Review #1

Proposal Number:	1229424
NSF Program:	Major Research Instrumentation
Principal Investigator:	Horn, Tanja
Proposal Title:	MRI Consortium: Development of a Neutral Pion Detection System for Hall C at 12 GeV JLab
Rating:	Good

REVIEW:

What is the intellectual merit of the proposed activity?
Strengths

In this MRI proposal, the PI and her colleagues request funding to augment the capabilities of Hall C at 12 GeV JLab through the addition of a photon / neutral pion detection system. The design proposed is very economical, in that the actual detector elements (~1100 PbWO4 crystals and their associated photomultiplier tubes) are being 'recycled' from the existing HYCAL detector that had been used in Hall B for the PRIMEX series of experiments. Moreover, the proposed device will be cantilevered off of the SHMS carriage, so a dedicated motion control system is not needed. Based on the demonstrated performance of these crystals û though while operating in a different environment - it seems quite likely that the spatial (directional) and energy resolutions needed for the proposed research program can be achieved. This device would thus make possible a few photon (e.g., DVCS) and neutral pion production experiments that are slated to run during the first few years of the 12 GeV JLab era.

The intellectual merit of this activity, that is, building the proposed device, is obviously strongly coupled to the intellectual merit of the experiments that it would enable. There is already an approved, high-priority program at Hall C for studies of exclusive production of charged pions and kaons, focusing on L/T separation over a broad range in Q^2 to test the validity of QCD factorization (i.e., decoupled hard and soft processes) at the higher energies attainable with the 12 GeV upgrade, and the PI is playing a leading role in that program. By carrying out a similar set of measurements for the exclusive production of *neutral* pions, one should be able to gauge quantitatively the importance of the non-pole contributions to pion production. As noted in the proposal, understanding where, kinematically, factorization can be assumed to work is critical if one is to have any confidence in the theoretical interpretation of much of the 12 GeV JLab program. Analogous measurements performed at 6 GeV, it should be noted, led to ambiguous results. The PI made substantial contributions to this lower energy program, so it is very reasonable for her to lead new efforts at the higher energies. The ability to reconstruct neutral pions, detected in coincidence with the scattered electron, will broaden the reach of these studies.

More generally, one of the primary goals of the JLab program is to begin describing the spin and flavor structure of the nucleon in terms of the Generalized Parton Distribution (GPD) functions.

A potent tool in this effort will be exclusive meson and photon production; but these measurements will be of limited value if one is not confident that QCD factorization is valid in the kinematic regions under study. Mapping out the Q^2 dependence of the L/T separated cross sections for meson production should provide critical information towards establishing the boundaries of these regions.

Weaknesses

As noted above, there is no question that studies of the onset of QCD or "hard/soft" factorization, and understanding where this occurs, are vital to the health of the 12 GeV JLab program, and I am strongly supportive of these efforts. I am less convinced, however, that the neutral pion measurements are so essential to this broader effort that one can justify building a new detector essentially dedicated to this task. A key issue for any detector development proposal is to ask if there is a demonstrated and strong need for the new instrument in the larger user community. While I dislike experimental proposals that simply include 'laundry lists' of everything one might possibly learn from a particular measurement, I believe it is very important in an instrumentation proposal to provide some indication of how the device might be used, beyond the specific measurement that is of interest to the PI's. Put another way, it is easier to endorse proposals for devices that will enable entire programs, rather than a particular measurement. In my view, this detector falls somewhere in between the two.

It is also not obvious to this reviewer that one can take the performance of these crystals when operating in one environment (as part of HYCAL in the low luminosity Hall B) and extrapolate to the more severe environment one encounters sitting directly downstream of a much higher intensity beam-target interaction point. Due to increased single-photon backgrounds, and the resulting (and rapidly growing) combinatorics of two-photon coincidences, it is non-trivial to estimate reliably the pion reconstruction efficiency, which can be expected to be highly momentum dependent. Depending on the source(s) of the background, there can also be significant position / angle-dependent effects as well. It is stated in the proposal that "several background simulations were performed," though no details are given as to what was actually done, or whether the simulated conditions were based on empirical measurements (for example, from instrumenting and reading out the response of a single PbWO4 crystal placed in the hall during real data taking). Without this information, it is difficult to interpret what a "worst case scenario" means, or to evaluate how encouraged one should be from the results shown in Fig. 3b. At a minimum, it would be useful to know the estimated occupancy of the ~1100 cells for a given (low) threshold and when integrated over a few hundred ns, and to show how this quantity varies with assumptions about the backgrounds.

These are more than just minor concerns. High combinatoric backgrounds lead not only to false positives (the 10% underlying the peak in Fig. 3b) but also to false negatives, and a real loss of efficiency. It would have been useful to know what fraction of the pions thrown in simulation wound up inside the peak of interest, and how well that fraction can be determined. An L/T separation measurement in the neutral pion channel may not only be statistically hungry, but will be very sensitive to these sorts of systematic uncertainties also.

What are the broader impacts of the proposed activity?
Strengths

On the scientific front, the immediate impacts of the proposed activity (i.e., building a photon detector) are fairly narrow, and will benefit primarily only those who seek to use this device to carry out certain measurements at Hall C at JLab. To the extent that these measurements contribute to our understanding of QCD, however, and provide important insights into the transition region where hard/soft factorization becomes a valid framework for interpretation of data, the proposed detector will enable measurements that are of great value to the entire nuclear and particle physics community.

Of equal significance, it is important to point out that many of the specific tasks that will be associated with this project, such as assembly and testing of the new amplifiers and voltage dividers to be incorporated into the PMT bases, will be performed by students, most of whom will be undergraduates. These and similar tasks (testing the digitalization electronics, mapping the sweep magnet, etc.) lend themselves quite well to work by students, who can both understand and be in control of their particular contribution to the effort, yet can feel they are a part of a much larger endeavor. All of the lead institutes on this proposal (CUA, ODU, FUI) can provide such experiences for their science-minded students.

Finally, the importance of having more women and other members of under-represented groups serving as PI's, experimental spokespersons, and mentors to grad students and undergraduate majors can not be over-emphasized. All of the institutions involved with this proposal, and the PI in particular, should be applauded for their efforts in this direction. The PI has obviously put in much time and effort, and drawn from her own experiences, to find creative ways to involve students at all levels and from a variety of backgrounds in this project. It may be a clichÚ, but she has done far more than just 'talked the talk' û she truly has walked the walk.

Weaknesses

None of any significance.

Program Specific Criteria

For instrument and development proposals: 1) the adequacy of the management plan; 2) the availability of appropriate technical expertise to design and construct the instrument; 3) the appropriateness of the cost of the new technology; 4) the need for development of a new instrument.

Comments on both strengths and weaknesses

Strengths:

For this instrument development proposal, I believe the construction of the device to be fairly

low risk, while there is a higher risk that it may not perform as a neutral pion detector to the level specified in this proposal (and needed for the associated experiments). The management plan for construction is reasonably robust: if this instrument were being built 'from scratch,' the greatest concerns would be obtaining the PbWO4 crystals and their PMT's from foreign suppliers in a timely fashion, and ensuring that a sufficient fraction of these met specifications. By reusing existing components, this is no longer an issue. Most of the other construction tasks, such as the temperature-controlled frame, the cantilevered support platform, and the new sweeping magnet, are well-established technologies, and should pose little risk to the consortium institutes that will oversee these projects, or to the JLab staff that will assist them. It was less clear that all the plans had been thought through at the necessary level of detail for the pre-amplifiers, fast ADC's, and the digitizing electronics, but again, none of the proposed techniques are truly state-of-the-art or require advances in electronic components beyond existing capabilities. Given the scale of this project, the consortium is reasonably large and diverse in the skills they bring to bear on this effort, and construction and installation of the detector should not present any significant challenges to the Hall C technical support staff.

The proposed schedule for component and material procurement, followed by assembly and installation, is very realistic. While there could always be minor delays or price increases, I do not believe there are any hidden show-stoppers in this proposal.

Weaknesses:

I find no real gaps in the management plan. The only concern I have with its eventual installation and use is the ease with which it can be moved and/or removed. There are probably ways that this instrument, its temperature-controlling frame, and its support platform can all be designed such that it is truly 'easy' to remove or re-install the device - but I am sure these are not the cheapest or fastest ways to build them. These sorts of mechanical issues were not addressed at all in the proposal, and I can only assume that the JLab staff has implicitly agreed to make sure that all of these aspects of the design will be worked out and implemented. There was also a very brief mention of even using the proposed photon detector in other halls, though I suspect this is not very realistic.

Summary Rationale for the Rating

The PI and this consortium have done an excellent job in devising a plan for constructing a photon / neutral pion detector that is very economical, fairly low risk, and that will add a new capability to the Hall C physics program in the 12 GeV era. The management plan and construction schedule are very realistic, and it is quite likely that this instrument would be ready for use before the JLab facility is ready to use it. This group of researchers also has a truly outstanding track record of involving students in meaningful ways on projects of this scale, including important efforts to bring in more women and other groups that are currently under-represented in physics.

Nevertheless, it is not obvious that the physics that this device will enable is as compelling as much of the rest of the Hall C 12 GeV program. It will augment what is learned from the charged

pion and kaon studies, but only if it can provide information on reconstructed neutral pions that is comparable in quality to that obtained for the charged mesons with a magnetic spectrometer. It also seems, as far as I could discern from the JLab PAC records, that of all the experiments rated in the top half of the priority list (and which will therefore run in the first five years of 12 GeV operation), there is only a single experiment, conditionally approved, that will require use of this instrument.