Proposal All Reviews: 2216566

Back to Proposal

Agency Name:	National Science Foundation
Agency Tracking Number:	**2216566**
Organization:	
NSF Program:	Major Research Instrumentation
PI/PD:	Niculescu, Maria
Application Title:	MRI Consortium: Development of an enhanced magnetic-spectrometer-free apparatus for an ultra-precise proton charge radius measurement

Review 1

Rating:

Very Good

Review:

Summary

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to intellectual merit.

In this MRI Instrument Development proposal, funds are requested to upgrade several of the detector components that proved so successful for the PRad measurement at Jefferson Lab. Based on that success, the PI's have requested, and received, significant additional beam time from the JLab PAC to mount and carry out PRad-II. The detector improvements requested in this MRI proposal would allow them to take full advantage of that opportunity.

As with most instrumentation requests, the intellectual merit of the proposed activity – designing, fabricating, installing, and commissioning the new instruments – is determined primarily by the quality of the science that it will enable. In this case, the science is focused almost exclusively on achieving a more precise determination of the proton charge radius r_p, with higher statistics and reduced systematic uncertainties than were possible in the original PRad measurement. It is not necessary in this review to try and summarize the current status of the various r_p experimental programs around the world, or to describe how (and why) the world-average value of this important parameter has 'evolved' since 2010. It is sufficient to simply point out the attention received, in both esteemed physics journals and in the more popular science magazines, by the ground-breaking (in their precision) results from μH spectroscopy in 2010, and the subsequent interest in the PRad experiment that seemed to confirm these surprising results in an e-p scattering measurement. There is no question that obtaining a statistically precise determination of r_p, via a method in which all systematic uncertainties are well understood, is a goal of very high scientific merit.

That said, I do have some concerns about the actual impact of the proposed measurement with an upgraded PRad detector system. The original PRad result was extremely important, as it moved the best e-p scattering data point into agreement with the truly exquisite μH results; it, in effect, 'solved' the proton radius puzzle. The fact that most of the more recent eH spectroscopy measurements are also now consistent with those from μH has provided the proverbial nail in the coffin. Assuming the PRad-II result will turn out to be statistically consistent with that of PRad, it is not obvious what will have been learned, given the unreachably small errors achievable using muon spectroscopy. One might argue – admittedly, playing devil's advocate – that the only remaining puzzle is why the pre-PRAD e-p scattering measurements got it wrong.

I also have concerns about the importance of the proposed upgrades for measurements beyond that of PRad-II. There are a few sentences noting some additional work that could be done, such as extracting the charge radii of other light nuclei; but nothing is discussed in any detail, and very little motivation is provided for these measurements. This sort of information may not be needed when arguing for PRad-II beam time in front of a PAC, but it would have helped significantly when trying to justify an MRI proposal as being a cost-effective upgrade that would benefit the broader JLab program. For example, would these detector improvements be of use in a new measurement of the Primakoff effect, a prime driver of the original PRad apparatus?

In short, I believe it can be argued that the PRad-II experiment is a very important, though not essential, measurement in the quest for a 'final' value for r_p, and one can now turn to the issue of whether this importance justifies the costs of the upgrades discussed in this proposal. It seems clear that some components of the proposed upgrade are crucial if the hoped-for level of improvement, compared to the PRad result, is to be achieved. The two new GEM detectors, for example, will provide much more complete information on the transverse components of charged particle tracks heading into the HyCal. The change from relying on a single plane to a pair of planes, especially if the latter are separated spatially along the electron path, will help with event selection, z vertex determination, background suppression, and other more indirect effects that are noted in the proposal. Moreover, this would be a fairly inexpensive and low-risk upgrade that would almost certainly benefit any other measurement that used this apparatus. The UVA group has extensive experience working with the technologies required to design and construct such detectors.

In a similar way, it is easy to see multiple advantages to the proposed conversion of the HyCal readout electronics from the current FASTBUS system to one based on flash ADCs. While this upgrade will probably lead to some increase in the system trigger efficiency, the real gains are in the reduction of several systematic errors that should be readily achieved. Like the GEM planes, this is an upgrade that is reasonably low-cost and low-risk, a move to state-of-the-art technology, but not so 'leading-edge' that large contingencies in costs and time would be required.

However, what is by far the largest component of the proposed upgrade – the replacement of 360 Pb-glass modules from the top and bottom of HyCal with 1500 PbWO4 crystals – is also, in my opinion, the most poorly justified. If the primary goal of the proposed upgrade is to be able to fine-tune the existing detector so that extrapolation of G^p_E down to a Q^2 of zero can be done with minimal uncertainty, then it is not obvious why upgrades on the outer edges (large scattering angles) of the calorimeter will have much of an effect. There is no discussion at all as to why the top and bottom sections of HyCal are the optimum regions to be replaced: is there a physics justification, or is it purely structural, given the way the modules are stacked mechanically? If one shaved $1M off the cost by not replacing the modules in the corners (i.e., directly above and below the current Pb-glass modules on the left and right sides), would this cause any problems? The reader is left with the feeling that the decision as to which Pb-glass modules should be replaced was somewhat arbitrary, other than to have at least a subset that extended over the full angular range of the HyCal detector. In terms of how well the crystal upgrade will help constrain the extrapolation of G^p_E down to zero, all we are given is the right panel of Fig. 4. If knowing the precise value of G^p_E out near Q^2 ~ 0.05 was an essential part of the proton radius determination, the upgrade would clearly help; but it is hard to see how this information will guide the functional form chosen for the extrapolation to 0. The left panel of the figure shows that the proposed crystal replacement would greatly reduce the ratio of the inelastic to elastic yield at the larger scattering angles. Table 1, on the other hand, indicates that the effect of this would be to take an insignificant contribution to the r_p uncertainty (0.0009) and render it totally negligible. The strongest argument for needing an upgrade to the HyCal crystals, based on Table 1, is the improvement noted in event selection. Unfortunately, there is nothing presented to indicate how much of this reduction in systematic error is due to the HyCal upgrade, rather than that of the GEM planes.

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to broader impacts.

On the scientific side, the potential impact of the proposed activity (providing additional constraints on the proton charge radius) is substantial. This parameter plays an essential role in atomic and optical physics, and a precise determination of its value is key to other high precision measurements. I am less convinced that accurate knowledge of r_p will help advance our understanding of how QCD 'works' in the non-perturbative regime of low-energy bound systems. Calculating the proton charge radius to a few percent of a fermi from first principles (QCD and QED) on the lattice seems a long way off, and I am skeptical that the proposed detector upgrade will "enable QED level precision in QCD studies," even allowing for reasonable hyperbole.

More broadly, though, it is important to note that many of the technical tasks associated with this project, such as design, testing, and assembly of the

GEM tracking detectors, the new readout electronics, and modifications to the current HyCal apparatus, will be carried out by students, or students working closely with more senior scientists. The proposal makes it clear that undergraduates will form a significant part of the work force, which is not true for many similar proposals. The tasks listed above (and spelled out in more detail in the proposal) lend themselves to work by students, who can both understand and be in control of their particular contribution to the effort, yet also feel they are part of a much larger endeavor. All of the lead institutes on this proposal (JMU, MSU, UVA, and NCAT) 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 should be applauded for their efforts in this direction. The MSU and NCAT groups, in particular, have outstanding track records in finding creative ways to involve students at all levels and from a variety of backgrounds in their scientific programs. In this respect, I rate the proposed activities well above the norm.

Please evaluate the strengths and
weaknesses of the proposal with respect to any additional solicitation-specific review criteria, if
applicable

The management plan for the proposed technical upgrades is well conceived and seems quite reasonable. Most of the technologies to be employed have been used before by this group (PbWO4 crystals, GEM planes) or by the support staff at JLab (readout upgrade with flash ADCs). The tasks are distributed among the lead institutes in accordance with the expertise and manpower available at each. By using existing or known technologies and components, the overall project risk is greatly reduced.

Most of the design, construction, and commissioning tasks should fall well within the funding and time windows outlined in the proposal. These will pose little risk to the institutes that will oversee these projects, or to the JLab staff that will assist them. The PI’s and their groups bring a large and diverse set of skills to bear on this effort. Construction and installation of the proposed upgraded detectors should not present any significant challenges to the JLab technical support staff.

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

Summary Statement

The PI’s have presented a reasonable and detailed list of proposed upgrades to the existing PRad apparatus that will enable them to achieve a significant improvement in both the statistical quality and reduced systematic uncertainties in their determination of the proton charge radius r_p. Much of the planned upgrade is economical, low risk, and will almost certainly add new capabilities for potential future measurements that might make use of this device. The management plan is sound and well thought out, and the proposed construction schedule is realistic. The PI’s also have an excellent and demonstrated tradition 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 to me that the physics case for the approved PRad-II measurement is so compelling that it justifies all of the proposed detector improvements. Specifically, the argument for replacing many of the current Pb-glass modules in HyCal with the smaller, more expensive PbWO4 crystals was not demonstrated as cleanly as it should have been, given the expense involved. Their higher resolution would certainly help, in a general sense, and some of the smaller systematic uncertainties would be further reduced, but the high cost of the new crystals (~$3.5M) sets a higher threshold for justifying this part of the upgrade that, in my opinion, was not met in the proposal under review.

Review 2

Rating:

Excellent

Review:

Summary

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to intellectual merit.

The proton radius puzzle has been a major outstanding issue in physics for over a decade. The PRad experiment published results in 2019. By going to much lower momentum transfer (Q^2) values than previously possible in electron-proton scattering, PRad removed the disagreement in proton radius between electron-proton scattering experiments and muonic hydrogen experiments. The PRad result disagrees with other electron-proton measurements but agrees with existing, precise, muonic hydrogen experiments. However, additional measurements using spectroscopy have also provided some new confusion of the proton radius. Given all this, there is clearly interest in having improved precision in the e-p scattering measurement.

This proposal aims to fund a an upgrade of that experimental equipment to be used at JLab to increase the precision of that experiment greatly. The upgrades proposed in the proposal seem likely to improve the precision of the results by decreasing the systematic uncertainties and increasing the amount of data collected.

The team has experience in these areas and is well qualified. It is the appropriate size for the project. The cost of this proposal is large but the budget is reasonable and it seems that with the proposed equipment they would have a high chance of reducing the uncertainty on the e-p scattering proton radius. The PIs have been involved in many construction projects at JLab, including the PRad experiment, to which this is an upgrade.

In the context of the five review elements, please evaluate the strengths and weaknesses of the proposal with respect to broader impacts.

The broader impacts of this proposal are strong. The question of the proton radius impacts many areas of physics due to the different techniques used for measuring and the fundamental nature of the proton.

The group of institutions involved in this proposal includes undergraduate focused institutions and HBCUs. Both of these aspects serve to broaden participation in physics research. The work done by all groups appears to be impactful and well suited to the groups involved. The lead PI is a woman.

Please evaluate the strengths and weaknesses of the proposal with respect to any additional solicitation-specific review criteria, if applicable

The proposed equipment would be installed at Jefferson Lab and would be used for reach at the beams provided there. Given the infrastructure of that lab, the equipment would be well maintained.

The overall budget is reasonable. The largest single item in the budget is the PbWO4 crystals and the PMTs & bases to read them out--this makes sense and is the central feature of the project. The other costs are much smaller and reasonable.

The project management plan seems reasonable and as the team is experienced, seems likely to be successful. The timeline seems mostly reasonable, but somewhat optimistic (especially given the widespread delays now).

The proposed project is well suited to a development program and will advance the measurement capabilities for this physics beyond what currently exist.

The amount of funding for this proposal is large but it is justified by the fundamental nature of the physics aim. The proton radius puzzle touches many areas of physics. Publications of new results in this area have been published in top-tier journals (the initial PRad results appeared in Nature).

Summary Statement

This is an expensive but excellent proposal.

Review 3

Rating:

Very Good

Review:

Summary

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to intellectual merit.

This proposal addresses compelling physics. The proton radius is a fundamental quantity whose value has apparently changed because of accurate experiments with muons. Furthermore, the PRad and PRad-II experiments are pioneering new experimental techniques to accurately measure very small angle scattering without the use of magnetic spectrometers. The first PRad experiment was very successful. It's results, published in Nature, suggest that part of the proton radius puzzle may be due to subtle systematic errors associated with electron scattering experiments using magnetic spectrometers. The PRad-II experiment aims to significantly reduce the error in the charge radius compared to PRad. The physics motivation for PRad-II is very strong and the probability of success is high, given the success of PRad.

The JLAB program advisory committee, while strongly supporting the experiment, noted "The upgrade of HyCal … implies 1500 additional PbWO4 crystals and a new electronic readout. The cost estimate is about $5M. While it is clear that the new readout based on FADC will strongly increase the rate of data taking (and thus reduce the statistical uncertainty), the PAC could not be convinced on the necessity of the costly replacement of the crystals for reaching the final uncertainty on the proton radius. …the PAC strongly supports the request for 40 days of beam time, pending a thorough technical investigation of the actual benefits or necessity of the HyCal upgrade…". A JLAB technical review of the experiment was scheduled and, according to Bob McKeown's letter, the experiment has now received full approval from Laboratory management. I have not seen a report from the technical review. It appears the HyCal upgrade, although expensive, does significantly improve the experiment.

The team is well qualified with considerable experience to perform the proposed activities.

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to broader impacts.

This proposal has several broader impacts. The upgraded HyCal calorimeter and experimental techniques for accurately measuring low momentum transfer scattering can be applied to other experiments. This proposal will help train many young people. I note that a PRad graduate student has won the 2021 APS Dissertation Award in Hadronic Physics. James Madison is a Primarily Undergraduate Institution that provides the opportunity to train undergraduates including minority undergraduates. Mississippi State University is a HBCU and will provide the opportunity to train African-American students.

Please evaluate the strengths and
weaknesses of the proposal with respect to any additional solicitation-specific review criteria, if
applicable

Summary Statement

This is a very strong proposal that builds on the successful PRad experiment to enable an even more precise measurement of the proton's charge radius. This is compelling physics that will likely attract considerable attention. This proposal provides important opportunities to train minority undergraduate and graduate students.

Review 4

Rating:

Excellent

Review:

Summary

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to intellectual merit.

This proposal concerns improving a detector to make a better measurement of the proton radius. This detector indeed would use novel technology.

The key scientific question here is: can the team achieve the necessary accuracy? As a theorist, I cannot address this question in detail. From the PRAD results I know that the team has an excellent record, and the JLab PAC rated it highest, but more than that I cannot say.

My high rating is based on the assumption that the team will be able to achieve the necessary accuracy. Then the question is: how important would the results be?

I believe that the results would be very important, with excellent potential to be very, very important. Indeed much more than that of obtaining a precise value of the proton radius. This is because of the inherent test of lepton universality. When the muonic hydrogen results were announced the discrepancy between the electron and muonic determination of the proton radius were very large relative to other hints of universality violation. No beyond the standard model theory could account for the proton radius puzzle and other possible muonic effects, such as those of the g-2 experiment and some LHC results. With the recent improvements in electron hydrogen spectroscopy that are cited this proposal (e.g the York experiment), it is now clear that possible violations are of similar in size to those of other hints.

Thus a very precise electron-proton scattering experiment could find substantial evidence for violation of lepton universality. Such would be very important for nuclear and particle physics. Furthermore. a discovery of violation could change the nature of many areas of physics beyond particle and nuclear physics.

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to broader impacts.

Using this apparatus would greatly increase the precision of ep scattering. This would have impacts on other studies such as hidden sector searches and the neutral pion transition form factor. Making this device work would provide great training for young researchers. In particular, African-Amerian students at an HBCU would benefit.

A successful experiment that hinted a violation of lepton universality would create wide attention and be a text book subject. But discovering a violation is not a necessary condition for the importance of the experiment. This is because of the connection between scattering and spectroscopy and because of the connection between muons and electrons.

Please evaluate the strengths and
weaknesses of the proposal with respect to any additional solicitation-specific review criteria, if
applicable

The MRI is for equipment. This is an equipment proposal.

Summary Statement

The use of the requested equipment could have wide impacts across many sub-fields of physics. It is addressing the question: apart from the mass difference, is the muon the same as the electron?

Review 5

Rating:

Very Good

Review:

Summary

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to intellectual merit.

The proposed activities will upgrade three PRad systems including calorimetry, readout, and tracking to reduce the uncertainties on the proton charge radius to 0.0036 fm. The improved measurement will help resolve the so-called “proton radius puzzle” associated with moderately inconsistent measurements using different techniques. The measurement will elucidate low energy QCD and lattice QCD or may uncover a signal of new physics and contribute to the Rydberg constant. The apparatus itself or similar measurement techniques may also be used to search for new physics and improve theoretical estimates for the g-2 discrepancy.

The proposed activities capitalize on low-risk technologies for which the PI’s have significant experience. The application of the technologies to improve the measurement of the proton charge radius is appropriate and noteworthy. The precision measurement may resolve the puzzle or signal new physics.

The plan is solid and the division of labor appropriate. The calorimeter and GEM technologies are individually low-risk and the probability of technical success high. There was little discussion of the development of the readout upgrade other than the components would be withdrawn from the JLab equipment pool. Presumably these are already available, and support identified.

As mentioned in the proposal, cost is a significant risk and perhaps the leading risk. This is quite possible because of supply chain problems and escalation associated with the pandemic. No remedy or descoping options were offered in response to this risk. This is a shortcoming that could impact project success. A comparison of performance with descoped options would have been very helpful.

The team has experience with the calorimetry and GEM trackers and are competent to handle these straightforward extensions.

Resources should be adequate as the team had responsibilities for the predecessor systems and JLab will provide significant support.

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to broader impacts.

The primary broader impact involves development of an enhanced detector system applicable to other measurements and education of students with a focus on underrepresented groups.

The extensions of the instrumentation techniques are appropriately low risk for the PRad-II upgrade, but consequently, not innovative. The dedication to education and to engage with underrepresented groups is evident and successful. The outreach seems to be rather passive, limited to participation in lab tours.

The group has successfully engaged with students and will continue to do so, there was no formal assessment proposed, but rather the number of past students offered. No assessment mechanism was offered for dissemination of the advanced technique or for participation in lab tours.

The individuals and team are well-qualified for the proposed activities and the resources available as they have built similar systems and educated students in the past.

Please evaluate the strengths and
weaknesses of the proposal with respect to any additional solicitation-specific review criteria, if
applicable

N/A

Summary Statement

The proposed MRI Track II project is well designed and appropriately has low technical risk for such an important measurement. The improved proton charge radius measurement will help resolve outstanding experimental inconsistencies and elucidate low energy QCD or perhaps signal new physics. The extended techniques may find applications in other e-p scattering experiments. There is a concern that the funding may be inadequate due to the unfortunately unexpected high rate of inflation. The broader impacts are associated with development of enhanced detector systems with potential applications elsewhere and student education with a focus on underrepresented groups.

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