Recommendations
Brookhaven National Laboratory
Nuclear and Particle Physics
Program Advisory Committee
12-14 September 2006

1. Responses to Presentations

On September 12, 2006, the PAC heard proposals for the RHIC complex and descriptions of letters-of-intent for two electric dipole moment experiments.

LoI: Search for an Electron EDM with PbF

This letter proposes a search for an electron electric dipole in the paramagnetic molecule PbF. Electric dipole moment studies are of keen physics interest, and are being pursued by a number of groups around the world. This experiment involves measurement of the energy splitting of magnetic sub-states caused by the interaction of an electron electric dipole moment with the intense internal electric field of the molecule. The goal of 10^{-32} e·cm is very ambitious. However, the letter of intent is missing several key elements. There is insufficient description of the intermediate goals and milestones required for the success of this project. Though control of systematics is paramount in such an experiment, there was little specific information given about systematic errors and no description of the elements required to meet the ultimate sensitivity goal. In addition, although a National Laboratory could potentially provide invaluable technical resources for any such measurement, there is no compelling reason for the particular choice of BNL as a site for development of the experiment. We therefore recommend that this experiment not be pursued further by the BNL group.

LoI: Search for a Deuteron Electric Dipole Moment Using a Charged Particle Storage Ring

This letter proposes a search for a deuteron electric dipole moment using a stored beam. The goal is a statistical precision of about 10^{-29} e·cm; an appropriate level for an experiment we expect would take a number of years to develop. In this experiment, a longitudinally polarized beam develops a vertical spin component due to the torque of the motional electric field in the ring bending magnets acting on the electric dipole moment. The PAC is enthusiastic about this ingenious new approach to electric dipole moment searches. Because it is a new technique, however, there will be a daunting new set of false edm effects and associated systematic errors to consider. We believe it is very important to identify the most important of these difficulties and address them with a combination of simulation and measurement. We strongly encourage the collaboration to investigate the options for measurements in existing rings with polarized deuteron beams. Development of a program of simulations and tests should include, but not be limited to, complete characterization (intensity, size, energy, polarization) of the tails of the beam and their effects on the measurement, investigations of resonant extraction, considerations of correlations between energy and position in the 'extraction' region, and characterization of the effects of common lattice imperfections. Indeed, short of implementing the resonant enhancement of vertical polarization described in the proposal, measurements of zero left-right asymmetries at the requisite level must be demonstrated. A clear plan for near-term milestones including consideration of these issues (over perhaps a two-year period) should accompany any request to the laboratory for continued support.

R20: Monopole Search

Although the PAC appreciates the advantages of the proposed detection technique – which relies solely on the intrinsic character of the monopole – it did not find the present document to be of sufficient detail to be evaluated as a proposal. Rather the level presented seemed more appropriate as a letter-of-intent.

To be evaluated as a proposal, the proponents need to give considerably more detail on both the motivation and the experimental sensitivity. Some comments on aspects of these are listed below.

<u>Motivation</u>

The observation of a magnetic monopole would be a profound discovery in physics. However, it is very important to compare the goals of the proposed experiment with what has already been achieved in earlier experiments, for example at FNAL, in a more complete fashion than in the present proposal. What parameter space for monopole searches is addressed by the present proposal and how does that compare with the previous searches? These parameters include the mass and cross section excluded by a negative result, the binding energy of monopoles to ordinary matter, and the frequency with which pairs of monopoles are captured in matter with zero net magnetic charge as a result.

The assumptions about production mechanisms for creating monopoles need to be clearly stated. For example, if Drell-Yan production is as effective as photon-photon production, the highest possible accelerator energy would seem to be favored. What advantages and disadvantages are there in carrying out this experiment at RHIC?

Also, in the proposed method, using (virtual) photon –photon collisions, the cross section estimates should be more clearly explained. In particular, the assumption of coherent production for monopole masses up to 10 GeV should be justified.

Experiment

Since there is little experience with the operation of a SQUID detector in the environment of a RHIC experiment, some experimental tests would be extremely valuable and should be carried out by the proponents. The results could well lead to modifications of the experimental design or to reevaluations of the sensitivity of the search.

With improvements along the lines suggested, the proposal could be resubmitted.

STAR and PHENIX FY07-FY09 Beam Use Proposals

FY07: 8 weeks (or as close to 8 as permitted by the FY07 budget) of
polarized p+p at $\sqrt{s}$=200 GeV, followed by
15 weeks physics running with Au+Au at $\sqrt{s}$=200 AGeV
FY08: 12 weeks (physics) d+Au at $\sqrt{s_{NN}}$=200 GeV, followed by
11 weeks (physics) polarized p+p at $\sqrt{s}$=200 GeV
FY09: 12 weeks (physics) of Au+Au, possibly at lower energies to search
for the QCD critical endpoint, followed by
10 weeks of polarized p+p at $\sqrt{s}$=200 GeV and a short polarized
p+p commissioning run at $\sqrt{s}$=500 GeV.

In its recommendation the PAC has been guided by the following considerations:

Both PHENIX and STAR request a long, high luminosity Au+Au run in Run 7 to explore physics questions that become accessible due to recently added detector capabilities. PHENIX will use the Hadron Blind Detector (HBD) to measure low-mass dielectrons, while STAR will use added tracking capabilities from the SVT plus SSD to explore and separate the charm and beauty contributions to non-photonic electron production. Furthermore, STAR will use the new Forward Meson Spectrometer (FMS) to study gluon saturation physics at forward rapidities in d+Au collisions.

The PAC places highest priority on Au+Au in Run 7. PHENIX will measure low-mass dielectrons to explore medium effects on vector mesons and chiral symmetry restoration effects in the hot and dense late hadronic phase, as well as low-mass thermal dileptons from the early QGP phase in Au+Au collisions. STAR will explore and separate the charm and beauty contributions to non-photonic electron production. Both collaborations will use the high statistics Au+Au data to see the response of the medium to penetrating high-p_t probes via three particle correlations, and in events containing a high-pt trigger photon.

While STAR and PHENIX roughly agree on the length of the requested Au+Au run in Run 7, they disagree on its timing. Running Au+Au beams at the beginning of Run 7 would compromise the low-mass dilepton capabilities of PHENIX as a large fraction of the Au+Au would be lost to commissioning of the new HBD detector. Running Au+Au second compromises the usefulness of d+Au in Run 7 for STAR because the FMS, which is needed for the gluon saturation studies in d+Au, is not fully operational at the beginning of the run and needs several weeks of commissioning. On the other hand, postponing d+Au collisions until Run 8 removes reference data for the beauty vs. charm separation in non-photonic electron suppression, as the STAR SVT will be removed after Run 7.

Each of the running sequences proposed by STAR, PHENIX, and C-AD has serious negative effects for one or more aspects of the physics program. Some of these difficulties are unavoidable consequences of the loss of Au+Au running in Run 6, due to budget constraints. The PAC thus had the difficult task of identifying lengths and sequences of different beam periods to minimize the long-term damage to both the heavy-ion and spin parts of the PHENIX and STAR physics programs.

The STAR request for d+Au running during Run 7, with both the SVT and FMS fully operational, cannot be satisfied without compromising the Au+Au low-mass dilepton program of PHENIX. In the PAC's opinion the low-mass dilepton program with the PHENIX HBD has high priority, and its successful completion during the next three running periods requires a successful high-statistics Au+Au run during the second part of Run 7. It is true that by postponing the d+Au run to Run 8 one loses its value for STAR as reference data for the non-photonic electron suppression in Au+Au collisions. One preserves, however, its usefulness for making a timely and unambiguous assessment (before the LHC heavy-ion program begins) whether gluon saturation effects play a significant role at RHIC at forward rapidities (and thus at LHC at mid-rapidites), and to this the PAC assigns a high value. The STAR proposal for d+Au running during Run 7 is squeezed from both sides (at the front end by the late availability of the FMS, at the rear end by the removal of the SVT during the shutdown between Runs 7 and 8) and leaves no margin for failure or reduced capabilities of any detector components. For the coming 3 years, the PAC assigns slightly higher priority to obtaining a complete set of low-mass dilepton measurements with the HBD than a complete set of Au+Au and d+Au for charm and beauty production at intermediate pt with the SVT. These latter measurements can and will be performed with higher precision and statistics after 2010 when the PHENIX vertex detector and STAR Heavy Flavor Tracker become operational. The PAC recognized the challenges of the SVT analysis as presented by the collaboration. Our recommendations reflect the above listed considerations and priorities.

The PAC's recommendation includes about 30 weeks of polarized proton running over the next three years, with p+p physics runs during each of the running periods. This permits continuous development of the polarized proton beam luminosity in the RHIC collider over the coming three years and should allow for the successful completion of both the STAR and PHENIX spin programs with $\sqrt{s}=200$ GeV polarized protons. Short polarized p+p commissioning runs at $\sqrt{s}=500$ GeV should be performed in Run 8 and/or Run 9 as beam time allows.

2. Recommendations for Future Planning

The PAC was asked to provide advice on the developing science cases for the RHIC II luminosity upgrade and the Electron-Ion Collider. We heard four presentations: physics opportunities with polarized e-p and e-A collisions at an electron ion collider, the physics that becomes possible with the increased luminosity of RHIC II, and how the physics of ep, eA and AA is related. We offer here some observations and suggestions for optimizing future presentations of the science case for the Nuclear Physics Long Range Plan. The PAC is concerned that an overarching plan for making the QCDLab case has not yet emerged.

It is important for electron-ion collider presentations, especially for eA physics, to clearly indicate the size and breadth of the core group of experimenters and their focused experimental goals. It is critical that an audience from the broader Nuclear Physics community see that such a group exists and that experimental approaches have been studied extensively. It will be important that the goals and progress toward developing an experiment(s) for ep and eA be discussed.

The Long Range Plan: Polarization Physics at an Electron-Ion Collider

In addition to the ongoing efforts, the Lab must take a more visible, active, and creative role in leading the development of the physics case for EIC. We were disappointed to find that the PAC's March 2006 recommendation along these lines has not been implemented. Now that the plans for RHIC II upgrades have been embraced by DOE and the NP community, it is time for the Lab to make planning for EIC its highest priority for a future facility in NP. We urge the Laboratory to follow our March 2006 advice and empower a high level group to work in this direction. With the NP long range planning process now underway, time is short. In addition, the Committee recognizes that lines of communication are open with corresponding planning at the Jefferson Laboratory, and we encourage further strengthening and formalizing of these exchanges.

Many ingredients of a compelling physics case were presented, however, the PAC felt that they have not yet coalesced to a point to ensure the support of the broader nuclear physics community. It would be useful for future EIC presentations to outline the parameters for the machine, explain the motivation for pursuing deep inelastic spin physics to higher energies, and present a few specific targets for *Phys. Rev. Letters* level results. Possible topics would include: a test of the Bjorken Sum Rule at the ~1% level; precision measurement of the gluon spin contribution to the nucleon spin especially at values of *x* lower than now possible; systematic study of transverse spin at leading twist; possible measurement of the quark orbital angular momentum in the nucleon via DVCS. There may well be others that we are not aware of. The importance and feasibility of each has to be demonstrated, and it is important not to claim too much. For example, it seems very unlikely that all four components of the nucleon spin will be measured anywhere unless there is a breakthrough in the theoretical understanding of orbital angular momentum.

The case for polarization physics at EIC should build upon and extend the world-wide program. There should be particular emphasis on how it extends the RHIC Spin program, without diminishing the importance of that program.

The eA Program and the EIC

The motivation for an EIC grows in large part out of the ongoing RHIC program and the electron-proton program at HERA. As quantum chromodynamics describes electron-nucleon and nucleon-nucleon scattering in terms of evolution of parton distributions, one expects overlap of the EIC proposal with the RHIC and RHIC II programs. In both theory and experiment the overlap between the eA, pA and AA programs could reflect any novel dynamics in both initial and final states. An additional important goal is to determine nuclear structure functions in the new kinematic range opened by such a facility.

Successful inclusion of this program in the nuclear physics priorities, however, will require fleshing out the parameters of the proposed accelerator facility, far beyond what was shown at this meeting. It is crucial to address developing progress and prospects in the facility capabilities, and explain the significance of its anticipated physics output in the light of the history of measurements of nuclear structure functions and diffraction, as well as the output of RHIC and RHIC II. At a minimum, what is necessary is a quantitative description of landmark results, and their implications for contemporary ideas in nuclear physics and quantum chromodynamics. Significant figures of merit should be identified to quantify success in addressing these ideas. What will the first few *Phys. Rev. Letters* address? What measurements at what precision are needed to write those Letters?

Presentations to the NP community should include a description of the fundamental spaces of parameters central to the physics in question, the coverage of those parameters by past and present facilities, and the potential coverage afforded by the new facility.

Most audience members will be familiar with plots showing ranges in x and Q^2, but more is needed. For discussion of the range of predictions for the A-dependence of the saturation scale $Q_s(x)$, a key theoretical concept in the description of high-density collisions, would be helpful. To make an informed judgment on the status of this proposal, it would be highly desirable to be able to gauge the effects of such a variation on the observables that make up the experimental program of an EIC.

Sharpening the arguments for this facility will have to be the fruit of regular and formalized exchanges between theoretical, experimental and accelerator physicists. The need for an organized group to develop these arguments at Brookhaven, bringing in substantial outside participation, is pressing. We recommend that such a group be formed immediately. To the extent possible, this should involve Laboratory-sanctioned coordination with the existing C-AD efforts on an EIC, and the corresponding RHIC II program. This group should be charged to prepare for presentations at the upcoming NSAC meetings. Both the EIC and RHIC II programs will benefit from such coordination. The PAC notes the existence of an energetic program of ongoing workshops, held most recently at BNL and urges the laboratory to build upon the results.

In addition, the Committee recognizes that lines of communication are open with corresponding planning at the Jefferson Laboratory, and we encourage further strengthening and formalizing of these exchanges.

RHIC II

"RHIC II" refers to the implementation of electron cooling that will increase the RHIC Au+Au luminosity by a factor ~10 and the p+p luminosity by a factor ~3. Upgrades to the STAR and PHENIX detectors, now in progress, will provide continuous improvements to the experimental capabilities for both heavy ion and spin measurements. The experimental upgrades are essential to exploit the increased luminosity provided by RHIC II. The PAC did not discuss these upgrades in any detail at this review, but in the past has found that they are well-motivated and will maintain RHIC at the scientific forefront.

Overall, the PAC feels that the scientific case for increased luminosity is very strong. The program will build on the discoveries made at RHIC thus far and promises a much more detailed, quantitative understanding of the properties of dense QCD matter than we have today. The PAC finds that, with the RHIC II upgrades, RHIC will provide important experimental capabilities complementary to those at the LHC. In fact, these programs should not be viewed to be in scientific competition. In analogy to condensed matter physics, much more will be learned from probing QCD Matter generated under vastly different conditions at both facilities than will be learned from one of the facilities alone.

Heavy ion studies at RHIC require a broad variety of techniques and it is easy for the uninitiated to get lost in the details. The RHIC II presentations to the broader NP community should be tuned to emphasize the essential underlying physics pictures, concentrating on a limited subset of key measurements that hold the most promise for future progress. At the same time it is important to be quantitative. An excellent example is the recent discussion of upgrades and luminosity projections for the spin program, where specific, quantified benchmarks were identified. The community should continue in its efforts to quantify the physics reach of RHIC II, comparing where appropriate to what has been achieved at RHIC to date and to projections of LHC heavy ion capabilities. Some examples where a quantitative discussion would strengthen the case include:

- what is the expected reach in transverse momentum of the γ+jet measurement, and what advance will it provide over existing jet quenching measurements?

- what is the precision to which charm flow will be measured, and what are the model predictions (with uncertainties) among which it can discriminate?

- what is the precision with which the onium states can be measured, and how well will their measurements address expectations from lattice QCD for a hierarchy of melting temperatures?