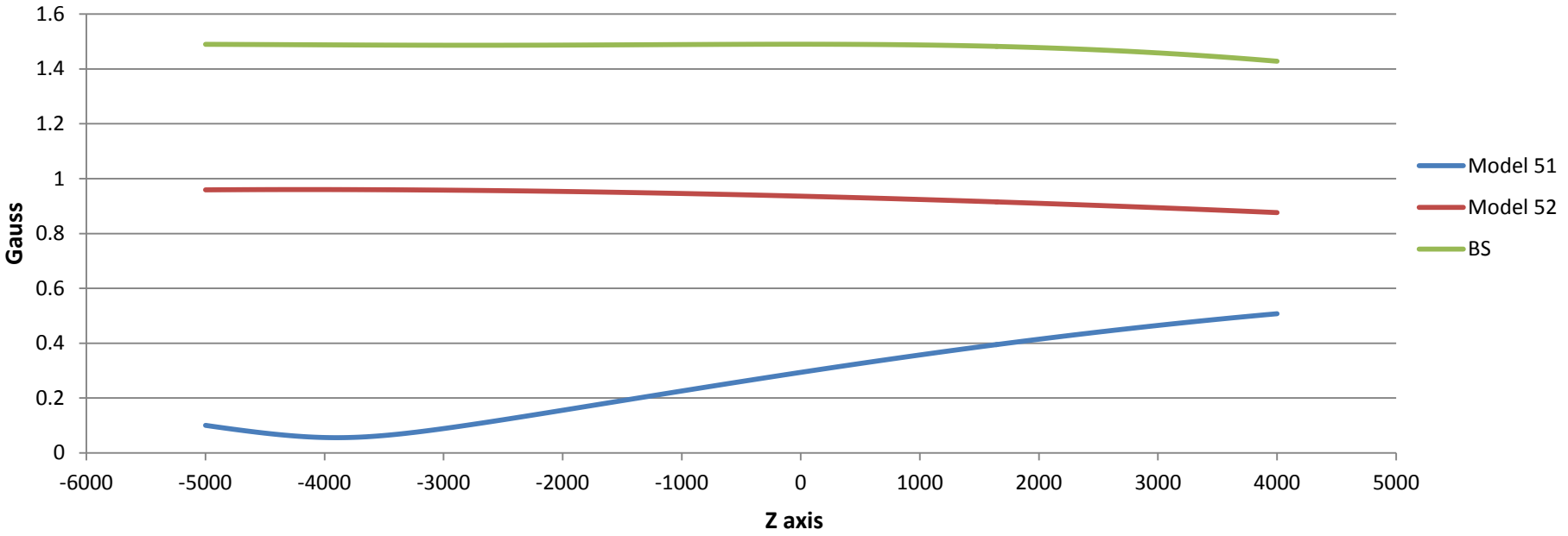
Line	LINE (nodal)	801	Cartesian
	x=15000.0	y=0.0	z=-5000.0 to 3000.0
Cartesian	CARTESIAN (nodal)	75x55	Cartesian
	x=0.0 to 22000.0	y=0.0	z=-15000.0 to 15000.0

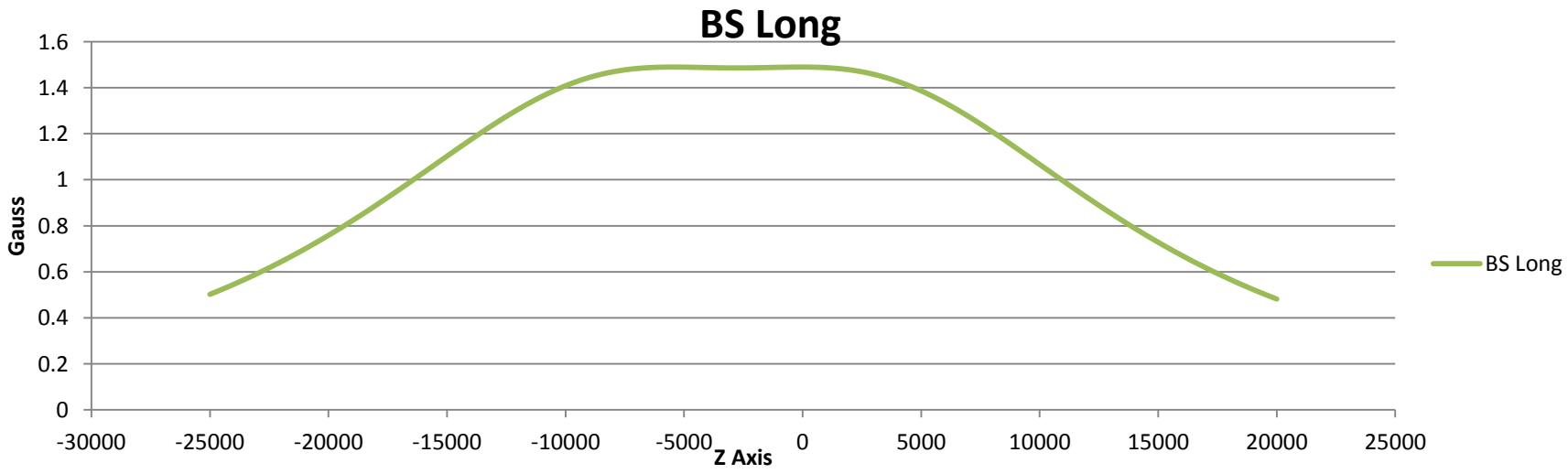
Orange line is where data sets to be shown on next slide were taken (y=0, x=15000, z=-5000 to 4000)



Both graphs are for Solenoid Mode 240Mev/c

Comparison of Biot Savart with Model 51 (No Yoke) with Model 52 (With Yoke) at RR2 demonstrating a possible Inconsistency in the Hall Model





Model Update – 27/02/2013

Before going through some of the relevant details I'll state the conclusion of this weeks investigations…

a) There do indeed appear to be artefacts in the output at the 1 gauss level (no real surprise)
b) The boundary conditions are not set up quite right and this is one key suspect in why we have some strange results at low field level.

    It would be good to correct these boundary condition problems and rerun a model, maybe the problem will go away – there is still the question of the accuracy of the model at the 1 gauss scale – see later slide.

The actual boundary condition problem is a subtle technicality – the boundary condition is defined on what VF call a reduced potential instead of a total potential. (The model is run in total potential). From VF:

"If the boundary conditions are on a volume of air that is defined as reduced potential, then they are only applied to the magnetisation field (not to the Biot-Savart coil field)."

Model Update – 27/02/2013

I think there are 2 points to think about:

a) Putting the scale of the problem into perspective.
b) Trying to justify why I think it is (was) worth the effort in understanding the problems that I'm seeing.

Model Update – 27/02/2013

Putting the problem into perspective.

A lot of what I've shown is around ~1 gauss scale. I think this leads to 2 questions:

1) Did we expect the hall model to give us accurate results at the 1 gauss level?
2) Do we care?

To answer 1, I've copied part of my correspondence to vector fields after I'd posed this very question to them last week….

Model Update – 27/02/2013

…
eg, Am I wasting my time looking at gauss level fields in a complicated model with multi Tesla magnets where I've had to compromise the meshing to get a reasonable solve time?
…

Dear Paul,

Thank you for your email. To answer your question - yes, I would say that you are wasting your time.

The mesh is only fine enough when the results do not change after a refinement of the mesh (mesh-convergence). A second point to discuss is the default convergence tolerance of 1E-08 (#ITSOLVTOL). You might try to tighten this to 1E-10 (but this will increase the solution time - it is probably better to have a finer mesh)…

I've tried running some simpler models and increasing this tolerance – no change to output. I now have models running with increased mesh resolution but this will take a while to solve. Despite the fact that we now have a suspect for the cause of the problem these finer mesh models could still provide me with some useful data so I'm letting them run. (Results not changing after an improvement in mesh resolution.)

Model Update – 27/02/2013

2) Do we care?

I don't know the answer to this yet.

If the problem I show you is only an issue at 1 gauss then we probably don't care too much – but bear in mind we are making decisions about the west wall compressors and RR2 based upon the output of the Hall model (and BS calculations) so we are instantly losing one of our justifications for making this decision. This may not be important but I can no longer justify these decisions based upon the Hall model - No Mice Note.

However do we trust the model at 5 gauss? – I can't answer this either, I don't think we have artefacts at 5 gauss but we also do not yet have any validation - and this may be more important.

I think it becomes self-evident why it is worth the effort (slide 1- point b) to try and understand this problem – even if end up defining a noise floor, as we need to know whether we have systematic errors in the model and we need to know whether these inconsistencies are just confined to the ~1 gauss level or whether they extend further up the magnetic magnitude scale.

I apologise as I've pointed this out every week ad-nauseum but there is a point this week…

Model Update – 27/02/2013

What I think we urgently need is an advisor/experienced consultant who is able to spend some time on this to guide the modelling effort and steer us around these problems.

I am not a magnetic or a modelling expert and at the moment I have no-one to look over these plots/output to offer guidance to me on a day to day basis.

There is a big difference between producing a model - which is what I've spent a large part of my time doing - and then interpreting the output. I can keep asking people for advice and continue to try to solve these problems by reduction every time I come across them, and of course that's a great learning experience, but this can be a slow process. I often lack the experience to directly solve these issues efficiently.

In fact I think this is necessary if we are going to deliver viable solution(s) that we trust in a reasonable time-frame.

Model Update – 27/02/2013

We discussed the possibility of using a consultant last year and I think that the problem was not well enough defined at that point to make a decision on this.

I think that we are now at a stage where we can point at several problems that we are having where guidance could save us a lot of time and effort.

Mike contacted VF yesterday and they stated that they would be interested in helping us out – perhaps Mike will comment more if he is at the meeting?

I think I need to flesh out a more well defined 'consulting scope' to approach VF with but in a nutshell (for me) I want more specific advice on:

1) Model Checks– Giving us confidence in the results/model that we have. Are there any glaring problems with the model? How can we be sure that we have a model we can trust? What are the likely limits (error bars) of this model? – Will what we learn carry over to sub models.

2) Validation - Advice on preparing us to integrate the data from R9 to the model? What agreement should we expect – how do we integrate this data with the model?