View Reviews

0000214311: Old Dominion University Research Foundation, Norfolk, Virginia

PI: Weinstein, Lawrence

Proposal Title: SHORT DISTANCE STRUCTURE OF NUCLEI - MINING THE WEALTH OF EXISTING JEFFERSON LAB DATA

Solicitation: DE-FOA-0000995 - FY 2014 Continuation of Solicitation for the Office of Financial Assistance Program

Reviewer Category: Primary

By Reviewer

Collapse All

Reviewer 1

Criteria

1. Scientific and/or Technical Merit of the Project

What is the scientific innovation of proposed research? What is the likelihood of achieving valuable results? How might the results of the proposed research impact the direction, progress, and thinking in relevant scientific fields of research? How does the proposed research compare with other research in its field, both in terms of scientific and/or technical merit and originality?

It is well known that many low-energy properties of nuclei can be successfully explained in terms of the Shell Model (awarded by the Nobel Prize to Goeppert-Mayer and Jensen in 1963), whose basic assumption is the independent motion of nucleons in a mean field (MF). These properties however are mainly sensitive to the average nucleon-nucleon (NN) distance (r_{NN} = 1.5-2 fm); recently it became possible to investigate nuclear structure at high values of energy and momentum transfers, probing inter-nucleon distances of the order of the nucleon radius (1 fm), i.e. in a region where serious doubts can be raised as to the validity of a MF description. Here a picture including NN short-range correlations (SRC) is not only more realistic but can also provide relevant insight concerning in medium short-range dynamics. As a matter of fact the short-range structure of nuclei cannot be inferred uniquely from the knowledge of the short-range behavior of the free NN interaction because the strong repulsive core in the NN potential, resulting from the analysis of NN elastic scattering data, is introduced by means of various form factors that leave a certain degree of arbitrariness, leading to different short-range behaviors of various NN interaction models. Moreover, elastic on-shell NN scattering cannot in principle determine the details of the NN interaction in medium, because two nucleons that experience interaction with surrounding partners, are off-the-energy shell. The investigation of the short-range structure of nuclei represents therefore one of the main challenges of modern nuclear physics. The detailed theoretical and experimental knowledge of nucleon dynamics at short NN distances (r_{NN} < 1- 1.5 fm) will eventually establish the quantitative limits of validity of the Shell Model, providing, at the same time, decisive answers to longstanding fundamental questions concerning the bound systems of nucleons, like e.g., the following ones: 1) are the properties of free nucleons affected by the nuclear medium?; 2) what are the features of the strong interaction between overlapping nucleons?; 3) what is the role of the nucleon substructures, quark and gluons, in shaping the structure of matter as we see it?. It should also be considered that at short inter-nucleon separations, the local density of a NN correlated pair may be up to 7 times higher than the average usual nuclear density (about

0.17 Nucleons/fm^3), i.e. comparable to the density in the core of a neutron star. The study of SRCs could therefore help us to answer several crucial questions of astrophysical character, like the formation of neutron stars from supernovae explosion, as well as neutrino cooling of the star. Identifying and studying SRC effects in nuclei has represented a formidable challenge to both theory and experiments for more than fifty years, but it was only recently that, thanks to the enormous progress made by many-body theories and experimental techniques, the theoretical and experimental studies SRCs were placed on robust grounds. The Jefferson Lab (Jlab) has played a fundamental role into this field: relevant information on NN SRCs in nuclei like ^{4}He, ^{12}C and other medium-weight nuclei has already been collected but much remains to be done to fully map the correlation structure of nuclei through the Table of Elements. New experiment are planned at the upgraded Jlab but the present proposal of Data Mining would sensitively help in elaborating and producing new relevant information on SRCs by extracting the information which is contained in existing raw experimental data collected at Jlab, as clearly explained in the proposal.

2. Appropriateness of the Proposed Method or Approach

How logical and feasible are the research approaches? Does the proposed research employ innovative concepts or methods? Are the conceptual framework, methods, and analyses well justified, adequately developed, and likely to lead to scientifically valid conclusions? Does the applicant recognize significant potential problems and consider alternative strategies?

The proposal is logical and feasible: the aim is to find, analyze and make available to the entire scientific community a wealth of experimental data that have been accumulated during the years at the Jefferson Lab's Hall C but not fully analyzed and distributed. Without this initiative these data, that required serious financial and human efforts, would be probably lost. The feasibility of the proposal is demonstrated by the success of the first stage of the project, characterized by a series of collaboration meetings organized in several countries, with the participation of an increasing number of experimentalists and theorists from different Universities and Institutions. The aim of these collaboration meetings has been to identify and fix the strategy of the research activity. The Data Mining proposal is supported with interest by Jlab and several US and European Universities and its success will represent an important step forward to reveal the details of the short-range structure of nuclei and hadronic matter. The proposal is quite innovative in that a highly qualified and synergic small group of senior experimentalist and theorists met together to elaborate a common research activity aimed at pinning down significant row experimental data and elaborating them to a final form that could became available to theoretical interpretations by various research groups all over the world. In this context it is highly desirable that the final product of the research will be "real" experimental data, free from any "theoretical correction" reflecting one's point of view. The high professionalism and qualification of the experimental and theoretical members of the team is full guarantee that such an event will not occur.

3. Competency of Applicant's Personnel and Adequacy of Proposed Resources

What are the past performance and potential of the Principal Investigator (PI)? How well qualified is the research team to carry out the proposed research? Are the research environment and facilities adequate for performing the research? Does the proposed work take advantage of unique facilities and capabilities?

The PI's CV is a clear demonstration of his high qualification and his first class contribution to the past and present research activities. The same can be extended to the other members of the research team: they represent internationally recognized leaders in experimental and theoretical hadronic physics. The support of Jefferson Lab, in particular of the Group Leader of Hall B, is solid evidence of a fully adequate research environment and a high level facilities supporting the research team.

4. Reasonableness and Appropriateness of the Proposed Budget

Are the proposed budget and staffing levels adequate to carry out the proposed research? Is the budget reasonable and appropriate for the scope?

The requested budget will be used for a 12 months support to a Post-Doc Associate and Graduate Research Assistant and to traveling expenses for meetings of the collaboration. The request for traveling expenses to be used for collaboration meetings with the participation of American and European members of the collaboration appears to be a very reasonable one. As for the financial request for the Post-Doc and Graduate Researcher if it is within the US standard, then I consider the overall proposed budget fully adequate and appropriate for the scope.

Overall Summary of the Proposal

Summary Score: Strongly Encourage Funding (5-6) 6.0

Reviewer 2

Criteria

1. Scientific and/or Technical Merit of the Project

What is the scientific innovation of proposed research? What is the likelihood of achieving valuable results? How might the results of the proposed research impact the direction, progress, and thinking in relevant scientific fields of research? How does the proposed research compare with other research in its field, both in terms of scientific and/or technical merit and originality?

Panelist Comment

The PI is requesting funding for a data mining initiative with the overall goal to provide a unified analysis and simulation tool for all nuclear data taken with the CLAS detector at JLAB in the past years. This is a multi-institutional effort and a relatively large group of experimentalists (of the CLAS community) together with theoreticians is behind this effort. It is mentioned several times that the initiative will give non-expert users the chance to analyze the data. The simulation package is also supposed to serve as a planning tool for future experiments, e.g. in the 12 GeV era of JLAB or elsewhere.

The proposal lists a huge number of physics opportunities. These range from the search for short-range correlations or non-nucleonic degrees of freedom in nuclei to the physics of meson hadronisation. Indeed, a recent Science article very nicely summarizes the spectacular findings, which were obtained at JLAB. From the proposal, however, it appears impossible to identify up to which extent the paper is a direct outcome of the initiative for which funding is requested. It should be mentioned that an initiative with the same title was already supported by DOE in previous years. As a result of the previous work, a new and more compact data format for the CLAS nuclear data was achieved.

2. Appropriateness of the Proposed Method or Approach

How logical and feasible are the research approaches? Does the proposed research employ innovative concepts or methods? Are the conceptual framework, methods, and analyses well justified, adequately developed, and likely to lead to scientifically valid conclusions? Does the applicant recognize significant potential problems and consider alternative strategies?

Although the storage of existing data is a very valuable enterprise and the physics is highly motivated, there remain a number of severe questions. The proposal does not explain, in a clear way, the added value of the previous initiative. It also remains unclear what are the next steps, milestones and so on. There is, in my opinion, a dramatic mismatch between a beautifully written summary of the physics background and the real task of the personnel for which funding is requested.

3. Competency of Applicant's Personnel and Adequacy of Proposed Resources

What are the past performance and potential of the Principal Investigator (PI)? How well qualified is the research team to carry out the proposed research? Are the research environment and facilities adequate for performing the research? Does the proposed work take advantage of unique facilities and capabilities?

The PI together with the collaboration behind him is a very respectful list of (rather senior) physicists. They belong to the world experts in the field.

4. Reasonableness and Appropriateness of the Proposed Budget

Are the proposed budget and staffing levels adequate to carry out the proposed research? Is the budget reasonable and appropriate for the scope?

As it remains unclear what the task really is for the postdoc and the existing PhD student, I cannot judge whether the requested funding is appropriate. My feeling is that the request is rather on the higher side and that the work could also be done by a talented PhD student.

Overall Summary of the Proposal

Summary Score: Encourage Funding (3-4) 3.0

Reviewer 3

Criteria

1. Scientific and/or Technical Merit of the Project

What is the scientific innovation of proposed research? What is the likelihood of achieving valuable results? How might the results of the proposed research impact the direction, progress, and thinking in relevant scientific fields of research? How does the proposed research compare with other research in its field, both in terms of scientific and/or technical merit and originality?

Panelist Comment

This proposal requests funds to continue a “data mining” effort utilizing the many years of CLAS data that have been recorded at JLab. I admit that I found this proposal to be rather strange, and the panel discussions indicated that I was not alone with this impression. The proposal discusses a very wide range of CLAS analyses that might shed light on short-range correlations, the connection between short-range correlations and the EMC effect, and other topics. The analyses that are discussed are clearly important and timely. However, the actual budget request is to support a post-doc and a graduate student to convert the many years of CLAS data summary tapes into a more compact and convenient form, and to implement a more convenient and user-friendly interface to CLAS simulation software. It appears that the physics analyses which are discussed will be done by co-investigators who have support outside of this grant.

There is merit in making the wealth of CLAS data more readily accessible. But I didn’t find a clear explanation why the proposed analyses REQUIRED the new data access method. Meanwhile, the proposed user-friendly simulation tools were only mentioned briefly. This was quite unfortunate. On the one hand, simulations are essential to estimate the trigger and detector acceptances, efficiencies, and resolutions necessary to turn measured particle tracks/momenta into physics quantities. On the other hand, this has the potential to be a huge task because Monte Carlo simulations are often quite challenging to do right. There was no indication of what physics processes would be simulated, nor was there any discussion of what computing resources would be available to the users.

2. Appropriateness of the Proposed Method or Approach

How logical and feasible are the research approaches? Does the proposed research employ innovative concepts or methods? Are the conceptual framework, methods, and analyses well justified, adequately developed, and likely to lead to scientifically valid conclusions? Does the applicant recognize significant potential problems and consider alternative strategies?

The procedures to convert CLAS data sets into the new framework are well understood. Three data sets have already been converted, and a fourth is in progress. In contrast, without more information regarding the proposed scope for the proposed simulation tools, I’m not in a position to evaluate the “appropriateness” of that part of the task.

3. Competency of Applicant's Personnel and Adequacy of Proposed Resources

What are the past performance and potential of the Principal Investigator (PI)? How well qualified is the research team to carry out the proposed research? Are the research environment and facilities adequate for performing the research? Does the proposed work take advantage of unique facilities and capabilities?

The PI is well known in the field. The co-investigators include a long list of very accomplished physicists who are fully capable of carrying out the analyses that are discussed.

4. Reasonableness and Appropriateness of the Proposed Budget

Are the proposed budget and staffing levels adequate to carry out the proposed research? Is the budget reasonable and appropriate for the scope?

The budget was reasonable, except that the travel request seemed large, given that most of the proposed meetings are to be held at JLab.

Overall Summary of the Proposal

Summary Score: Encourage Funding (3-4) 3.0

Reviewer 4

Criteria

1. Scientific and/or Technical Merit of the Project

What is the scientific innovation of proposed research? What is the likelihood of achieving valuable results? How might the results of the proposed research impact the direction, progress, and thinking in relevant scientific fields of research? How does the proposed research compare with other research in its field, both in terms of scientific and/or technical merit and originality?

What is the scientific innovation of proposed research?

The proposal address inclusion of a user-friendly simulation component to the already implemented data analysis capability. This is a necessary step to produce physics results; the data analysis is only half the job.

What is the likelihood of achieving valuable results?

There is clearly an abundance of topics that can be addressed. These researchers have been involved in these studies for many years. The technique proposed has produced valuable results in the past. This is effort is likely to do so as well.

Adding the simulation component is arguably a more challenging tasks that the data analysis component. It will require greater computing resources. On the other hand the basic components have already been developed and used for many years. The challenge is in packaging them for easy use. The entire enterprise does not depend exclusively on this component however.

How might the results of the proposed research impact the direction, progress, and thinking in relevant scientific fields of research?

Beyond the scientific interest of the physics results, the technical program proposed could serve as a model for similar efforts in the future at Jefferson Lab, in particular for the 12 GeV program.

How does the proposed research compare with other research in its field, both in terms of scientific and/or technical merit and originality?

Technically, the approach is orginal in the particular architecture chosen, that of a service-oriented framework.

2. Appropriateness of the Proposed Method or Approach

How logical and feasible are the research approaches? Does the proposed research employ innovative concepts or methods? Are the conceptual framework, methods, and analyses well justified, adequately developed, and likely to lead to scientifically valid conclusions? Does the applicant recognize significant potential problems and consider alternative strategies?

How logical and feasible are the research approaches?

The underlying methods are those that have been used for many years. The technical deployment is new but the data analysis component was proven in the pre-cursor effort.

Does the proposed research employ innovative concepts or methods?

Yes, see above.

Are the conceptual framework, methods, and analyses well justified, adequately developed, and likely to lead to scientifically valid conclusions?

Yes.

Does the applicant recognize significant potential problems and consider alternative strategies?

Alternate method are not in the game plan, as far as I can see. That having been said, exporing the innovative components of the technical work almost preclude having hightly developed alternative approaches within the context of this proposal.

3. Competency of Applicant's Personnel and Adequacy of Proposed Resources

What are the past performance and potential of the Principal Investigator (PI)? How well qualified is the research team to carry out the proposed research? Are the research environment and facilities adequate for performing the research? Does the proposed work take advantage of unique facilities and capabilities?

What are the past performance and potential of the Principal Investigator (PI)?

He has a long record of success in this area of research.

How well qualified is the research team to carry out the proposed research?

The principals of this team have worked together for many years on these topic. They are very well qualified.

Are the research environment and facilities adequate for performing the research?

The question of adequate computing resources is not addressed in the proposal.

Does the proposed work take advantage of unique facilities and capabilities?

This reviewer cannot comment.

4. Reasonableness and Appropriateness of the Proposed Budget

Are the proposed budget and staffing levels adequate to carry out the proposed research? Is the budget reasonable and appropriate for the scope?

Are the proposed budget and staffing levels adequate to carry out the proposed research?

The tasks assigned to the proposed personnel appears reasonable.

Is the budget reasonable and appropriate for the scope?

Yes.

Overall Summary of the Proposal

Summary Score:	Strongly Encourage Funding (5-6) 5.0

Reviewer 5

Criteria

1. Scientific and/or Technical Merit of the Project

What is the scientific innovation of proposed research? What is the likelihood of achieving valuable results? How might the results of the proposed research impact the direction, progress, and thinking in relevant scientific fields of research? How does the proposed research compare with other research in its field, both in terms of scientific and/or technical merit and originality?

This proposal requests an extension of an earlier DOE funded project under the same title by the same PI and Co-PIs. The objectives of the proposed project is to facilitate the analysis of existing nuclear target data taken at JLab with the CLAS detector. The software infrastruture for allowing easy access to these CLAS data have already been developed, with some fraction of the nuclear target data processed and available for researchers interested in analysing them. The goal of the present proposal is to make more of the CLAS nuclear target data sets available, and to further develop the software tools for extensive simulations needed for publishing the physics results. The requested fund will support a postdoc and a graduate student at ODU.

The physics topics associated with the nuclear target data taken at CLAS cover a very broad range. They include the 2N and 3N short-range correlations (SRC), the intriguing connection between the EMC effect and the SRC, search for pre-existing Delta components in deuteron and in nuclei, color transparency and nuclear modification of nucleon properties, tagged EMC effect, etc. The ODU group is well known for their contributions in this area of research. In particular, their work on the possible connection between EMC and SRC has attracted much attention and interest.

As a result of the extensive experimental programs at JLab and theoretical effort, the physics of 2N SRC is now quite well understood. More detailed studies on SRC could be obtained from further analyses, as mentioned in this proposal. However, it is unlikely that they would lead to major new insights. The idea to search for N-Delta and Delta-Delta admixtures in SRC pairs is an interesting one. If such "pre-existing" Delta components are found, either in deuteron or in nucleus, it would be a major new result. The challenge, however, is to isolate these presumably tiny "pre-existing" Deltas from the abundantly produced Delta particles. Theoretical inputs from the theorists, as discussed in the proposal, would be crucial.

In principle, the idea of making the CLAS data easily accessible to non-experts is commendable. It would encourage, for example, more undergraduate and graduate students from smaller institutes to participate in physics analysis. In pracice, however, it is not clear from the proposal whether any non-experts have benefited from this so far. Nevertheless, it is important to archieve the wealth of data obtained at CLAS6, and this proposal will certainly allow more researchers to take a quick look of existing data to check out their ideas or speculations. This proposal is definitely cost-effective in maximizing the physics output from JLab 6 GeV programs. In anticipation of new findings from 12 GeV upgrade, it is also extremely helpful to check any new results against the existing CLAS6 data.

2. Appropriateness of the Proposed Method or Approach
How logical and feasible are the research approaches? Does the proposed research employ innovative concepts or methods? Are the conceptual framework, methods, and analyses well justified, adequately developed, and likely to lead to scientifically valid conclusions? Does the applicant recognize significant potential problems and consider alternative strategies?

3. Competency of Applicant's Personnel and Adequacy of Proposed Resources
What are the past performance and potential of the Principal Investigator (PI)? How well qualified is the research team to carry out the proposed research? Are the research environment and facilities adequate for performing the research? Does the proposed work take advantage of unique facilities and capabilities?

4. Reasonableness and Appropriateness of the Proposed Budget
Are the proposed budget and staffing levels adequate to carry out the proposed research? Is the budget reasonable and appropriate for the scope?

Overall Summary of the Proposal

Summary Score: Encourage Funding (3-4) 4.0

Reviewer 6

Criteria

1. Scientific and/or Technical Merit of the Project
What is the scientific innovation of proposed research? What is the likelihood of achieving valuable results? How might the results of the proposed research impact the direction, progress, and thinking in relevant scientific fields of research? How does the proposed research compare with other research in its field, both in terms of scientific and/or technical merit and originality?

The majority of the proposal is a list of a large number of interesting physics projects related to effects which can be studied using nuclear targets. These include short range correlations, non-nucleonic components, nucleon modification, color transparency and hadronization. It is impossible for the reviewer to gauge how much benefit, if any, the data-mining effort has had on these topics or how much benefit is to be expected. Most of the discussion is a very general retrospective of results which have been obtained from other experiments or from earlier analyses of CLAS data. No specifics are given as to how much of the mined data has been looked at. Section 2.2.2 is the most specific in relating these physics subjects to the data-mining effort, but it makes only a qualitative statement that a large set of available data is helpful. Since some of the data are already available, an indication of the advantage of the data-mining effort should be included.

In section 3 the proposal states that "Researchers at many institutions are already using the data mining software infrastructure and analyzing data...". It would be very helpful to have a list of these users, presumably both in and outside the data-mining collaboration, and some indication of what progress they have made. Instead, the statement is followed by a list of all the collaborators and their subjects of interest. There is some mention of on-going work but, even there, no indication as to whether the data mining has affected these analyses.

Most of these projects were already described in the original data-mining proposal. It is not clear how they have progressed in the past three years. It would be good to know how many of them have become CLAS-approved analysis projects, how many have begun analyzing data and how many are making use of the output of the data-mining operation.

2. Appropriateness of the Proposed Method or Approach

How logical and feasible are the research approaches? Does the proposed research employ innovative concepts or methods? Are the conceptual framework, methods, and analyses well justified, adequately developed, and likely to lead to scientifically valid conclusions? Does the applicant recognize significant potential problems and consider alternative strategies?

Very little information is provided with which to evaluate success of data-mining operations so far.

The project has contracted greatly from original goal of repeating track reconstruction with more modern software and cuts. The present implementation of data mining uses the existing reconstruction and doesn't even attempt to improve cuts. Rather it repackages the existing already-reconstructed tracks in a more efficient and more compact format and organizes information on the applied cuts so the information is easily retrievable.

It is not clear that there is anything wrong with this change of focus. The original plan, to find new values for cuts and reanalyze all of the raw data was very ambitious and did not offer any clear advantage over the reconstruction efforts which had already been carried out.

Given this reduction in scope of the project, however, it is surprising that this, now much less ambitious, program has not progressed much more rapidly. Only three of the eleven data sets have been processed, with a fourth being in progress.

The planned future activity, beyond completing the processing of the remaining eight data sets, involves setting up a Monte Carlo server at ODU which will allow users access to simulation data without having to run the Monte Carlo programs themselves. This would be useful for inexperienced users, such as undergraduates doing part-time research activities. It is unlikely to be of much benefit to more experienced users and groups which would already have GSIM and RECSIS running on their own computers.

3. Competency of Applicant's Personnel and Adequacy of Proposed Resources

What are the past performance and potential of the Principal Investigator (PI)? How well qualified is the research team to carry out the proposed research? Are the research environment and facilities adequate for performing the research? Does the proposed work take advantage of unique facilities and capabilities?

The collaboration includes a number of groups with excellent reputations and a history of making good progress in the field.

These groups appear to be interested in a variety of physics which can be extracted from the existing data sets. The unifying theme of the proposal is the 'data-mining' activity which presently consists of repackaging data into a uniform and compact format and compiling cuts associated with each data set in an easily accessible form. Future plans would similarly make Monte Carlo simulated data more easily available, with simulation conditions corresponding to each data set. While these 'data-mining' activities would be beneficial for inexperienced users, such as undergraduates, it is less clear what impact they will have for the more experienced groups which make up the majority of this collaboration.

4. Reasonableness and Appropriateness of the Proposed Budget

Are the proposed budget and staffing levels adequate to carry out the proposed research? Is the budget reasonable and appropriate for the scope?

Travel seems inflated, being based on assumed travel from Norfolk, VA to Los Angeles rather than, for example, Newport News.

It is not clear how travel funds are to be divided among collaborating institutions or whether these funds are only for travel by ODU personnel and the data mining postdoc.

The need for one or two collaboration meetings each year is questionable. The groups are working on a variety of different topics united only by the fact that they are using data from the data mining effort. Since the whole point of the data mining is to make the data easily accessible, the use of the data doesn't seem to justify dedicated collaboration meetings. There is mutual interest in the physics and overlap in that the data were all taken with CLAS, but that is true for other CLAS experiments as well. It seems the CLAS collaboration meetings would be an adequate forum for discussion of these projects without dedicated meetings of the data mining collaboration.

Overall Summary of the Proposal

Summary Score: Encourage Funding (3-4) 4.0

Reviewer 7

Criteria

1. Scientific and/or Technical Merit of the Project

What is the scientific innovation of proposed research? What is the likelihood of achieving valuable results? How might the results of the proposed research impact the direction, progress, and thinking in relevant scientific fields of research? How does the proposed research compare with other research in its field, both in terms of scientific and/or technical merit and originality?

Panelists Comments

Extension of a previous project with the same name and people involved. Much of the basic work is done, including first results. Now results should be further “harvested”. 2N and 3N short range correlations, and the connection to the EMC effect, as well as preexisting Delta components, color transparence and in-medium modification of the nucleon’s properties. Basic description of the nucleus is in terms of individual nucleon motion in a mean field. This is mostly sensitive to effects at the mean separation. At higher momentum transfers one can probe short range correlations. One can not simply extend from 2 body interactions to the case in the nucleus where off shell effects due to interactions with other nucleons play a role. The goal is to get quantitative limits on the validity of the shell model and to find answers to open questions on e.g how nucleons are affected by the medium, interaction of overlapping nucleons, transition form quarks and gluons to nucleons as effective degrees of freedom. Information on the short range correlations could also be important for understanding the formation of neutron stars. As a goal this project helps prepare new measurements at CLAS12, from not only the physics side, but also in setting up the corresponding groups. This is more than just data analysis. In particular one goal is to make the simulations easier to use.

There is a wide range of aspects that can produce very interesting results. The flip side of that is that the benchmark for success can easily be redefined depending upon where actual progress is made. It is not sufficiently clear how much of the exiting data have been looked at and what has really come out of the existing effort? Are there people outside of this group who are using this data? Do others use it, or does this group simply put the data in a different format for their own use?

There seems to be a big mismatch between the beautiful physics motivation on the one hand and the lack of details on e.g. the simulations on the other hand. It is going to be very difficult to make a general simulations program that can be used as a black box by e.g. beginning students. The detailed tasks are not really clear, and one of the written reviewers indicated that the achievements in the current funding period are very different from what was promised. Furthermore, there is no clear picture ift other funding applications on this topic are planned or submitted.

2. Appropriateness of the Proposed Method or Approach

How logical and feasible are the research approaches? Does the proposed research employ innovative concepts or methods? Are the conceptual framework, methods, and analyses well justified, adequately developed, and likely to lead to scientifically valid conclusions? Does the applicant recognize significant potential problems and consider alternative strategies?

Panelists Comments

The goal is to evaluate data that are available, but were collected for other purposes. These data are appropriate for these studies. Thus one can do important physics without having to collect new data. This is a combination of senior experimentalists and theorists. The tools are being developed It is not clear who outside of their active group has benefited from being able to use these data. The repackaging of the data seems beneficial for inexperience users, but probably this has very little impact for more involved scientists.

3. Competency of Applicant's Personnel and Adequacy of Proposed Resources

What are the past performance and potential of the Principal Investigator (PI)? How well qualified is the research team to carry out the proposed research? Are the research environment and facilities adequate for performing the research? Does the proposed work take advantage of unique facilities and capabilities?

Panelists Comments

The members are leaders in their activities with impeccable international reputations. The structure of this proposal is different form most other proposals in this program. This seems to be a type of networking activity. It was not clear that what we have in front of us is the complete picture. Is this everything or only the ODU part of it? If this is everything, then it was not presented who is really doing what.

4. Reasonableness and Appropriateness of the Proposed Budget

Are the proposed budget and staffing levels adequate to carry out the proposed research? Is the budget reasonable and appropriate for the scope?

Panelists Comments

There are very little details about what the Postdoc will actually do, and thus there is very little basis to refute the conjecture that the personal costs seem inflated for this task. It is not clear how the funds will be distributed, unless of curse further applications connected to this are envisioned.

Overall Summary of the Proposal

Summary Score: Discourage Funding (1-2) 2.0

External Notes

No comment to display

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