Hi All:

Below you will find my comments on the HFT proposal draft. We've already seen remarks from Spiros. Hopefully, the other committee members will send their comments quite soon now. Meanwhile, the HFT team is supposed to be sending us preliminary cost and schedule information any day now.

Carl

I've finally made it through the entire HFT proposal draft. There's a lot of good stuff here. If we get a favorable review panel, it could fly through. But I'm more concerned than Spiros was that the present version is open for trouble if we get a review panel that is "out to get us". Why?

For CD-0, we need to:
(a) Demonstrate a compelling scientific need,
(b) Show that our proposed detector will satisfy that need, assuming it meets specifications, and
(c) Show that there is good reason to believe the detector will meet specs (but we needn't "PROVE" that the detector will satisfy the specifications until later).

I think the current draft does a good job on (c). In contrast, it has shortcomings on (a) that should be rather easy to fix and on (b), which are going to be harder to fix.

What are they?

(a)

The current draft does a good job of describing the essential science that the HFT will facilitate. But "need" also implies that the proposed science can't be done today. In our case, that means demonstrating that the proposed science can't be done with the SVT+SSD+TPC+TOF. The review panel members that DOE selects may or may not be aware of the excellent performance that has been demonstrated with the SVT in the Cu+Cu data. But DOE certainly is/will be aware of it. Thus, the proposal needs to explain why we can't do the proposed science with the current system.

This should be very easy to fix since the SVT has two fundamental limitations:
-- It can't operate at speeds much faster than 100 Hz. Once we have DAQ1000, that will be a very serious limitation at RHIC-I luminosities. At RHIC-II luminosities, it would be death. You don't need to belabor this point, but you should say it.

Two committee reports from STAR Management are available. Both studies conclude that the SVT is not a good pointing device for the HFT, in part for the reasons quoted above. We can and should make reference to these reports.

-- Multiple scattering. The current draft does a very nice job of showing how the HFT performance can be predicted/understood with simple "hand calculations". Why don't you include a similar hand calculation for the existing system? That should demonstrate it's intrinsic limitations for topological charm reconstruction very clearly.

Answer:

The predicted performance of the SVT detector system is shown in Figure 1 and Figure 2. The system is MCS limited so even though the detector resolution on each SVT layer is assumed to be 50 µm in the R-Phi direction and 40 µm in the Z direction, the pointing resolution at the vertex is not as good.

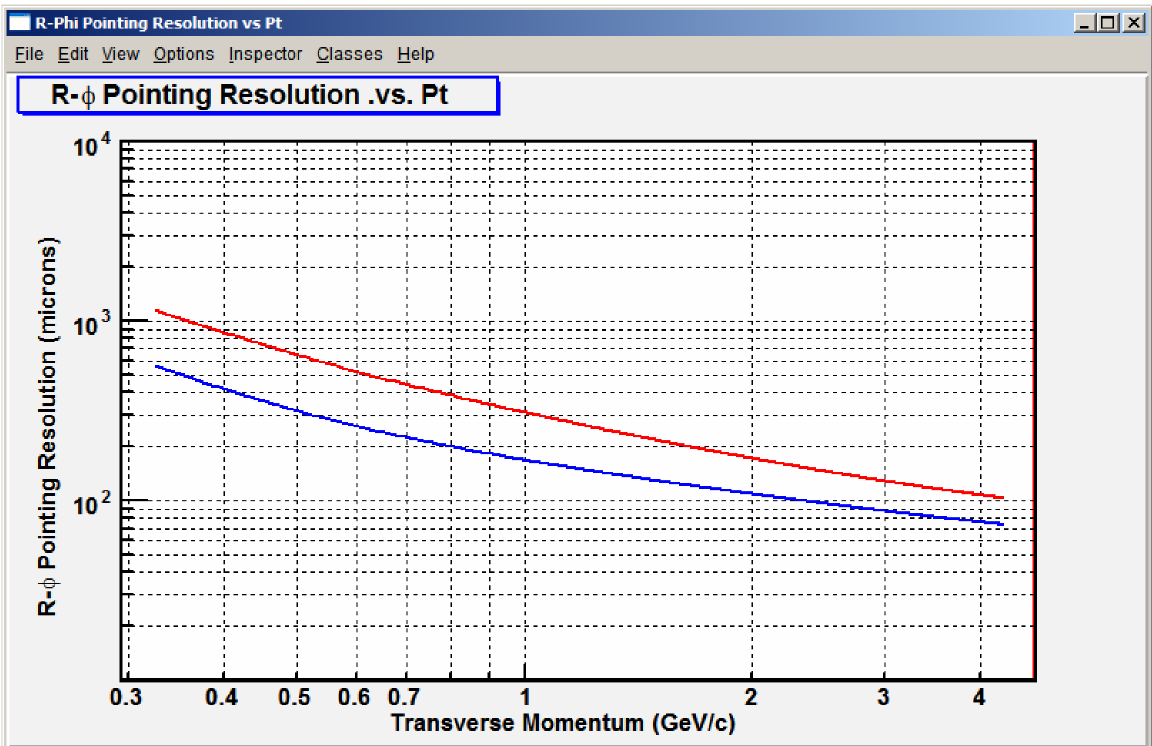

Figure 1: The predicted pointing resolution of the TPC+SSD+SVT at the vertex in the R-Phi direction. The blue line shows the pointing resolution for all three SVT layers. The red line shows the effective resolution if the first layer of the detector is not hit and only layers 2 and 3 have hits.

At the mean p_T of the Kaon from the decay of a D^0 (750 MeV), the pointing resolution is about 200 µm in the R-Phi and Z directions. This gives us the possibility of seeing D^0s with the SVT … but the software reconstruction efficiency is not expected to be very high due to the relatively poor pointing resolution that is supposed to pick out a non-vertex decay with $c\tau \approx 125$ microns.

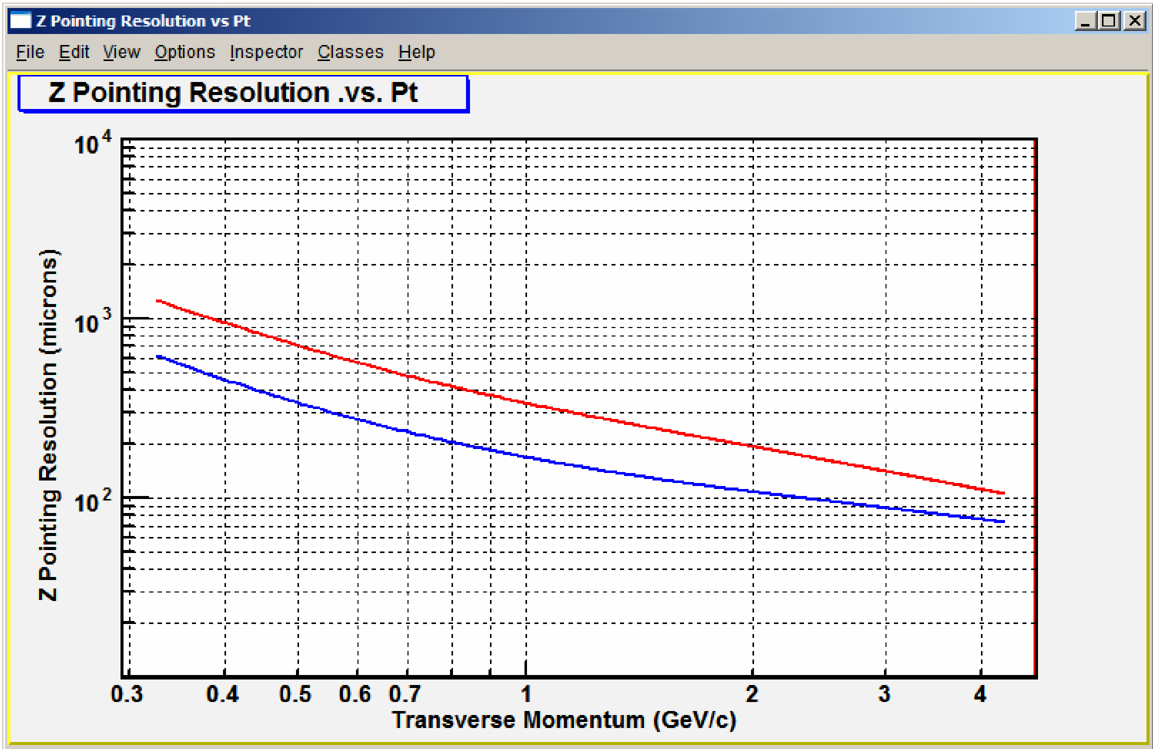

Figure 2: The predicted pointing resolution of the TPC+SSD+SVT at the vertex in the Z direction.

(b)

The current draft really doesn't go near far enough in addressing the pile-up issue. A simple comparison of Figs. 27 and 36 demonstrates that detector occupancy is a very important issue. Part of the inefficiency arises from the TPC (and it would help if you made it clear to the reader how much). But the efficiencies in these figures are well below typical TPC only values, so the Si is also making a significant contribution.

The TPC has a geometric acceptance of slightly less than 90%. Similarly the SSD has a geometric acceptance of slightly less than 90%. Together they are only about 75% efficient and so the curves in figures 27 and 36 in the proposal should not rise above the 75% level. Both of these acceptance factors are included in the GEANT simulations and in the hand calculations. However, all of the new detectors (PXL and IST) are assumed to have 100% acceptance and so any inefficiency seen in these layers is due to occupancy and ambiguity issues, alone.

If Figs. 27 and 36 were the "final answers", everything would be okay. But they aren't. Table 8 indicates that the existing AuAu simulations underestimate the rates in the PIXEL layers by a factor of ~3.5. In fact, the underestimate is worse than that. Table 15 indicates that we could have up to an additional 22 clusters/cm^2 due to accidentals. That's 10 times the PIXEL-2 occupancy of the AuAu simulations! For pp, the situation is as bad or worse. If I assume a RHIC-II 500 GeV luminosity of 4 x 10^{32}, the 2720 in the bottom equation on pg 61 becomes 7200. Hence, we can expect 62 hits/cm^2 in

PIXEL-1 and 14 hits/cm^2 in PIXEL-2 just from pile-up, with accidental clusters and other backgrounds on top of that.

The current draft is up-front about the lack of pile-up in the simulations. But it nonetheless begs the questions, "How much worse is it going to be?", and most especially, "Are things going to be bad enough to fall off a cliff?"

What to do?

The "ideal" fix would be to simulate the all the out-of-time events, pile up their contributions, then proceed. That's clearly prohibative in terms of CPU. But it's also not needed. I think it would suffice to augment the existing GEANT events with extra randomly generated PIXEL clusters at realistic densities, then rerun the reconstructions that generated Figs. 27 and 36. The difference from the current reconstructions will indicate how big a problem pile-up is likely to become.

Carl’s comments are basically correct. Ultimately we need to understand the effects of pile-up on our detector system. The only reason we haven’t done it with ITTF is for lack of CPU time and available manpower … at the same time. A pure random pileup background would be a sufficient exercise. It is not completely accurate in the sense that real events have correlations built in; however that is a detail. Random events will tell us the gross effects of pile-up.

Unfortunately, even this exercise is not easy to carry out. The reconstruction of events by ITTF is relatively slow. It takes longer to reconstruct an event than it does to generate it with the STAR implementation of GEANT. So adding random hits to an existing event will only save half of the CPU time. We would still have to do the reconstruction pass over the event … and that’s where the CPU time goes.

However, there is good news in the hand calculations. Howard Wieman has composed several well written web pages that describe how pile-up affects the overall efficiency for finding hits on a detector. See, for example,

http://www-rnc.lbl.gov/~wieman/HitFinding2DXsq.htm,
http://rnc.lbl.gov/~wieman/GhostTracks.htm
http://rnc.lbl.gov/~wieman/HitFindingPadVsStrip.htm

The bottom line is that the probability of picking the wrong hit on the Pixel detector is given by the following formula:

Equation 1: $$\text{Inefficiency} = (1 - \text{Efficiency}) \approx 2\pi\, \sigma_x \sigma_y \rho$$

Where σ_x is the convolution of the detector resolution and the projected track error in the ‘x’ direction, σ_y is the convolution of the detector resolution and the projected track error in the ‘y’ direction, and ρ is the density of hits.

As Carl has observed, the density of hits on a detector is a function of the integration time of the detector elements. Thus, by changing the integration time of the detector from 4 milliseconds to 200 microseconds we can decrease the inefficiency of the system by a factor of 20x or equivalently relax the pointing requirements of the detector system in front of this layer.

An even faster detector may be the solution to the pileup problem because a faster detector suffers less pileup. In fact, it may be possible to build a Pixel layer with only 10 microseconds integration time and then there is no pileup. An integration time of 10 microseconds, or less, means the detector is only exposed to the multiplicity of a single event. Under these conditions, the pointing requirements of the detectors in front of the Pixel layers is much reduced.

[I recognize that this doesn't account for the effects of pile-up in the TPC. You can refer to the conclusions of the TPC review last October to indicate that finding the tracks in the TPC is not expected to be a problem. Furthermore, within the next month, we should be tracking events in the TPC at up to 30% of RHIC-II rates. And we'll be doing it without the advantage of TOF and IST hits to anchor the tracks and tag the primaries at the two ends. In fact, down the road we should be able to improve on the TPC-only track-finding efficiency with smart utilization of the clean hits that the SSD and IST will provide.]

I also have a closely related concern that is not discussed at all in the current draft. As I understand it, the integration time for the prototype detector will be 4 ms vs. 200 us for the final detector, i.e. a factor of 20 longer. During Run 7, we were often up and taking physics data when the instantaneous luminosity was around 30% of the expected typical RHIC-II luminosity. If I put those two numbers together, I conclude that pile-up in the prototype at RHIC-I luminosities is going to be up to 7 times worse than in the final detector at RHIC-II luminosities. How is the prototype going to perform under such extreme conditions?

The 4 msec detector is a good detector because it allows us to learn about the technology, and learn how to build a high tolerance detector alignment system; and thus ensures success with the final detector. But a 4 msec detector is not the best detector for doing D^0 physics. It is relatively slow and thus suffers for a large amount of pileup. For example, Figure 3 shows the predicted efficiency for a 4 msec detector compared to a 200 microsecond detector based on hand calculations. The solid blue line is the single track efficiency for finding a kaon in the TPC+SSD+IST+PXL. The dashed blue line is the efficiency for finding D^0s and is a translation of the single track efficiency curve after doing the Lorentz kinematics representing a pair of daughter particles from the decay of the D^0 parent.

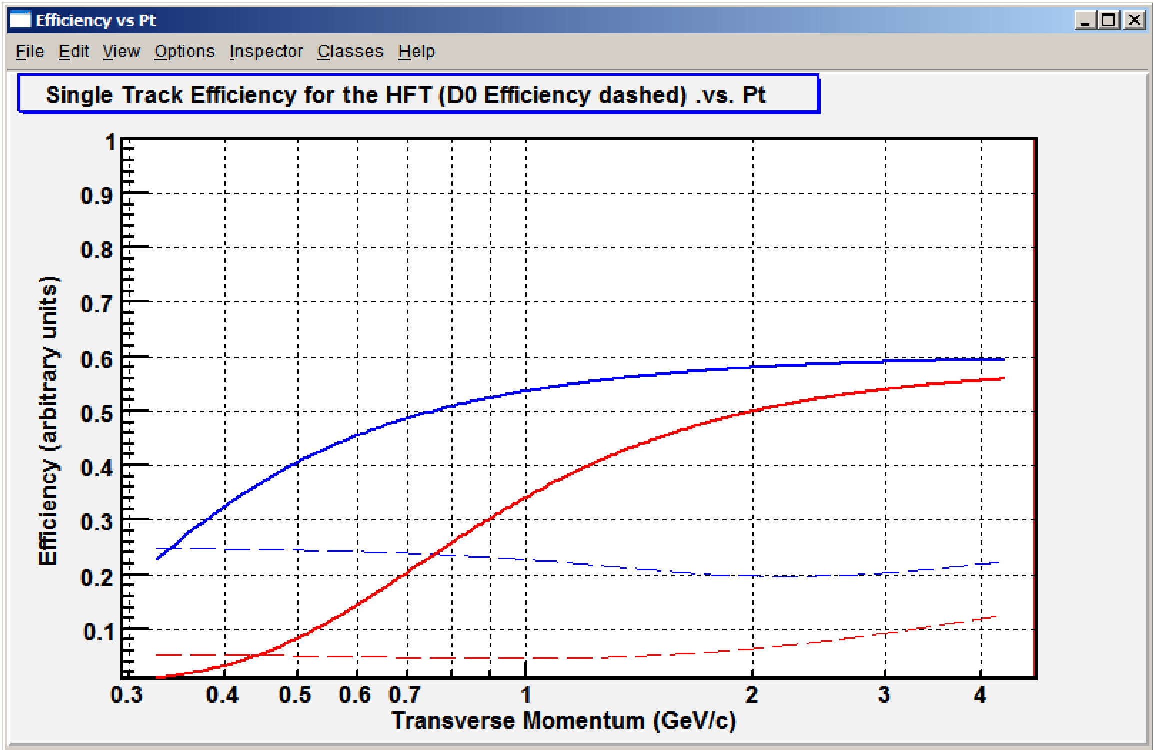

Figure 3: The predicted single track efficiency for the HFT (TPC+SSD+IST+PXL) is shown by the solid lines. The dashed lines show the D0 efficiencies. The blue line is for the 200 µsec detector, and the red line is for the 4 millisecond detector. Pile-up is included in these calculations at RHIC II luminosities.

Finally, my last general remark is that the current draft never really tries to justify the need for the IST layers. However, unlike the above issues, that's probably okay for this stage. But their importance will need to be made clear at the CD-1 and CD-2 stages. (Maybe through explicit simulations showing what happens as various layer(s) are removed?)

The IST is not needed to do heavy ion physics with a 200 µsec detector. The IST is a spin detector and should be evaluated on its merits to enhance the spin program. Figure 4 shows the predicted efficiency of the HFT without the IST layers. The TPC+SSD+PXL detectors are all active. The IST layers are not used. Further, the calculations in Figure 4 are deliberately pessimistic to show that the system still works when extra background is added and the detector resolution is twice as bad as assumed by the detector design specifications. The primary conclusion is that the system (minus IST layers) works.

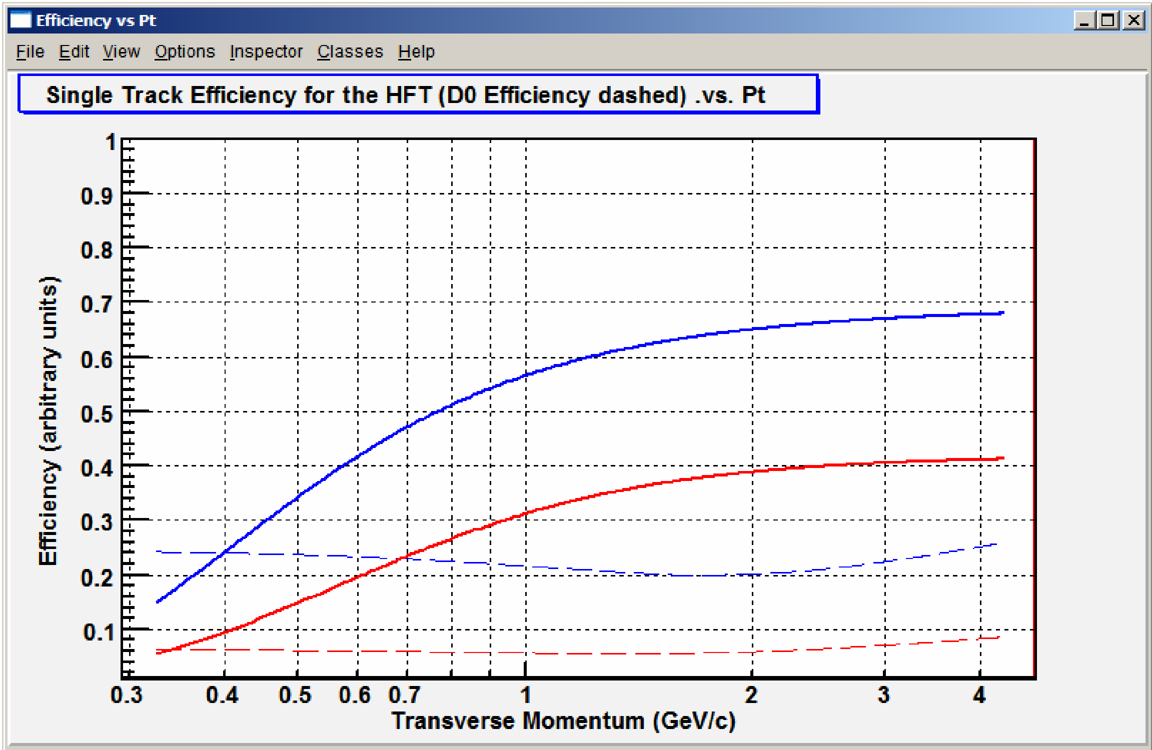

Figure 4: The efficiency for finding D0s in a 200 µsec PXL detector when the IST layers are not active. These calculations were done under deliberately pessimistic assumptions (see text). The blue lines are the efficiency curves for a detector with TPC+SSD+PXL. The red lines are the efficiency curves when the SSD has been removed from the system. The red line is for the TPC+PXL detectors and shows that the loss of the SSD has a negative impact on the overall efficiency for finding D0s.

Another important conclusion can be drawn from Figure 4: the SSD is required to do good physics with the HFT. The red line in the figure shows what happens when the SSD is removed from the tracking system; the D0 efficiency drops by a factor of 4.

The reason that the HFT works without the IST layers is because the pointing resolution of the TPC+SSD is sufficient to guarantee that the PXL layers are highly efficient even in the presence of pileup. The pointing resolution of the TPC+SSD system, under pessimistic assumptions, is good enough to keep a 200 µsec detector from being overwhelmed at RHIC II luminosities. In fact, the inefficiency of the first pixel layer is only 4% (see Equation 1) and so it is difficult to make a dramatic improvement upon this level of inefficiency. The second pixel layer is less efficient but the addition of the IST layers do not help that problem because they have a relatively short lever arm compared to the SSD and their long strips introduce ambiguous hits that increase the apparent hit density so that the IST hurts a 200 µsec detector as much as it helps. This conclusion is not true for a 4 millisecond detector. A 4 millisecond detector must have the IST layers as well as the SSD in order to handle the extraordinary levels of pileup.

Table 1: Pointing resolution, detector resolution, and efficiencies for a 200 μsec detector that uses only the TPC and SSD as a pointing device for the PXL layers. The pointing and detector resolutions are deliberately pessimistic to emphasize the worst case scenario (see text).

Radius	PointResOn	PointResOnZ	DetRes	DetResZ	Density	Efficiency
0.230	1002	1210	47	1350	0.26	0.97
0.070	399	1221	15	15	13.60	0.71
0.025	67	122	15	15	84.93	0.96

Enough for the "big picture" concerns. Here are my minor suggestions:

-- Pg 27, Fig 2: I don't understand the difference between the "Thermal" curve and the "Ideal thermal" curve. I know I should look at Ref. [16]. But I haven't. The reviewers won't either.

Agreed. We should explain it or not show the curve. The ideal thermal curve is hard to see and is of primary interest to model builders who like it as a reference. It is not so relevant, here.

-- Pg 30, par 3: The c-cbar pair mass is typically assumed to be quite a bit larger than 2.2-2.3 Gev.

-- Pg 33, Fig 7 caption: I don't see any open symbols on my printout.

I believe the caption is out of sync with the figure. The figure was updated after QM and the caption was not.

-- Pg 41, par below Fig 11: The HFT only provides a factor of 10 improvement for vector mesons? I thought it was supposed to be quite a bit better than that. Meanwhile, are the assumptions here consistent with the numbers in the next section?

It is not clear to me that the HFT helps at all. The number of conversion electrons that come from the mass of the PXL and IST layers may overwhelm any gains from raw resolution. We have debated this point, internally, many times without reaching a firm conclusion.

Frank Laue struggled with this when he tried to decide if the SVT and SSD would help the heavy flavor program or hurt it. His conclusion was that the mass hurt more than it helped … but truthfully, we won't know until we make our low mass run next year.

-- Pg 43, par 3: The statement here about obtaining the necessary pp reference data "with reasonable beam-time allocations" is probably true when triggered pp events will suffice. But many of the proposed AuAu measurements involve charm reconstruction in untriggered AuAu events. How are those applications to be addressed? Likewise, on pg 79, what "efficient trigger" for high-pT charmed events do you envision?

-- Pg 57, Fig 22: There is an "inversion" in each of these panels. In the right-hand panel, the PIXEL-2 and IST-1 curves are inverted. That presumably comes about because IST-1 introduces multiple scattering but provides no z coordinate measurement. Noting that would be worthwhile. In contrast, the IST-1 and IST-2 curves are inverted in the left-hand panel. I can see how that can come about, but it's only a guess. It would be useful to explain this for the reviewers.

Good observations and thank you studying the figure and reading the chapter so carefully. You win the proverbial bottle of champagne! Your observations are correct. The SSD, IST2, and IST1 layers alternate in terms of which dimension they offer the best resolution. The TPC give approximately uniform resolution in both dimensions. The SSD improves upon this but only in the R-PHI direction (see the green lines in figure 22a and 22b). The IST2 layer further improves the resolution in the Z direction but actually degrades the pointing resolution in the R-Phi direction due to MCS and the effects of different lever arms. The IST1 layer further improves the R-Phi pointing resolution but loses ground in the Z direction. Finally, the action of the PXL layers is nearly symmetric and thus the resolution on the vertex is nearly symmetric.

We will update the text of the proposal, accordingly.

-- Pg 72, Fig 31: This is one of your money plots, but it doesn't have the impact that it could. How about adding a lower panel, replotting the "after cuts" curve on an appropriate linear scale? The D^0 peak will jump out.

Yes, good point.

-- Pg 76, par 3: I'm not sure I understand how the current discussion explains why the primary vertex peak is broader.

Agreed. I am confused by the text, too. I believe that the correct answer is that the refit helix is an improved fitting routine and does a better job than the standard ITTF routines with defined the primary vertex distribution. Thus the refit displaced vertex distribution should be narrower than the primary vertex distribution. The displaced vertex distribution can be made even narrower after more refinements to the code.

-- Pg 85, just below Fig 40: I think you mean "MIMOSA-5" here.

Yes.

-- pg 94, last line: Shouldn't the outer layer be at 7.0 cm?

Yes, that's an oops. 5.0 cm is consistent with what is shown in the figure, however, the mechanical design is evolving and you see the evolved mechanical design in this chapter. The new mechanical design is not consistent with what was simulated. 7.0 cm was used in all of the simulations. Executive decision required.

-- Pg 96, Fig 45: Am I correct that the cooling fins and other "end of ladder" mass will only be on the east end of STAR?

Yes.

-- Pg 110, Table 18: It appears these numbers assume 10^{27} luminosity. That's not crazy for an AVERAGE at RHIC-I. But C-AD delivered collisions at INSTANTANEOUS luminosities of >3 x 10^{27} during Run 7. Which number is more appropriate for this calculation?

If we were fully consistent, everything in the proposal would use $8x10^{27}$ as the RHIC II design luminosity. We have not fully succeeded in that goal. In a few places, we state what was assumed in the calculation and you have to scale that result to the real RHIC II luminosity. There may already be updated text for this section … will have to check.

-- Pg 112, Fig 60: For the prototype, the plan is to obtain very low effective dead time by reading out overlapping frames into multiple buffers. This figure seems to imply that the plan for the final version is to have 2 buffers, but no overlapping readout for the initial 1 ms dead time. Is that correct?

-- Pg 120, vertex resolution spec: "... from intermediate to large B-meson decays ..." looks like a cut-and-paste garble.

Agreed. Some of us like to work with large B mesons; they are easier to see.

-- Pg 128, Fig 73 caption: I've never heard of a Si die 8 meters by 7 meters.

Good point.

-- Pg 130, par 2: Is is clear that the SVT optical survey system will do the job, in light of the fact that Working out the SVT alignment proved to be an extremely challenging task.

-- Pg 130, Sect 5.12: "forward GEM tracker". Also, this paragraph mentions water cooling. I don't remember seeing a mention of anything but air before this. I presume this is for read-out electronics outside the active region. But that should be made clear.

-- Pg 136, par 1: I've never seen out-gassing measured in W/m^2 before.

See the discussion of weird units at:

http://www.lesker.com/newweb/Technical_Info/VacuumTech/Outgas_01_WeirdUnits.cfm?CFID=89596&CFTOKEN=93657057

-- Pg 137: I think we ran somewhat more than 10^9 Au ions per bunch this year.

Yes, various (older) documents from Roser say 1.1 10^9 per bunch. It might still be higher … but it can't go up by much because the system will melt down if the beam currents go up too much more (vacuum issues).

-- Pg 138, last par: I thought IST was to be supported off both ends.

Truthfully, its not yet known. The design is in flux and good ideas come up every day … but we should be consistent within a single document … that is a good policy and you are correct on that point.

-- Pg 139: Is it an accident that the summary never mentions pp collisions?

-- Pg 140, 1st line: The SSD is the "fifth" layer.

Yes. Perhaps it would be better to say that the SSD is the outmost layer of the HFT. Counting above three has always been one of my weak points. I suspect that others may have the same issues.

That's it!
Carl

Thanks for the careful read.