BNL PAC Recommendations, 9/10/04

RHIC Run 5

The PAC agrees with the plan to divide the Run 5 beamtime roughly equally between heavy ions, colliding Cu+Cu, and polarized protons.

There was a consensus within the PAC that a 8-10 week pp run at $s^{1/2}$ = 200 GeV should have the highest priority and must not become the victim of possibly difficult budgetary boundary conditions.

A brief polarized pp run at higher energy $s^{1/2}$ = 400-500 GeV would be desirable, because it would allow a first practical exploration of the challenges posed by the depolarizing resonances known to exist above 100 GeV beam energy. Once achieved, we recommend a brief (unpolarized) physics run of one or two days to make first measurements at the higher energy.

A substantial run of polarized p+p at $s^{1/2}$ = 200 GeV is an essential component of the effort to establish a robust spin program. An 8-10 week run is necessary for luminosity development, and it should produce enough data to allow for a significant measurement of spin observables. The luminosity development is an important prerequisite for a successful spin program when all polarization enhancements are in place. The physics results from the run would be of immense value to the SPIN community, which has been starved of data.

A Cu+Cu run at $(s_{NN})^{1/2}$ = 200 GeV is the logical next step in the exploration of the E-A landscape of heavy ion reactions. In particular, the Cu+Cu run will make it possible to measure the system size dependence of important observables, such as the high-p_T hadron suppression, in a different geometry. The results of this run are expected to provide critical tests of the parametric dependence of parton energy loss on density and path length. The Cu+Cu system is a good choice, because it will significantly extend the range of measurements with respect to the number of participant nucleons, compared with Au+Au, but still allow for a direct comparison with the Au+Au measurements in an overlap region.

The Cu+Cu run should accumulate an integrated delivered luminosity of at least 7 nb^{-1} at 200 GeV, which should be feasible during an 8 week run. Several PAC members felt that the Cu+Cu run was still part of the "exploratory phase" of the RHIC heavy ion program, making it advisable to also obtain data at other energies after the luminosity goal at 200 GeV has been achieved. The PAC recommends a two-week run at 62.4 GeV and a one-day run at injection energy. This would allow the experiments to complete their systematic survey of the energy - nucleon number - impact parameter space.

RHIC Out Years

Here we present a summary of the PAC discussion on the RHIC running for the out-years, defined as the 3-5 years of running following the Run-5 period.

There was broad consensus that the world-class spin program is at a critical stage of transition from a developing program into a full-fledged physics program. It is critical to maintain a minimum of 8 weeks of running each year in the out years in order to sustain machine development and experimental measurements. Significantly longer runs should await the functioning of the cold snake in the AGS.

There was a feeling that the Cu-Cu running in Run-5 closes out the exploratory phase of the heavy ion program. Many felt that more detailed species scans and energy scans require more targeted specific physics predictions from theory or other data-driven motivations (from Run-4 and upcoming Run-5) that could be quantitatively tested. Others felt that the general survey of system sizes was not complete. Some expressed the view that these new systems should provide the opportunity for quantitative predictions to be tested in detail.

There was also a consensus that many crucial measurements in heavy ions require some upgrades to the STAR and PHENIX detectors. All felt that another long Au-Au full energy run would be best timed after the STAR time of flight and PHENIX vertex detector at least are available. There was discussion of the issue of trading running time for detector upgrades in a constant effort scenario. There was broad consensus that this difficult trade was worthwhile since the benefits of the upgrades are crucial. The inclusion of charm observables and at higher luminosity tagged photon-jet measurements is important to the physics program.

The physics topic of deuteron-gold reactions is an important topic to pursue at RHIC. It is essential, however, to expand the capabilities of the existing detectors beyond single inclusive measurements and also to further define focused physics goals. However, difficult decisions may need to be made concerning long deuteron-gold (or preferably proton-gold) studies weighed against continued spin development and detector upgrade funding.

The next few years should be very exciting, but also require much effort on the part of theorists and experimentalists to be more quantitative in their studies and in the expectations of what physics can be gained as we pass out of the initial exploratory phase. Some suggested that future PAC presentations should appear more as proposals for physics goals with a stronger basis for luminosity benchmarks and with more theoretical guidance.

PAC Assessment of the First Four Years of RHIC

RHIC has now had four runs since the completion of construction. These included Au-Au collisions at 130 and 200 GeV $(s_{NN})^{1/2}$, d-Au collisions at 200 GeV and polarized p-p collisions at 200 GeV.

By any standard, the high energy heavy ion accelerator performance and the success of the experimental program has been superb. The results strongly indicate that a new state of hadronic matter is produced in these collisions and the phenomena show a variety of systematic, and most interesting features. It is clear that the current program is extremely well targeted towards elucidating the fascinating physics underlying these results.

The accelerator has gone through a commissioning phase in which its performance has increased at a rate exceeding the most optimistic plans. A great deal of credit is due to the Collider-Accelerator Department and its scientists, engineers, and technicians.

The detectors have also performed exceedingly well. All have worked essentially as designed. It is also gratifying to note that the different experiments are in agreement in all aspects for which their capabilities overlap. The four experiments have also shown the value of the differences in their design in their range of measurements accomplished. The outstanding success of the program is reflected in the fact that a number of PAC members now view the "exploratory" phase of RHIC as essentially complete.

Having said this, we note that absolutely key physics knowledge remains to be gained from experiment if we are to understand the basic physics of the new types of hadronic matter we can produce. These new experimental forays require new upgraded capabilities for the detectors. Without these upgrades, there is a danger that the progress of this promising field will stagnate. Acquiring these in a timely manner now appears as the largest challenge to the program.

The polarized proton program has progressed, for natural reasons, in a slower fashion. New equipment needed to be designed, constructed, and commissioned, including a polarized jet target for absolute polarization measurements, and a superconducting (Siberian) snake for the AGS. Given these requirements the advances in the polarized proton program have been successful. This program is now poised to make the basic measurement of the gluon spin distribution in the proton and to begin the new study of transversity measurements.

The key challenge for the polarized proton program in the future is to provide the relatively long runs needed to develop the necessary luminosity and to achieve the expected polarization at high luminosity. Later in the program, the proponents envision very interesting measurements at 500 GeV where W boson production is possible and leads to the possibility of measuring the "spin content" of the sea antiquarks in the proton, as well as new tests of fundamental symmetry in the electroweak interactions. The studies involving the W boson also require upgrades to the detectors, particularly the forward tracking systems.

EoI: New RHIC II Detector

The group has raised the issue of a new detector for RHIC II with enhanced capabilities. The PAC agrees that this is an important question for the relativistic heavy ion community as it prepares the case for RHIC II.

We feel that the question of a new detector should be embedded in a broader community discussion of RHIC II – a discussion that hopefully will culminate with consensus on a number of important issues. The first issue must be the physics drivers behind RHIC II. In our view these drivers must include qualitatively new goals, not simply a continuation of the current RHIC program into a regime of higher rates. Theorists should play an important role in formulating this science justification. The discussion should also address the relationship between RHIC II and the LHC, and the future of the partnership between the heavy ion and spin programs that has evolved at RHIC.

As the current Expression of Interest has raised issues that should be part of this broader discussion, we encourage the authors to play an important and constructive role in this process.

Once a reasonable degree of consensus has been reached on the RHIC II drivers, the RHIC experimental community should begin a critical discussion of the detectors that will be required. This discussion should include the future of STAR and PHENIX, including the potential of these detectors if substantial funding, commensurate with that required for a new detector, were available for upgrades. It should include new detectors of the type proposed in the Expression of Interest: it is important to explore several creative new directions. It should include a cost/benefit analysis – physics gained per additional dollar of investment. Finally, the choice or choices that emerge from this discussion must be carefully costed, with realistic contingencies, and then evaluated in the context of plausible budget scenarios.

Some of the issues are potentially contentious. Thus we urge BNL management to help lead the community discussions in a way that will maximize the opportunity for reaching consensus. The laboratory has already recognized the importance of community workshops in preparing for future Long Range Plan discussions of RHIC II. These workshops should be inclusive and focused first on the physics case. Once the physics goals are established, the Laboratory should encourage an open-minded discussion of detector options. If a consensus can be reached, the RHIC II community will then enter the Long Range Plan discussions unified and with clear goals.

E964R: Hybrid Emulsion Experiment

We find that the proposed AGS-E964 (revised) experiment is compelling and we reaffirm its approval. Its technique provides unique access to the physics of multiply-strange hypernuclei. Improvements in the apparatus implemented since the initial proposal have strengthened the experiment considerably. We urge that funds be found for it to run in FY2006, prior to modifications of the AGS Switchyard that will eliminate the beam splitting to the D-line.

The experiment will study doubly strange hypernuclei produced by stopping Ξ^- in an emulsion. It has a hybrid emulsion-counter design utilizing the high purity K-beam (>90%) from the D6 beamline. It will produce an estimated 100 doubly strange hypernuclei of various mass A, a factor 10 more than previous experiments. It will provide information on the Λ-Λ interaction in nuclei, will search for evidence of the H^0 bound to a nucleus through study of the A-dependence of the Λ-Λ binding energy and decay modes, and will measure Ξ^- atomic X-rays, whose peak energies and widths are sensitive to the Ξ^--nuclear interaction. It will improve the measurement begun by E373 at KEK of the Λ-pair invariant mass distribution near threshold.

This is a resubmission of a proposal that was initially approved by the PAC in 2001 as a "compelling experiment" but was never run. While the physics goals of the resubmitted proposal remain the same, there have been significant technical advances since the initial proposal which will speed up the data analysis considerably. Double-sided silicon strip detectors provide higher resolution tracking than the fiber bundles in the previous design, thereby decreasing the rate of false tags of stopped Ξ^- by a large factor, and the automated scanning algorithms have been accelerated. The overall gain in speed to scan an emulsion plate is a factor 10. In addition to improvements in detection and analysis efficiency for stopped Ξ^-, the coverage of the Ge/BGO array has been increased by a factor 2.

We find the physics addressed by this proposal to be compelling. The measurements have implications for strangeness in neutron stars, and the Λ-pair mass distribution near threshold is a unique probe of the Λ-Λ interaction. The collaboration has a proven track record in the application of hybrid emulsion techniques to the study of hypernuclei. The recent technical changes are well chosen and will improve the performance of the apparatus significantly. The D6 beamline is expected to produce the requested K- purity at the requested intensity. Funding for the experiment from Japanese sources has been approved and the collaboration can be ready for beam in November 2005.

P969: Measurement of the μ Anomalous Magnetic Moment to 0.2 ppm

The PAC enthusiastically recommends approval of P969 with its highest "must do" ranking for its physics goals and their potential impact. It strongly encourages the Lab to seek support and embark on this experiment as soon as possible and to make it a very high priority.

Experiment E821 successfully completed its measurement of the muon anomalous magnetic moment (g-2) with a precision of 0.54 ppm, about a factor of 14 improvement over the classic CERN results of the 1970s. It finds a provocative 2.7 σ deviation from the Standard Model prediction, even when hadronic loop uncertainties are included. Such a discrepancy is suggestive of a large new physics effect, with supersymmetric loop contributions representing the most natural candidate explanation. If that interpretation is correct, it has dramatic implications for future collider studies, dark matter searches, flavor-changing neutral current reactions etc. In addition, it would represent a first sighting of a fundamental new space-time symmetry in Nature.

Proposal P969 aims to further improve the experimental determination of the muon g-2 by an additional overall factor of 2.5. That level of improvement is well matched to anticipated reductions in the theoretical hadronic loop uncertainties that will benefit from several sources of new e^+e^- and $\gamma\gamma \rightarrow$hadrons data expected to become available during the next several years. Together, such improvements in theory and experiment could elevate evidence for new physics in g-2 to the more robust 5 sigma level, or if the discrepancy fades, lead to an important stringent constraint on potential new physics which future high energy physics discoveries would have to confront. In either case, the proposed improvement would be very important for the coming LHC era when roughly the same scale of new physics accessible to the muon g-2 will be directly probed in high energy pp collisions.

The PAC unanimously agreed that the physics motivation for P969 was very strong and that the proposed upgrades and running strategy were well formulated. The collaboration draws on the experience gained from E821 and the existing storage ring infrastructure that represents a major financial investment. The PAC believes that the P969 collaboration is capable of accomplishing its physics goals if properly supported. It feels that the required upgrades and AGS running need to be carried out in a timely manner. Otherwise, the expertise of the collaboration might be lost. Also, the physics it explores is naturally matched to possible LHC discoveries that are expected by the end of this decade.

P970: Deuteron edm

This innovative experiment proposes to measure the electric dipole moment d_D of the deuteron D^+ at a sensitivity of 10^{-27} e cm, sufficient to detect the violation of *P* and *T* symmetries at a level predicted by theories beyond the Standard Model. The proponents argue that the detection of d_D at the proposed level "is at least one to two orders of magnitudes more sensitive than any current electric dipole moment limit." The potential impact of observing a nonzero edm on new CP-violating physics would thus be very high.

The proposed experiment requires the construction of a new storage ring where the deuteron polarization and applied electric and magnetic fields can each be controlled to high precision. A key feature of the proposal is the introduction of a radial electric field at a "magic strength" to cancel the much larger anomalous magnetic moment spin precession of the stored deuteron due to the external magnetic field.

The proponents have done an excellent analysis of the various ways that variations of the external fields and deuteron polarization can be employed as experimental checks. However, the experiment is technically very challenging, requiring tight control of systematic effects as well as adequate statistics.

Because the experiment requires a new facility, its capital cost is significant, estimated by the proponents at $35M. Considering that this would be a beginning of a major program, the actual total cost of the experiment in the judgment of the PAC is likely to be much larger.

The PAC was impressed by the presentation and concept of this proposal. However, we are not convinced that the proposed experiment will be able to compete favorably for funding, and possibly scientifically, against alternative proposals, given the advantages of these other proposals. Some other proposals are substantially less costly, or exploit new or planned major facilities, or have the support of collaborations with established track records in the proposed technique. Some of these other approaches have stated goals that place them at or beyond the design goals of the current proposal, in comparable time frames.

The proposers compared their deuteron edm experimental sensitivity with existing neutron edm and other limits. While this is valuable, the competitiveness of the experiment will depend on comparisons to other efforts now underway or planned for the next decade. For example, a collaboration has formed to pursue a new neutron edm experiment at a specialized facility at the Spallation Neutron Source. The experimenters project a factor of 500 improvement over current limits (established around 1990). Such improvement is plausible as it is consistent with the general trend of a factor of 10 every eight years. Experiments on diamagnetic atoms are also expected to make substantial improvements in the next decade. Developed technologies such as the mercury cell approach should yield modest improvements in the very near future (a factor of

4 to 5 $\times 10^{-29}$ ecm), while new approaches, such as the Princeton program on ^{129}Xe, have intermediate-term goals of 10^{-30} to 10^{-31} ecm and long-term goals of 10^{-33} ecm. Still other programs are focused on existing and planned radioactive beam facilities, where additional sensitivity can be gained by studying nuclear systems with unusually large polarizabilities. When one takes into account the underlying sensitivities to CP-violation parameters, several of these efforts will provide strong competition.

On balance, the PAC does not recommend approval of this experiment.

LoI: Search for the Pentaquark

The LOI proposes a search for the 2^+ resonance by means of a “formation experiment”, in which the 2^+ - assuming it exists - is produced through one of its known main decay channels, K^+n. Such experiments can be “definitive” in the sense that their ability to confirm or refute the existence of the 2^+ is only limited by the achieved energy resolution, but not subject untestable assumptions. The PAC encourages the preparation and submission of a proposal that could be carried out in a timely fashion and achieve a meaningful upper bound on the 2^+ width (significantly less than the present bound of about 1 MeV).

The proposed experiment makes use of an existing detector (E949) and an existing K^+ beam line (LESB3). It plans to use a beam with a wide energy spread (due to energy loss in the target) and measures the invariant mass of the produced state by observation of the decay products. Since the mass resolution is larger than the anticipated width of the 2^+, the observed signal will be proportional to the intrinsic width of the resonance. In principle, the experiment could also be able to determine the spin, and maybe even the parity, of the 2^+, if it exists.

The simulations of the expected signal, given the detector response and acceptance, are in an early stage. It is important to demonstrate the achievable upper limit on the resonance width and the accuracy with which the width could be measured if a signal is observed. In particular, it will be important to understand how the reachable limit depends on the spin and parity of the 2^+ and on the relative phase between the background and the resonance. A critical limitation may derive from the ability to measure the energy and angle of the “recoil” proton. The proposal should also spell out the commitment of members of the E949 detector collaboration to the successful operation of the detector and clarify how a timely analysis of the data to be taken can be assured.

LoI: Neutrino Scattering and Cross Sections

This letter of intent describes a new effort to measure the strange quark contribution to the spin of the proton and the Q^2-dependence of the proton axial form factor. The experiment will also measure neutrino-carbon charged and neutral current cross sections in the quasifree and single pion production modes that would be of considerable value for future and ongoing experiments. It is possible that kaon production cross sections may also be obtained.

The experiment provides a unique opportunity to measure the isoscalar axial form factor of the proton, a fundamental observable in proton structure. A sensitivity of the measurement of the strange quark contribution to the spin of the proton $\Delta s = \pm 0.04$ is anticipated once planned Jlab measurements of F_2^s are completed. A new concept for a fine grain tracking detector coupled with the intense low-energy neutrino beam available at the AGS has great promise for making measurements with this precision feasible at sufficiently low momentum transfer.

The PAC considers the physics of this experiment compelling. While deep inelastic scattering provides tantalizing information about the spin carried by the strange quarks, the proton isoscalar axial form factor can only be uniquely obtained by such a measurement. It is essential that a full proposal for this measurement strive to achieve the best possible precision to be definitive and we hope that the projected error of $\Delta s = \pm 0.04$ might be improved. We encourage the collaboration to fully consider what can be done to achieve this. This should include better understanding of the backgrounds and the variety of nuclear effects and the theoretical relation between the isoscalar axial form factor and the strange quark spin. The collaboration should consider the need for antineutrino data to address some of the background processes. We would also expect the collaboration to quantify the physics goals for neutrino-nucleus cross sections.