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Dear Colleagues:

I have found myself unsettled by much of the discussion regarding the future of RHIC spin, and have endeavored to understand the reason for this. My first insight was that my malaise was symptomatic of a general lack of comfort with the entire planning process, but let's concentrate on spin in particular.

I would like to "speak" openly and in fact bluntly. I very much hope that I can do so without having my commitment to a healthy and vital spin program at RHIC questioned. I won't belabor this point other than to note that I have defended the objectives of this program and its viability within the PHENIX collaboration at each and every RHIC program review and in many other informal contacts with various funding officers. You should regard the comments that follow as my own personal ones, that is, they do not represent an official PHENIX policy statement. But I will share these thoughts with the PHENIX Executive Council, and will inform you of any developments that result from that dialogue that could lead to a more formal position.

Much of the discussion to date has concentrated on the tremendous benefits that would accrue from running 37 or even 32 weeks per year rather than 27 weeks. This of course is true, and we can't make the case often enough when speaking to those with the ability to affect this. However, I think we have also been engaging in a bit of reality avoidance regarding what we could expect given the most ideal assumptions.

To investigate this, I performed an exercise in the spreadsheet I developed during the preparation of the PHENIX Beam Use Proposal. The spreadsheet incorporates the CAD guidance on both linear-ramps to maximum luminosity within a year and on the year-by-year

expected luminosity improvements. Thomas Roser has repeatedly stated that the full value of the year-by-year improvements can be expected only in years dedicated to running that species, but that applies to precisely the case I investigated:. Given this ideal world, when *would we reach the stated RHIC Spin Collaboration (RSC) goals if we ran **only** spin?*

Recall that the stated goals were a direct photon measurement using 320 pb^{-1} at 200 GeV. It is my understanding that the physics plots described in the various RHIC planning documents assumed 320 pb^{-1} ***in*** the physics analyses, i.e., 320 recorded, all of it analyzed.

Below are the results for CAD's pessimistic and optimistic guidance, along with the geometric mean of these two extremes (the geometric mean is what was used in the PHENIX Beam Use Proposal). All values are for p+p running at 200 GeV and are expressed in inverse picobarns. The two lines for each scenario correspond to 27 and 37 weeks per year. The green shaded cells denote the year in which the luminosity integrated over all previous years approaches or exceeds 320 pb^{-1}. (Note that for the 27 week per year pessimistic scenario this condition is not really satisfied even in Run-10, hence the cross-hatching in that cell.)

		Run-4	Run-5	Run-6	Run-7	Run-8	Run-9	Run-10
Pessimistic Assumptions	27 weeks	3.0	7.4	21	40	55	55	55
	37 weeks	5.3	13.3	38	72	99	99	99
Geom. Mean Assumptions	27 weeks	4.5	11.3	32	62	84	84	84
	37 weeks	8.1	20.3	58	100	151	151	151
Optimistic Assumptions	27 weeks	6.9	17.3	50	94	129	129	129
	37 weeks	12.4	31	88	168	230	230	230

While it may be argued that comparison of one decade-old model to another model does not necessarily lead to obvious progress, think it does help to quantify the basic issue we are confronting. My conclusions, ***if*** we accept the CAD model assumptions, are

1. No minor tweaking of the number of weeks per year will lead to a near-term delivery of anything approaching the originally stated goals of the RSC. This statement becomes even more definitive if we recall that the 320 pb^{-1} was assumed to have 70% polarization. In the table I presented I have blithely summed all years without asking when 70% is likely to be available.

2. The original planning for 320 pb^{-1} was roughly a factor of 10 too optimistic. Presumably the same factor applies to the 800 pb^{-1} called for at 500 GeV, which (assuming we blindly pursue the original luminosity targets) would begin to be addressed only after going green for 200 GeV in the various scenarios, i.e., in 2010+.

3. Some serious re-examination of the spin goals will be necessary. Here the good news is a) I know that it's ongoing, and b) the 320 pb^{-1} was clearly a gold-plated measurement.

4. Something has to give. There is ***NO*** possible solution that simultaneously satisfies the requirements of the heavy ion program, the desire to have a beam development runs ***each*** year for spin and the need for sustained periods of polarized running to achieve specific luminosity and polarization goals. This is certainly true in 27 and 32 week scenarios, and probably true even in 37 weeks.

You might hope that these conclusions would be followed by some serious guidance as to how we should proceed. I don't possess that level of wisdom. Instead I offer the following somewhat pointed observations:

- I take great exception to statements that failure to develop *x* weeks of spin running (or any other mode running) by year *y* will result in significant losses to other facilities. This will occur only if one wishes to instead perform difficult measurements that do ***NOT*** offer the benefit of RHIC's superior reach in *x* and Q2. RHIC is the most outstanding hadron physics facility in the world, and serious people will make serious commitments for the long haul. I note that RHIC first appeared in the Nuclear Physics Long Range Plan in 1983, and (as a parochial case in point) Columbia made a commitment to a relativistic heavy ion program beginning in 1986. While the initial Columbia efforts were in fixed target physics, these programs were explicitly anticipatory, i.e., the commitment would not have been made without the assumption that RHIC would eventually become available. What I propose in the present case is that RHIC can already provide a physics harvest in spin topics that is adequate to maintain a healthy program while we all strive to achieve some fraction of the ambitious goals originally defined by the RSC.

- It is imperative that we re-assess those goals in light of our current knowledge of the machine performance. This is part of the ongoing process, but it is important that we do so quantitatively, to insure that we are all on the same page. The very last thing we would want is some impossible-to-meet metric established that serves only to set us up for failure, as has been the case at some other large collider facility in the U.S.

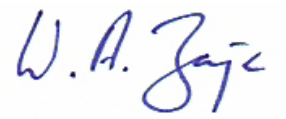
- We must look for gains in integrated luminosity in all possible areas. Much of the discussion as emphasized what can be obtained by changing the *dt*, but given fiscal realities, it is imperative that the *L* term also be aggressively attacked. Within the context of the CAD model, substantial increases could be realized by a) decreasing the long ~14-week ramps to achieve maximum luminosity in each running period and b) decreasing the 5 week set-up times that make running a second species so costly. In this context I will cite my colleague Glenn Young, who has considerable insight into machine operations, and who has noted the tremendous benefits that accrued at CERN via development of super-cycles in the PS that allowed pulse-by-pulse context-switching, with the subsequent efficiencies in parallel development of intensities.

- While more efficient mode-switching is clearly desirable, I remain convinced that in the short term we maximize RHIC's scientific output by minimizing the number of switches. This has been the guiding principle of the PHENIX Beam Use Proposal. Applying it stringently was useful not only in developing an optimal plan, given the imposed planning assumptions, but also for forcing us to confront and define our priorities. As part of that process, we accepted the arguments that 5 weeks of polarization development in Run-4 were an absolute necessity, even though it exacts a real and painful penalty on an already luminosity-limited measurement in the Run-4 Au+Au segment. These same discussions also led us to modify our original suggestion of a dedicated spin run in Run-5. (I believe a similar evolution occurred within the STAR collaboration.) Thus, it is with some consternation that I read in the summary of the spin discussion group "*Toward this end, it is very important that the first long polarized pp collision run be scheduled during the anticipated FY05 RHIC running period. This is not currently the case in the 5-year Beam Use Proposals submitted by either STAR or PHENIX under the assumption of 27-week/year runs; thus, it requires consideration of alternative RHIC running scenarios.*" The only was I could endorse such a statement is if "alternative" is interpreted as "additional weeks", or as "opening the phase space to completely reconsider the current Beam Use Proposals". It is my personal belief that the most probable result of that process would be elimination of the 5 weeks of beam development in Run-4. Would this be a desired outcome?

That last question is more than rhetorical. ***If*** the extended spin running in Run-5 is ***essential***, and ***if*** the budget realities do not improve substantially, then it could well be that the solution could required forgoing beam development (in RHIC, not the AGS) in Run-4. These are the sort of very tough questions that concern me greatly, and that can be addressed only when planning takes place in an environment (such as the preparation of the Beam Use Proposals) that seeks to optimize the physics output of the collaborations integrated over both physics topics and running periods.

It is my hope that we can find a mutually agreeable set of solutions via significant increases in the available running time, and it is my fervent hope that failure to do so in the near term does not jeopardize the extraordinary collaborations that embody the tremendous physics breath of the RHIC program.

Sincerely,



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