Report of the 2019 Committee of Visitors
Division of Physics
National Science Foundation

Meeting Dates
June 20-21, 2019

Submitted on behalf of the Committee by
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To
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Table of Contents

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I. Summary and Recommendations

The 2019 Committee of Visitors (COV) for the Physics Division (PHY) of the National Science Foundation (NSF) met in person on June 20–21, 2019.

The COV was charged to address and prepare a report on:

- the integrity and efficacy of processes used to solicit, review, recommend, and document proposal actions;
- the quality and significance of the results of the Division's programmatic investments;
- the relationship between award decisions, program goals, and Foundation-wide programs and strategic goals;
- the Division's balance, priorities, and future directions;
- the Division's response to the prior COV report of 2015; and
- any other issues that the COV feels are relevant to the review.

This document is the resulting report.

The COV's inputs to the standard NSF template for COV responses are in Appendix A. The meeting agenda is in Appendix B. The COV's membership and subcommittees are listed in Appendices C and D. The full charge to the committee is in Appendix E.

Immediately below are the observations, suggestions, and recommendations developed in the course of the COV's deliberations.

Observations, Suggestions, and Recommendations

1. *Observation:* The COV was impressed with the processes for evaluating funding proposals that the Physics Division uses. The result of these processes is a portfolio of projects that contains excellent science, important educational thrusts, and considerable flexibility to respond to emerging opportunities.

2. *Suggestion:* An important factor in the success of the award processes is the expertise and dedication of the Program Directors. The Physics Division is fortunate to have a group of excellent Program Directors in these key roles. We suggest that the Division leadership continue to do everything possible to make these positions as attractive as possible to talented people.

3. *Observation:* The Physics Division has responded appropriately to the recommendations of the 2015 Committee of Visitors.

4. *Suggestion:* The issue of broadening participation by underrepresented groups in the projects funded by the Division is important. We are pleased that the Division clearly takes this issue seriously and we are optimistic that progress

will continue to be made. As pointed out by previous COVs, improvements should be made to the processes of collecting data on the diversity of proposers and their groups.

5. *Observation:* In the 4-year period under review by this COV, funding for the Physics Division has remained relatively constant, while the number and size of research proposals have increased. This has meant that the success rate of proposals has decreased and that pressure to reduce the size of awards has emerged. We believe the Program Directors have handled this situation in a thoughtful and consistent way. However, unless overall funding increases the situation will become increasingly difficult. We very much hope that NSF will see a significant increase in funding, as such an increase is certainly justified by the scientific output of the programs.

6. *Suggestion:* The COV found in a number of programs that funding for postdoctoral scholars seems to be a consistent problem. This is concerning, as such positions are important both for workforce development and for achieving research goals. Presumably this problem is directly related to the average size of grants. We suggest that the Physics Division examine the priority that is placed on postdoctoral support in grants, although we are not suggesting that there is an easy solution.

7. *Suggestion:* We heard from the various programs different opinions on the optimal length of grants. It seems that the situations do indeed vary. We suggest that the different programs define the default length of a grant according to their situation and needs, but also that the Program Directors have the flexibility to assign lengths according to the needs of different grants.

8. *Suggestion:* In looking for ways to enhance the buying power of grants, some members of the COV noted that, while a typical research grant is expected to cover the full tuition of graduate students supported by the grant, NSF Graduate Fellowships are capped at a generally lower level and universities cover the remainder. We suggest that NSF may want to explore with universities whether a similar standard amount for tuition and fees would be appropriate for graduate students supported by research grants.

9. *Observation:* The Midscale program appears to be a good fit for some important projects supported by the Physics Division. This is a welcome development.

10. *Observation:* The review of the Physics Frontier Centers program currently underway should provide important guidance for the Physics Division. We look forward to the results of the review.

11. *Observation:* For some subfields within the purview of the Physics Division, there are well-established processes whereby the community sets long-range research priorities on a regular basis. Examples include the Long Range

Planning process for nuclear physics, the Particle Physics Project Prioritization Process for elementary particle physics, and the National Academies Decadal Surveys for several subfields. We believe that these processes, generally involving FACA committees, are very important in gathering community input and in providing guidance to the relevant funding agencies. It appears that the Physics Division is well connected to these processes and we expect that they will continue to use this external input as an important part of setting priorities.

12. *Recommendation:* There are a number of recently launched NSF-wide initiatives, including the Big Ideas. In general the Physics Division has done a commendable job of engaging with these research and process initiatives to enhance the overall portfolio of research projects. Concerning the Big Ideas, the Division has important connections to two of them: Windows on the Universe and Quantum Leap. Because these two have different funding mechanisms, it seems that it has been much easier to leverage Windows on the Universe to empower existing programmatic priorities than the Quantum Leap. The COV believes that this difference is unfortunate and somewhat artificial. We thus recommend that the NSF examine how the initiatives can all be made straightforward to coordinate with existing programs. The COV also recommends that staffing be enhanced in areas where Program Director workloads have increased as a result of their obligations to support Big Ideas initiatives.

13. *Suggestion:* It is our understanding that proposers are now supposed to describe other sources of support and commitments and how they relate to what they are proposing. Review panels are also supposed to comment on these relations. From our review it appears that compliance with these guidelines is sporadic. We suggest that the Division examine the situation and take appropriate action.

14. *Recommendation:* The COV heard concerns, from a number of directions, about what information is provided by the NSF to proposers and others. For example, it is not clear that, following evaluation of a proposal, the complete results are provided to all PIs on the proposal, not just the principal PI. Also, the NSF Award Search on the public web page has problems; it is difficult to use and one cannot tell if the dollar amounts are for one year or for multiple years. We also believe that the instructions for annual reports from grantees could be improved. We recommend that the Physics Division take a comprehensive look at their system for providing information to proposers and others and make improvements as needed.

15. *Observation:* We note that, while the observations, suggestions, and recommendations included in this section represent the overall perspective of the COV, the subgroup reports contain more detailed observations and recommendations that should be especially useful to the individual programs.

II. Proposal Review and Decision Process

The 2019 COV was very impressed with the thoroughness, fairness, and openness of the process the Physics Division uses to solicit, evaluate, and make decisions on proposals for research funding. A critical part of this process is the expertise and dedication of the Program Directors.

Prior to our face-to-face meeting in June 2019, members of the COV were given the opportunity to review a number of "eJackets" of proposals that had been considered by the Physics Division over four funding years. These proposals included some that had been funded and some that had not. These reviews were organized according to ten subgroups, each of which focused on a specific programmatic area. There were extensive online discussions of the subgroups prior to the face-to-face meeting, including the relevant Program Director(s). The members had the opportunity to discuss various issues with the Program Directors as well as request access to additional proposals as needed. Physics Division staff vetted the jackets to make sure committee members with a Conflict of Interest on a particular proposal did not participate in reviewing that jacket.

During the face-to-face meeting the subgroups met again to follow up on any remaining issues and to finalize their reports. These reports are included in Section V of this document. They contain the detailed findings of the COV, but many of the findings apply broadly to the work of the Division and we discuss those in this section. We include in the appendices the response template, which contains our responses to specific NSF-posed questions.

Review Process

Typically a PHY research proposal undergoes a three-step process of review before a decision is made. The first step is several ad hoc (generally by mail) reviews. This is followed by a panel discussion of the proposal along with other proposals in that area. Finally, the relevant Program Director reviews the proposal and the other input and makes a recommendation. This recommendation generally amounts to the final decision, as the Program Directors are justifiably given a large role in making such decisions. The COV believes that overall this is a process that is fair and consistent, resulting in excellent research and educational efforts.

Not all proposals go through exactly the process described above. For example, large grant proposal reviews often involve a site visit or reverse site visit. This has the advantage of allowing the Program Directors and the proposers the opportunity to address issues in detail, but such visits are expensive and also run the risk of favoring larger proposals.

Ad Hoc Reviewers

The COV members were uniformly impressed with the choices of ad hoc reviewers by the Program Directors. The reviewers were clearly experts in their fields and represented the variety of experience that is needed to provide an overall balanced assessment. In almost all cases that we examined, the ad hoc reviewers did an excellent job. In the few cases where an ad hoc review appeared to be superficial, the fact that the overall review was multi-faceted meant that in the end the right decision was still reached.

We were also impressed by the general diversity of ad hoc reviewers who were chosen by the Program Directors. The geographical diversity was representative of the geographical diversity of the community. Concerning women and underrepresented minorities, the diversity of reviewers appeared to be at least as great as the diversity of the community. The Program Directors also appeared to welcome as reviewers less senior researchers who had already demonstrated significant accomplishments.

Panels

Panel reviews are in many ways complementary to the ad hoc reviews. The panel reviews use as important input the ad hoc reviews that have been provided, but the panelists also make their own judgments and, most importantly, are in a position to make comparative judgments about proposals. The COV found that the panel reviews we considered had provided very useful perspectives to the Program Directors.

Program Directors

In general the COV was impressed with the thoughtfulness of the proposal evaluation summaries and recommendations provided by the Program Directors. Although well informed by the ad hoc and panel reviews, the Directors were not reluctant to depend on their own expertise and experience as well. As we indicate in other sections of this report, the Program Directors in the Physics Division play a crucial role in identifying and supporting a premier and diverse research program. The Division and the community are very fortunate that such an outstanding group is in place.

Feedback to PIs

An important part of the review process consists of providing feedback to PIs following the funding decision. Such feedback is critical to ensure that the process is fair and transparent. It also has an important role in educating and supporting scientists so that, especially in the case of a declination, their next proposal submission may be more successful. The COV believes that the Physics Division

Program Directors do a fine job of providing such feedback. For example, a PI whose proposal has been declined can contact their Program Directors to get more extensive information on the review process and result.

Conflicts of Interest (COI)

It is clear that the Foundation in general and the Physics Division in particular continue to take Conflict of Interest issues very seriously. This is entirely appropriate and is expected by the community. It is also critical to ensuring that the review process is fair and unbiased.

Final Comments

The COV is impressed with the overall process in the Physics Division for evaluating research proposals and making often difficult funding decisions and we have no substantive suggestions for improvement on a Division-wide basis. We congratulate the Division leadership and the Program Directors for putting in place such an impressive and successful process. We hope that the Division will have the resources going forward to fund even more great science using this process.

III. Broadening Participation

We include this discussion as a major section because we trust that everyone views this issue as crucial for the future. Science in general, and certainly the field of physics, would benefit from a more diverse workforce and many people would benefit from increasingly diverse opportunities. Obviously the complete solution to the problem will involve many efforts outside NSF, but we believe NSF is in the position of playing a critical leading role.

We commend the Physics Division for the development of a diversity plan and for its skillful use of internal co-funding in support of broadening participation. Our committee heard, in a number of different contexts, concerns related to the collection and analysis of data on diversity and inclusion in physics research. The previous COV recommended that the Division change the timing of requests for such data from PIs and this recommendation has been addressed to some extent. But we believe that at least at the Physics Division level, and perhaps at a much more general level, it is important to assess how one can most effectively collect, analyze, and use data related to diversity and inclusion.

IV. Additional Specific Issues

Continued Support for Program Directors

As indicated throughout this report, the COV was very impressed with the work done by the Physics Division in identifying and supporting high-quality research in physics, especially the work done by the Program Directors. We strongly endorse the model in PHY, in which the Program Directors are highly respected in their own scientific disciplines and respected by the community in general. This is crucial to NSF's effectiveness in driving the science forward – the Program Directors have a significant level of autonomy in decision-making, along with time and resources to communicate with individuals within the research program and best understand their needs and circumstances. We congratulate the Division leadership for supporting the work of the Program Directors in this way. Given the increasing demands on the Program Directors' time, due to NSF-wide initiatives and continuing funding concerns, we hope that the Division will be able to preserve the special and effective roles of the Program Directors.

In a related point, apparently in the last significant reduction in funding for science, the individual grants supported by the Physics Division bore the brunt of the burden, largely because of decisions made well above the Division. If there are similar situations in the future, we recommend that the process be transparent and that the Program Directors have a strong voice in deciding the appropriate balance of cuts.

Pressures on Individual Grants

The size and number of individual grants are of some concern. The implementation of broad initiatives, while commendable in many ways, can have the unintended consequence of squeezing individual grants unless the total amount of funding available increases significantly. The COV understands that such increases are not always possible in the short term, and we applaud the Division for putting priority on maintaining the importance of individual grants. In particular, we strongly endorse the policy of not letting the individual grants total drop below a 50% floor level with respect to the Physics Division research funding level. One issue related to this came up in discussions of several programs within the Division. That is, the limited size of many grants is making it increasingly difficult to fund postdoctoral positions. This is very disturbing, as the availability of such positions is critical not just for workforce development but for the advancement of science as well.

Length of Grants

The COV discussed the issue of the appropriate length of a grant, which currently can be as long as 5 years. Overall, we recommend that the Program Directors continue to have flexibility when it comes to the length of the grant. An important issue is the default length of a grant. There are several factors that ultimately determine the best length for any specific grant, and these factors vary between the different program areas. Some areas believe there is significant motivation to change to a 5-year grant cycle as a baseline expectation, which would help with the workload on the PIs and the NSF staff. In other program areas, 3 years appears to be the right time scale for the reviews, as it helps PIs and Program Directors react to changing science opportunities, trends, funding, and performance. We recommend that different program areas have the flexibility to adopt the length of cycle that best fits the program's needs and that within that guideline the Program Director still have some flexibility.

CAREER Award Program

The CAREER award program at NSF has some complications. Most of the complications apparently result from misunderstanding between PIs and their institutions and the NSF concerning the goals of the program. The NSF states that the CAREER award, while intended for promising junior researchers, is not strictly a research excellence award. Instead, it is an alternative research funding program with an educational component. Many institutions apparently believe that the program is a research excellence award program and thus strongly encourage their junior researchers to apply for the award. Such blanket pressure can burden some junior researchers with applying for a type of award they are not especially well suited for, at some cost to their research productivity.

A contributing factor to the misunderstandings described above is the fact that there is a similarly named program (the Early Career Award program) at the Department of Energy that is in fact a research excellence award program.

We also point out that the NSF description of the CAREER award as not being a research excellence award is actually inconsistent with the fact that the CAREER award program is NSF's funnel to the competition for PECASE awards.

The COV recommends that NSF aggressively clarify for the community, including the leadership of academic institutions, the goals of the CAREER award program.

Connections of the Physics Division to the Big Ideas Thrusts

The current connections of the Physics Division to the Big Ideas thrusts (Windows on the Universe and Quantum Leap) present interesting contrasts. The COV understands that these two thrusts involve different funding mechanisms. Windows on the

Universe is a meta-program and Quantum Leap is a solicitation requiring at least 3 PIs from at least 3 different disciplines. Given this difference, it is striking that the Windows on the Universe program is showing signs of much greater effectiveness at providing significant additional funding to an important research area. We hope that the NSF leadership will take a hard look at these programs and, if possible, change the structure to make it easier for the programs to be effective in initiating and supporting important research, including ongoing research in the already existing programmatic areas of relevance.

We also note that when the funding for a multi-disciplinary Big Idea program ends it may leave financial burdens in its wake that can heavily burden the core programs. We strongly encourage the development of mechanisms that will minimize these adverse impacts.

Structure of the COV Process

The structure of the COV process has changed significantly in recent years because of the availability of e-jackets and of better electronic communications. We believe that, because of these changes, the process has definitely improved and the COV discussions are more in depth and thoughtful. However, the process also now involves substantially more total time commitment from the COV members and most likely from the NSF staff as well. More guidance from NSF about the scope of effort could possibly help keep the time commitment under control.

V. Reports of the Subcommittees

A. Atomic, Molecular, and Optical Physics/ Quantum Information Sciences

1. Introduction

The AMO/QIS Subcommittee of the 2019 COV reviewed three programs: Atomic, Molecular, and Optical Physics – Experimental (AMO-E), AMO-Theory (AMO-T), and Quantum Information Sciences (QIS). These three programs have been overseen by John Gillaspy (NSF), Steve Southworth (Argonne National Lab --- rotator), Alex Cronin (Arizona --- rotator), and Michael Cavagnero (Kentucky --- rotator.) Gillaspy, Southworth, and Cronin have taken responsibility for the AMO-E program, Cavagnero for the AMO-T program, and Cavagnero and Cronin have overseen the QIS Program.

These three programs represent an extraordinarily diverse portfolio of science: attosecond laser probing of atomic and molecular processes, precision measurements of the electron's electric dipole moment that have had remarkable success in constraining supersymmetric theories, axion searches that provide crucial limits on the dark matter constituents of the universe, and the quantum gas microscope, which is capable of addressing and detecting individual atoms in a lattice --- just to name a few.

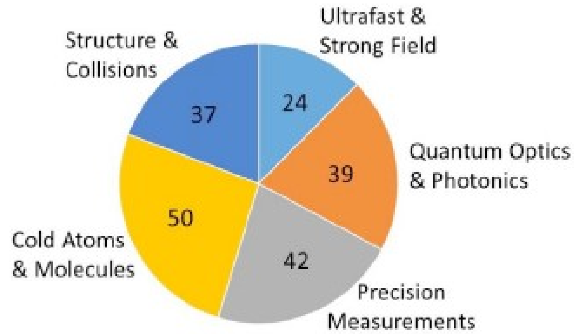
In our report, we briefly review the fields covered by these three programs, discuss their budget and funding rates, assess the proposal review process, and consider the relationship of these programs with others in the MPS directorate. Finally, we make three recommendations which we believe to be important for the success of the AMO and QIS programs moving into the next decade. Our chief concerns are the importance of effectively integrating the QIS Program into the Quantum Leap Initiative to more optimally fund this important research direction, adjusting the length and minimum size of awards including how graduate student tuition should be supported, and maintaining a clear succession plan for Program Directors.

2. Examples of Science Supported by the Three Programs

2.A AMO -- Experiment

The AMO experimental program at the NSF is divided into five sub-fields: Precision Measurements, Cold Atoms and Molecules, Quantum Optics and Photonics, Ultrafast and Strong-Field physics, and Structure and Collisions. The number of Awards in these categories are shown in Figure 1.

Figure 1: Number of Awards in each sub-field of AMO Experiment. The pie chart shows all the awards from AMO-E that started in 2015-2018. The total is 192 awards.



While these categories are a useful way to organize the areas of experimental AMO program into categories that are roughly similar in size, the boundaries between them are fuzzy, as many areas of AMO physics fall into more than one category.

2.A.1. Precision Measurements

Precision measurements have been a driver in AMO physics for decades, and are in many ways a defining activity of the field. Precision measurements have benefited enormously from the development of new technologies, including the dramatic improvement in atomic clock technologies, resulting in inaccuracies at the 10^{-18} level.

In 2015-2018, the NSF supported programs as diverse as searches for dark matter, tests of local Lorentz invariance, and searches for a possible time variation of the proton-to-electron mass ratio. A particularly notable experiment conducted by researchers from Yale, Harvard and Northwestern universities, the ACME team, reported a new measurement of the shape of the electron charge (Fig. 2). The results show that an electric dipole moment of the electron, if one exists, is less than 1×10^{-29} e-cm. This extraordinarily precise measurement supports the Standard Model of particle physics describing the fundamental forces and particles in the universe, and importantly, places severe constraints on theories that go beyond the Standard Model, including supersymmetry.

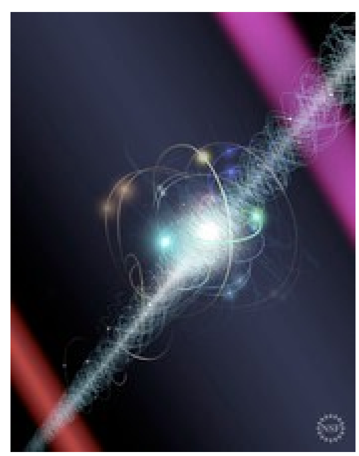


Figure 2. Artist's representation of an electron in a ThO molecule as it is interrogated by read-out lasers.

2.A.2. Cold Atoms and Molecules

The ability to cool atomic gases to quantum degeneracy, a capability achieved 25 years ago, has led to an explosion of research that connects with a multitude of other areas of physics and science. Cold atoms are frequently employed as qubits in quantum information science and as stand-ins for electrons in quantum simulators of electronic materials. A major accomplishment in this area has been the development of the "quantum gas microscope" in which individual atoms in a two-dimensional array may be detected and manipulated in a site-specific way. In the 2015-18 period, the NSF initiated support of research on the synthesis of cold molecules from cold atoms, cold atoms in optical lattices, the coupling of atoms to optical resonant cavities, and the physics of atoms confined to low dimensions.

2.A.3. Quantum Optics and Photonics

In 2015-18, the quantum optics and photonics subfield has supported research, such as four-wave mixing, wavefunction tomography, and quantum optomechanics, that explore the quantum properties of light. This work is closely related to quantum information science. In a notable development, photonic crystal fibers are used to create linear atomic arrays with integrated optical fields.

2.A.4. Structure and Collisions

Determination of atomic structure has been a core activity in AMO physics for decades. In recent years, exciting developments in this field have included the realization and study of exotic species such as antihydrogen, the production of highly-excited "hollow states" of atoms and molecules, and the observation of "Efimov states," in which normally non-interacting atoms bind together through three-particle interactions. Many of these are now accessible to high precision measurements. The example shown here involves an important recent milestone achieved in antimatter.

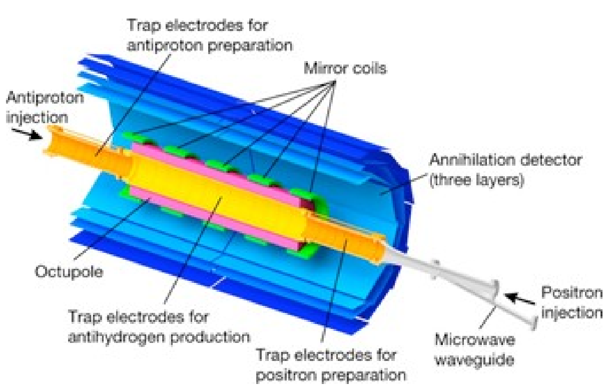


Figure 3. [from Wertele, *et al.*, Nature **548**, 66 (2017), supported in part by PHY-1806305].
The antihydrogen production and trapping region of the CERN ALPHA-2 experiment is shown. Antiproton and positron plasmas on either side of the production region lead to antihydrogen at the center of the trap. This experiment has recently yielded a precision test of the hyperfine splitting of the anti-hydrogen ground state, which in turn tests the CPT theorem.

2.A.5. Ultrafast and Strong Fields

Ultrafast and strong-field physics is one of the fastest-growing areas of AMO science, driven by the ever-more detailed observations and control of quantum dynamics of coupled electronic and nuclear motion in atoms, molecules, and plasmas. The enabling physics has come largely from greater control over ultra-strong

(>volt/angstrom) laser fields with sub-femtosecond pulse durations. The AMO-E and AMO-T programs support leading work in this area, and have strong connections to the Physics Division program in Plasma Physics as well as the MPS programs in Chemistry and Condensed Matter Physics. The DOE Basic Energy Science Program is also a strong supporter of this science, particularly due to the development of free-electron x-ray laser facilities at DOE's SLAC National Accelerator Laboratory, with kilovolt photon energies, fields of kV/Angstrom, and pulse durations in the 300 femtosecond to 300 attosecond range.

A major thrust in this subfield of AMO is to produce "molecular movies" that capture the motion and interactions of electrons and atoms during photo-initiated processes such as strong-field ionization, photo-induced excitation, and photo-dissociation. These movies can reveal information about dynamics that are unavailable using more conventional approaches. One example, strong-field bond rearrangement, is shown in Figure 4.

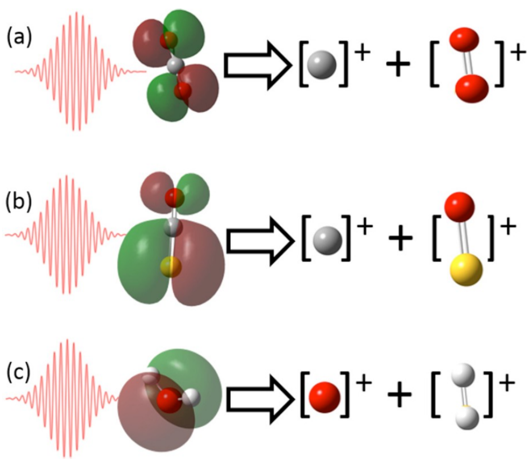


Figure 4. Ultrafast strong fields can induce bond rearrangements in tri-atomic molecules, as shown here:
(a) $CO_2 + n\omega$ lz $O_2^+ + C^+$.
(b) $OCS + n\omega$ lz $SO^+ + C^+$.
(c) $D_2O + n\omega$ lz $D_2^+ + O^+$.
In these three cases, strong-field double-ionization with an infrared (800nm) laser can lead to bond formation between two atoms that shared no bond in the parent molecules.
(Zhao *et al.*, Phys. Rev A **99**.053412, Supported in part by NSF Grants PHY-1757778 and PHY-1723002.)

<u>2.B. AMO -- Theory</u>

The AMO --- Theory program supports fundamental and applied theoretical investigations of the properties of atoms, molecules and light and their various interactions in wide-ranging environments. The largest component by funding is cold atoms, but the breadth of topics in AMO theory is impressive, stretching from attosecond processes to high-precision measurements that could have impacts on cosmological scales.

The wide range of novel phenomena arising from the cooling of atomic and molecular gases to ultracold temperatures, along with their potential applications in the development of new quantum technologies, has given a major impetus to theoretical AMO physics in recent years. The scope of the program has accordingly increased to encompass fundamental studies in quantum statistical mechanics, quantum thermodynamics, non-equilibrium phenomena, and quantum information science. The program has successfully partnered with DMR/CMMT to support research in these fields.

One recent exciting result supported by AMO-T was the prediction of the Efimov state of He (Science **834**, 551 (2015)). Two helium atoms do not interact with each other and will not form a chemical bond. However, like a Borromean ring, three He atoms can form a weakly-bound molecule. This was observed three years ago using a reaction microscope.

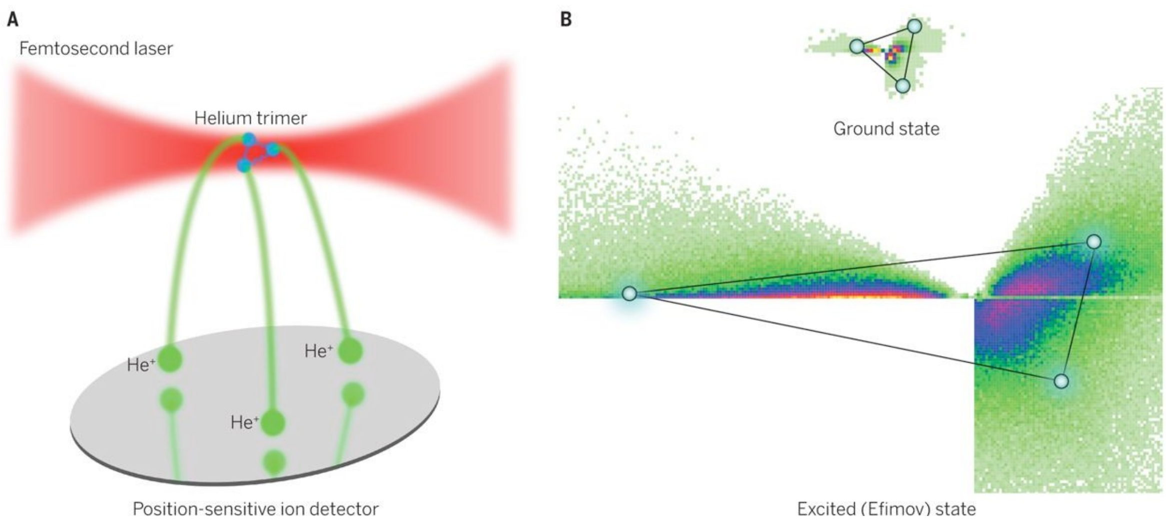


Figure 5. A femtosecond laser dissociates helium trimers. The three helium ions (A) fall onto a position-sensitive detector with timing resolution. The He trimers have two configurations (B): a small one in the ground state, and a large excited structure, which is the Efimov state. Science **348**, 551 (2015).

2.C. Quantum Information Science

A quantum system containing multiple quantum bits ("qubits") can exploit the property of quantum entanglement to create a new paradigm for computation that is theoretically capable of exponentially speeding up the time required to perform certain calculations that can be expressed in a quantum algorithm. Furthermore, entanglement provides the means to create secure communication channels on which it is fundamentally impossible to eavesdrop.

The NSF has long supported research in this area. Furthermore, the recent rapid growth of QIS should have a strong influence on the Quantum Leap Initiative at NSF.

QIS-funded fundamental studies of quantum information should be viewed as essential to the success of Quantum Leap work on quantum simulation, quantum communication, quantum metrology, and quantum technologies.

In the period 2015-2018, the NSF supported both theoretical and experimental research in QIS. Some of these projects include the demonstration of a 51-atom quantum simulator based on coherent interactions enabled by excitation to Rydberg states (Fig. 3). This simulation with an unprecedented number of controlled qubits enabled the observation of novel many-body interactions that are computationally intractable with classical computers. Additional major results with broad implications include quantum simulation of molecular energies, the demonstration of a topological Anderson insulator, and the use of entangled photon pairs as a quantum resource.

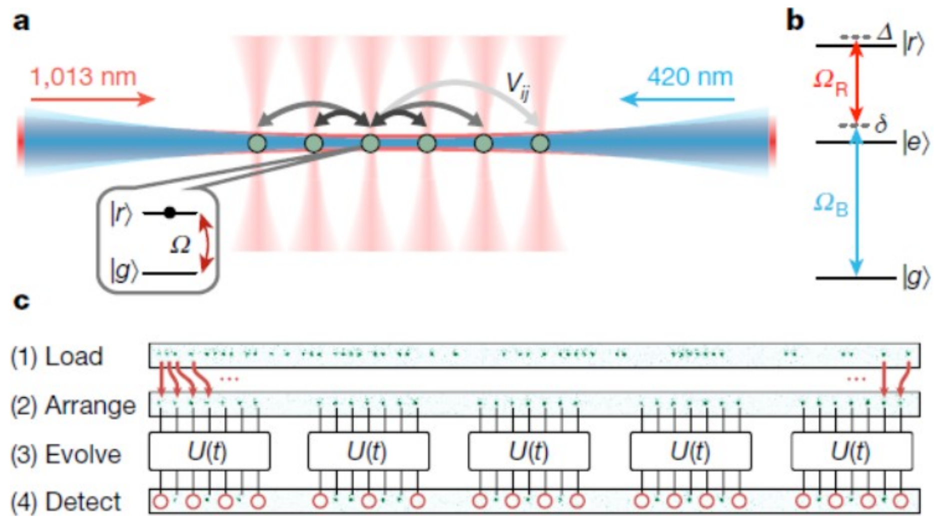


Figure 6. Probing many-body dynamics on a 51-atom quantum simulator (Nature **551**, 579 (2017)).

3. Budgets and Funding Rates

The AMO-E/AMO-T/QIS program's annual budget of $18/4.5/5M out of the PHYS annual budget of $282M supports approximately 41/14/15 new awards plus several supplements each year with a success rate of 47%/35%/30%. (These numbers are averages over the FY 2015 – 2018 period under consideration.) The high success rate is consistent with the outstanding ratings the proposals received by ad-hoc committees and panels. The program utilizes about 5-6 peer reviews per proposal, combining individual review reports and panel summaries, to select proposals for awards. Most awards are funded for 3 years. Some, such as CAREER Awards, are 5 years.

AMO-T heavily supports, along with a contribution from QIS, the Institute for Theoretical Atomic, Molecular and Optical Physics (ITAMP), which is currently located at the Smithsonian Astrophysical Observatory at Harvard University. For AMO-T the fraction of reported publications by subfield are largely reflective of the funding distribution.

The QIS Annual Report states that "The NSF MPS/PHY QIS program budget of $5 million is a subset of roughly $50 million spent by NSF on quantum information annually, averaged over 2015-2018. This $50M is distributed with about $8M in the Computer Science (CISE) Directorate; $12M in the Engineering (ENG) Directorate; and $30M in the Mathematical and Physical Sciences (MPS) Directorate."

The funding and number of awards for the three programs remained flat through FYs 15-17, with an increase in AMO-T and QIS in FY18, coinciding with the start of the Quantum Initiative (Figures 7 & 8).

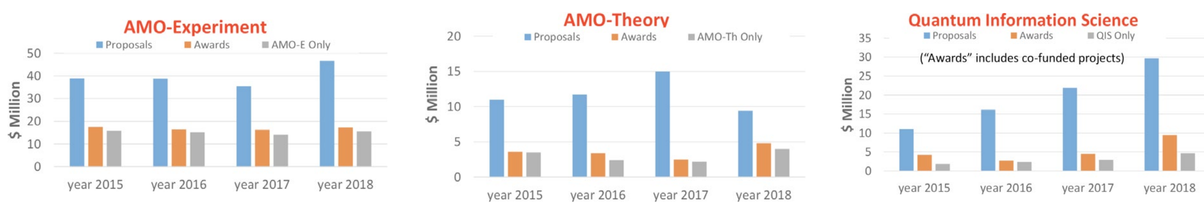


Figure 7. Budget sums for paneled proposals and awards.

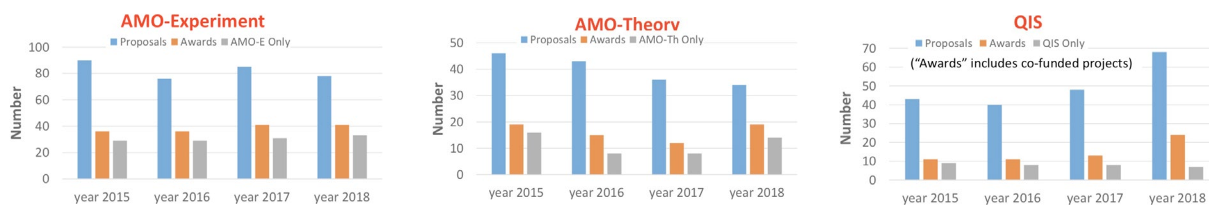


Figure 8. Number of paneled proposals and awards.

There was a significant increase in the number and funding of CAREER awards in AMO-E/MO-T/QIS, from 5 in FY14 to 9 in FY18, driven by the high quality, number, and interdisciplinary character of the proposals. They were also able to leverage significant co-funding, resulting in a total of 26 AMO-E/AMO-T/QIS CAREER awards out of 59 PHYS CAREER awards in FY 16-18 (the FY 15 information was unavailable for PHYS).

4. Analysis of the Program, its Management, and the Review Process

4.A. QIS and the Quantum Leap Initiative

Our primary concern coming out of this review is the relationship between the QIS program and the Quantum Leap initiative. It appears that the QIS program is the engine that has largely given rise to the much larger, multi-disciplinary, Quantum Leap effort. This should be viewed as an important success of the fundamental bottom-up model for innovation that has guided the NSF since its inception.

The current implementation of the Quantum Leap initiative is solely focused on activities that cut across traditional Division and Directorate boundaries, i.e. that are inherently cross- and multi-disciplinary. This has been used as a justification for separating the Quantum Leap and QIS programs, but it ignores the fact that the QIS program, even though it is solely managed by Physics, is inherently cross-disciplinary by the nature of the quantum information research challenges. This essential connection must not be lost.

We find plenty of evidence that the community recognizes the key role of the QIS program. Of the three programs we reviewed, the proposal success rate in QIS is lower than the others because of the significantly higher proposal pressure. This is only going to increase due to the Quantum Leap program's need for fundamental work in potential break-through areas in QIS algorithms as well as physical architecture down to the qubit level.

The only possible solution to this problem in our view is strong cooperation between QIS and Quantum Leap, including co-funding where appropriate. We note that there has been full integration of another Big Ideas initiative, "Windows on the Universe," with near universal satisfaction about the impact this has had on new research frontiers such as multi-messenger astrophysics.

For example, the quantum leap towards genuinely entangling the yet-unbeaten record of 14 ions in 2011 in Austria was achieved with a noise-resistant quantum gate conceived by AMO physicists. To date, more than a decade of ion-surface trap technology development in several labs in the US has been unable to match this record. Europe's QIS major initiatives recognize the critical role of fundamental science in achieving such breakthroughs. For example, see the composition of the first round of funding of the $1.1B/10yrs European Quantum Flagship in Figure 9 below. Also note the lack of investment in *quantum* software. To make an analogy, a strong investment in software would have been unwise when we were first developing the transistor. At the national level, Germany's own quantum flagship program provides an additional $700M/5yrs, for "Quantum technologies — from *basics* to markets", where the emphasis on fundamental science is recognized with a separate $3B/7yrs for hi-tech R&D where 5 out of 57 centers of excellence are for quantum *technology*.

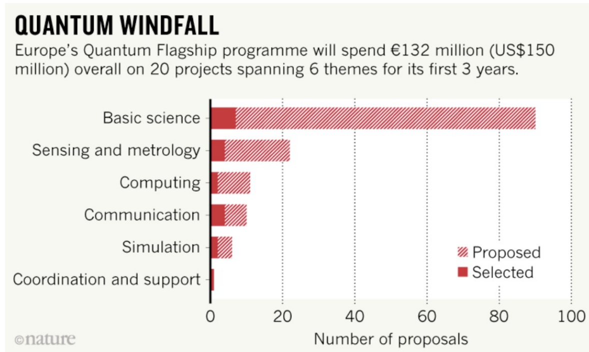


Figure 9. Composition of Europe's Quantum Flagship first round of funding, where basic science was funded at the highest level of support (image from *Nature News* 2018, data from Quantum Flagship).

4.B. Diversity and balance of the portfolio

The scientific diversity and balance of the AMO and QIS portfolios is impressively broad, and we appreciate the work and care that goes into maintaining this. The subfields are listed above in part 2 of this section.

The human diversity is a continuing issue, not only for AMO but for NSF and all across U.S. Physics. The fraction of proposals awarded that were submitted by female PIs in AMO-T/-E/QIS of 56% / 36% / 63% is consistent with their respective program success rates. Our panel did not receive further detailed information to make specific recommendations, but we do feel that the NSF has a responsibility to encourage diversity in all ways that it can. The PDs are encouraged to continue their mentoring and engagement efforts as outlined in the Physics Division Diversity Plan of 2016.

4.C. The Review Process

The proposal review processes in AMO-T, AMO-E, and QIS were extremely thorough and effective. This is due in large measure to the integrity and hard work of the PDs, who share work among the three programs. The presence of permanent or rotating PDs in all three programs contributes to the depth of knowledge about the community and contributes to the fairness of the review. Essential elements of the review process include:

- Extensive ad-hoc reviews: Each proposal receives up to seven external review requests. Program directors work together to ensure that appropriate expert opinions are sought.
- Panel Reviews: Panels in each program consider new and renewal proposals on an annual basis. Each panel consists of experts in each major subfield of the program. This can be especially challenging, since the proposals can extend from quantum simulators for strongly-correlated materials to axion searches. Inevitably some areas of science are not well-represented on the panel. We did not find that this has led to bias in the overall programs, however. An important mechanism to ensure fairness is the flexibility of the Program Directors to weigh the advice of their reviews with other information available to them.

- Detailed written summaries are prepared by the Program Directors, which not only report but also analyze the information collected by the review, often adding additional important ancillary information such as the history of the PI's work, important career milestones such as tenure, and the context of other programs such as EPSCOR.
- Co-funding is an essential element of this program, since much of the frontier work is multi-disciplinary. We found that the level of co-funding can be as high as half or more, particularly in the QIS program. This is an important way that the AMO/QIS programs add value to the greater science community across the NSF.
    - We were told that co-funding is a time-consuming activity, and that increased workload connected with the Quantum Leap solicitations was paradoxically having a negative impact on the amount of co-funding within the core programs.
    - We were also disappointed to learn that the Quantum Leap initiative has not been available for co-funding with QIS, because it is purely devoted to cross-divisional and/or cross-directorate group funding. This lack of flexibility is in our view a lost opportunity to maximize the science benefit of the Quantum Leap. Furthermore, closer coordination between Quantum Leap and QIS and AMO-T seems to us to be essential in order to prepare for the eventual integration of the QIS "pipeline" from fundamental to applied activities. *The connection of the whole Quantum Leap ecosystem to its fundamental QIS parent should be as strong as possible.*

  Each year, the program directors have summarized the relative rankings of the ad-hoc and panel reviews in a scatter chart, which we were shown. The correlation between ad-hoc and panel reviews is far from perfect. This underscores the critical importance of the experience of the Program Director, who must consider these rankings as important input, but not the only decisive factors in making an award.

We found that both intellectual merit and broader impact were fully considered in the proposal review process. In many cases the argument for broader impact is particularly important because of the connections of AMO and QIS activities to advances in condensed matter theory, particle physics, computer science, and similar NSF cross-disciplinary activities. Examples described elsewhere in this section include the ACME project on precision tests beyond the Standard Model and cold atom quantum simulators.

The staffing level for these three programs, even with the extensive review process, is probably adequately served by three full-time directors. However, the review process is under pressure because of the increased load on PDs to develop and participate in other solicitations related to NSF's Big Ideas. This pressure must be addressed, in our view, by adding additional personnel, possibly in the form of rotators (STAs) or temporary experts to work with the PDs.

4.D. Standard length of grants.

Our panel found that the customary three-year grant is often too short to complete project milestones in AMO and QIS areas, particularly in experiment, and strongly recommends a transition to a five-year grant cycle for most standard and continuing grants. We cite several reasons for this conclusion:

- Many subfields such as precision measurements, ultracold atoms, and ultrafast molecular processes have reached such a level of complexity and sophistication that five years makes much more sense than three years for milestone achievements. Thus five-year renewal cycles make sense from the point of view of producing significant science to review at renewal time.
- Graduate students working in these fields likewise have a longer path to a viable thesis than that afforded by a three-year funding cycle. Graduate students require sufficient security to participate in higher impact projects that have become more the norm in AMO.
- Some five-year grants are already standard, such as the CAREER grants, and these have been quite successful in the AMO program
- Finally, we know of no compelling arguments for not making five-year awards the standard. Suggestions that three-year grant cycles are more conservative for the programs do not convince us, since a three-year review on the basis of incomplete or insufficient progress is in fact more susceptible to mis-estimating the potential impact of a difficult project. Other arguments involving the financing of out-year commitments seem to us to be manageable by many mechanisms already in place at NSF.
- Other programs in the Physics Division, such as experimental high-energy particle or gravity research have different cultures and timelines, and so these programs may reach different conclusions on this issue. The PDs directors should therefore have the flexibility to choose the most effective funding profiles for each subfield in Physics.

4.E. Program Staffing Levels

Program directors are the heart of the NSF single-PI research program. Over the years, the AMO Programs and, more recently, the QIS Program have been privileged to have very talented PDs. That said, the turnover in program leadership has been somewhat chaotic. It appears to us that an ideal staffing level for the combined AMO-T, AMO-E, and QIS Programs would be a long-term NSF Staffer to provide institutional memory and training for rotators, and two rotators who are willing to commit to the Foundation for the maximum period of four years. Ideally, this group of three would comprise two experimentalists and one theorist, although we note that several theorists in the past have been excellent leaders of the AMO-E program.

4.F. Alternative tuition models

Finally, we wish to report on discussions we held about the tuition costs for graduate student research for NSF. We heard that at many universities the cost to a grant for a graduate student is as high or higher than the cost for a postdoc. This inverted cost curve is unstable, since it will drive many projects to eliminate students in favor of postdocs for their program. We propose that some solution must be found for this, and we wish to propose one: fixed rates for graduate tuition.

For many years NSF Graduate Fellowships have been funded according to a model where the NSF pays the institution a fixed sum *in lieu* of all tuition and fees. By contrast, program grants must budget for full tuition. In a decade of flat overall NSF funding and rising tuition, this discrepancy has grown. We propose that NSF adopt a uniform policy that treats ordinary grants in the same way as NSF Graduate Fellowships, and adopts a fixed-fee model for all graduate students. We believe that universities will be willing to accept this solution, as they have for years with NSF Graduate Fellows, as a cost that must be borne in order to enjoy the benefits of an NSF-funded faculty research program.

5. Program Response to 2015 Recommendations

a) *Proposal review, selection, and funding process.* The 2015 COV AMO/QIS Sub-Committee felt that this process was working well, and it has remained essentially unchanged in the last four years. As they pointed out, the key to a successful process that leads to final awards are dedicated and talented PDs. The Sub-Committee emphasized the importance of retaining both effective permanent staff and rotators in this roll. (The maximum term for rotators is four years.) It is abundantly clear that the current group of program leaders in QIS and AMO from 2015 to 2018 have served the Foundation and the community in exemplary fashion. We simply reiterate the recommendation of 2015 that Division leadership do everything possible to assure a smooth line of succession for the rotators and to retain a PD on the permanent NSF Staff.

b) *Program Director workload.* The number of proposals handled in 2015 --- 2018 averaged about 80/(PD*year) in AMO-E, 40/(PD*year) in AMO-T, and 50/(PD*year) in QIS. This is below the level recommended in 2015. However, the workload of the PDs has been systematically growing as more and more NSF-wide initiatives require their attention (see the discussion in Section 4 above.)

c) *<u>Status of Co-Funding</u>.* The 2015 COV Sub-Committee stated the importance of seeking out and maintaining creative funding connections between Divisions within the MPS Directorate, and even between different Directorates. Over the period being reviewed, the percentage of AMO-E proposals funded jointly were 17%, 19%, 24%, and 20% moving from 2015 to 2018. For AMO-T, the percentage fractions were 16%, 47%, 38%, and 26%. (The number in 2015 excluding ITAMP funding was 11%.) For QIS, the co-funded proposals comprised 38%, 27%, 38%, and 67% of the total number of awards; if ITAMP funding in 2015 is excluded, the number is 33% as opposed to 38%. In terms of the dollar percentage integrated over the four-year period, the averages were 10%, 23%, and 69% for AMO-E, AMO-T, and QIS, respectively. The current Sub-Committee commends the ongoing efforts of the PDs to maintain these connections, views these results as appropriate, and encourages the continuing efforts within the three programs along these lines.

d) *The NSF/DOE Partnership in Basic Plasma Science and Engineering*. Because plasma physics is now in its own Program, this will be discussed by the Plasma Program Sub-Committee.

e) *Program Income and Dollars/Award*. The 2015 COV Sub-Committee discussed the importance of maintaining a minimum grant size to allow useful efforts, and commented that the average AMO-T award size was only about $70K. Over the 2015 - 2018 period we considered, the average size of the AMO-T awards was ~$84K for standard individual investigator awards, and ~$61K for the RUI program. We believe that this represents an appropriate response to the 2015 COV concerns.

6. Recommendations

R1: QIS should integrate with Quantum Leap

As described in Section 4.A., we strongly recommend that the Physics Division work with the Foundation at the highest levels to integrate the Quantum Leap Initiative and the QIS Program, possibly in the way the Windows on the Universe initiative has been integrated into existing programs in astronomy and astrophysics. Integration could be as simple as co-funding appropriate proposals, but could extend much further, with the goal of obtaining the greatest scientific impact. This will then naturally lead to the most effective integration of quantum information scientists and engineers, along with their algorithms processors, and areas of investigation.

R2: Five--year grants should become the standard length

We recommend that the AMO/QIS programs make much greater use of the opportunity for five-year standard and continuing grants. As stated in Section 4.D., five-year awards have multiple benefits for AMO and QIS due to the increasing sophistication and difficulty of the work as well as the negative effects of 3-year reviews on students and on the panel, proposal process, and Program Director Workload.

R3: Three full--time staffers working together

We recommend that the AMO/QIS programs continue to operate in close cooperation with three full-time program Directors. As we described in Section 4.E., an ideal staffing level for the combined AMO-T, AMO-E, and QIS Programs would be a long-term NSF Staffer, to provide institutional memory and training for rotators, and two rotators who are willing to commit to the Foundation for the maximum period of four years. We recommend that an effort, accompanied by the appropriate financial resources, be made by Physics Division Leadership to ensure such continuity of leadership.

B. Elementary Particle Physics and Cosmology Theory (EPP Theory)

Program #1286: Theoretical High-Energy Physics
Program #1288: Theoretical Particle Astrophysics/Cosmology

1. INTRODUCTION

The aim of theoretical physics is to contribute to the progress of science, whose promotion is intended to advance our "national health, prosperity, and welfare" as well as our security[1]. The specific role that the elementary particle physics theory and cosmology programs ("EPP Theory" hereafter) play is multifold, as we briefly review below.

Generally speaking, EPP Theory explores, formulates, and secures knowledge of the mathematical description of natural phenomenon as manifested by observations and experiments, especially in the high-energy domain. This ranges from refining and disseminating knowledge of the basic underlying theories (the Standard Model of particle physics, ΛCDM model of cosmology, etc.), to developing new techniques and knowledge for precision analyses of our current leading theories (higher-order calculations in QCD, EFT techniques, scattering amplitude techniques, etc.), to building models that reveal alternative descriptions that are "potentially better" in some way than the current theories (more unified, theoretically more consistent, not fine-tuned, etc.), which in turn expands understanding of the impact of current data and suggests directions for future experiment (expanded Higgs structures, flavor dynamics, inflationary models, supersymmetry variants, dark matter variants, extra dimensional manifestations, etc.). These activities also frequently include suggesting new experiments and new experimental analyses at existing and planned facilities.

In addition to the tight and direct connection that EPP Theory has kept with the experimental developments and planning, EPP Theory has also benefited from a more "formal" component of its research. Research in such varied topics as string theory, conformal field theories, ADS/CFT correspondence, supersymmetric gauge theories, and strongly coupled field theories has not only improved understanding of the structure and context of current standard theories but also has given ideas on how to expand current theories into "beyond the Standard Model" (BSM) directions, which has aided the activities described above with more direct contact with experiments.

Because high energy enables probes to smaller length scales, and because higher energy is synonymous with earlier cosmological times, the work of EPP Theory has an extraordinary breadth of impact in science. EPP Theory aims to describe and understand how nature's building blocks are put together at the smallest scales, and it aims to understand the universe's origins near the beginning of time. Advances over the last three decades have brought together the two seemingly disparate disciplines of elementary particle physics, that addresses questions on the smallest distance

[1] From the National Science Foundation Act of 1950 (P.L. 81-507).

scales, and cosmology, that addresses questions on distance scales of the size of the observable universe.

The practically limitless frontier and breadth of EPP Theory present unique opportunities for transformative discoveries and ideas, along with many challenges for a funding agency to maximize these opportunities. The report below assesses, according to our charge, the effectiveness of the EPP Theory program and its Program Director (PD) to fairly and effectively identity and issue awards that promote the progress of science and fulfill the Division's mission.

2. RESEARCH HIGHLIGHTS

Research accomplishments within EPP Theory span a wide variety of applications within fundamental physics. These include phenomenology, model building, cosmology and formal physics. Overall, we find that the program correctly identified – and therefore funded – some of the most impactful research occurring in the field today. We list below a few examples of the important research supported by NSF awards.

Some of the most important experiments in elementary particle physics are those connected to the Large Hadron Collider at CERN. There are many outstanding research accomplishments among phenomenologists contributing to the success of this program on many fronts. They include enabling higher precision theory calculations to compare with data; interpretation of the data to refine, cull and in some cases eliminate theories that were previously acceptable descriptions of nature; and, suggest new experiments and analyses to maximize the experimental investment. In this latter category, among the many ideas initiated by NSF researchers includes new approaches to search for stable and quasi-stable particles.

For example, in research supported by the NSF (1620074/Mohapatra), David Curtin (while a postdoc) and collaborators emphasized the challenges of the standard LHC detectors to discover neutral long-lived particles with decay lengths greater than approximately 100 meters, which are motivated possibilities within many new physics scenarios that address the hierarchy problem, dark matter, and baryogenesis. The detectors' requirements on triggers and their acceptance criteria are well suited for other types of new physics searches, leaving the door open for experimental ideas that could cover these long-lived particles. The research has led to the proposed MATHUSLA detector, which released its physics case document (arXiv:1806.07396) last year to wide interest in the community.

Similarly, an outstanding additional suggestion with different emphasis and design parameters was pursued by another group led by Jonathan Feng and postdoc Felix Kling (as a part of 1620638/Feng). The detector, FASER, is to be placed near the beamline and aims to detect long-lived dark photons that would be impossible for the standard detectors to see. This outstanding idea accompanied by modest investment requirements has already been approved for installation within LHC tunnel in 2021[2].

The Higgs boson was not an assured discovery before it was established in 2012, nor was its mass known at that time. After its established existence and established mass, potential questions about mass generation have now become essential questions to address. For example, what explains the separation of scales of the Higgs boson, which is quadratically sensitive to the scales of almost any new physics imaginable, in comparison to apparent high-energy scales such as the Planck scale, the possible neutrino seesaw scale, and possible unification scale of forces?

Among the many new interesting analyses of this issue of the low electroweak scale, we highlight the "relaxion" idea initiated by NSF researchers P. Graham (1720397/Susskind), D.E. Kaplan (1519353/Kamionkowski and 1818899/Kamionkowski), and Surjeet Rajendran (1638509/Murayama), collaborating across three different institutions. This is a completely new approach for attacking the hierarchy problem, and is an illustration of fruitful model building work within EPP Theory that has transformed our conceptions of how nature might have organized itself. This idea does not require conspiring additional weak-scale dynamics to stabilize the weak scale, nor does it insist on anthropic explanations that put it out of reach of more traditional scientific inquiry. Implications both theoretically and experimentally are wide-ranging, as the nearly 300 citations to the original paper attest. This is also an example of the deep interdisciplinary research of modern physics, as what was once considered entirely a particle physics problem potentially may be solved through mechanisms involving cosmological evolution.

Within the cosmological realm, there have been many interesting new developments in inflationary theory, and early universe dynamics. An outstanding example of this is the work of Adrienne Erickcek (Erickcek/1752752, CAREER award), who has initiated studies of how early universe dynamics, including reheating from inflation and possible early matter phases, can affect dark matter annihilations within halos. It is a well-known problem in cosmology that there are very few experimental handles to determine what was happening in the universe before big bang nucleosynthesis. This research employs clever new theory ideas to motivate careful observations of certain types of data and to re-interpret incoming data to constrain dark matter creation mechanisms, which will test known possibilities (e.g., reheating after inflation) or perhaps will reveal inconsistencies with presently conceived possibilities. Work is progressing very nicely, for example, in predicting populations of halos from various density fluctuation power spectrum inputs to compare with observational signals (arXiv:1905.05766).

Progress in formal physics and string theory has been equally strong. This work focuses in part on deepening our mathematical understanding of gauge theory and

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[2] "FASER: CERN approves new experiment to look for long-lived, exotic particles" CERN News (7 March 2019) https://home.cern/news/news/experiments/faser-cern-approves-new-experiment-look-long-lived-exotic-particles (accessed 15 June 2019).

the connections between gauge theories and gravity. A very recent example of this is the realization, through studies of quantum entanglement and the black-hole information paradox, that there appears to be a conflict between Einstein's equivalence principle, unitarity, and quantum field theory as presently understood. At stake in these investigations are several pillars of modern physics that were previously often thought not to be at risk in our fundamental descriptions of nature. One idea for resolution is the so-called "firewall", which results in the breaking of the entanglement of particle pairs going in and out of a black hole. Key NSF-funded contributors to this idea and subsequent researchers have been Joe Polchinski (1620625/Polchinski), who was one of the originators of the Firewall conjecture, Lenny Susskind (1720397/Susskind) and Raphael Bousso (1820912/Horava). The original firewall conjecture, its defenses, and even the subsequent attacks on it through other suggestions for resolving the paradox have significantly increased our understanding of quantum entanglement, holography and other concepts on the frontier of theoretical physics.

Finally, a very recent topic to capture the attention among the formal physics, quantum field theorists and the phenomenological physics communities is the Swampland conjecture, initiated by NSF researcher Cumrun Vafa (1719924/Georgi), with important further development shortly thereafter by Arkani-Hamed, Motl and Nicolis (hep-th/0601001), who originated the related weak gravity conjecture. The idea is that there are many theories that are inconsistent with string theory, but appear to be perfectly valid low-energy quantum field theories. Such theories reside in the so-called "swampland". Theories that might be in the swampland include theories that purport to have unbreakable global symmetries, theories whose masses and charges violate some inequality conditions, and wrong signs for higher-order operators in the effective action. The Swampland conjecture in inchoate form was discussed nearly 15 years ago by Vafa; however, the power of its implications are only now starting to be appreciated by the wider community. For example, more than a third of all the references to the original paper have occurred in the past 12 months. Work is continuing on the formal side to test the conjecture in a variety of circumstances, and the implications of the conjecture are being investigated by phenomenological theorists, yielding questions about the appropriateness of various beyond the Standard Model "modules", such as global symmetries, extremely light states, and various assumed higher-order operator coefficients. We expect much further progress and understanding to be revealed in this fascinating direction in the coming years.

3. MERIT REVIEW CRITERIA

The PD employed a combination of mail reviews and panel reviews to assess the EPP Theory proposals. The mail-reviewers are experts in the specific, and sometimes highly-specialized, areas covered in the proposal. The panels served two functions. First, they reviewed the recommendations of these reviewers, addressing reasons for possible differences between the assessments in different mail--in reviews that were evident in some cases. Second, and perhaps more importantly, the panel provided a prioritization of various scientifically important and scientifically sound proposals in *different sub--areas* that would be very worthy of funding but could not be funded only because of the limited dollars available for the program. We are impressed with the effort the PD has made to ensure fairness, transparency of the selection and prioritization processes, while recognizing the importance of communicating the rationale behind the decisions to the PIs.

The PD's efforts to ensure that all EPP Theory proposals are to be reviewed by experts in the field are to be commended. We found that the mail--in reviewers, the panels and the PD's analyses all paid attention to both the intellectual merit as well as the broader impacts of the proposed work. The former included not only the scientific soundness of the proposed research and the potential for advancing knowledge of the field, but also whether this research could be potentially transformative. Recognizing that not all good and very necessary research will turn out to be transformative, we are very satisfied that excellent and essential solid research needed for progress in the field was funded along with high--risk/high--reward proposals. The two directions are truly complementary.

Starting in FY18, as the result of increasing numbers of proposals and the inevitable limitations of staff support, the PD was forced to adjust the program review process, replacing several of the external mail--in reviewers who wrote reviews for each proposal with panel members who also wrote formal reviews. We are aware that the PD had resisted this change for many years, even as other programs within the Division had increasingly adopted this practice (sometimes in even more severe forms), and that this change was adopted as the result of discussions with Division leadership. We understand that this reduces the burden NSF imposes on the external reviewing community, as well as the burden on internal NSF support staff in sending out and managing reviewer invitations. We are nevertheless concerned about potential impacts of this practice for the reviewing process, especially as it might tend to artificially ease the gap between the perceptions of mail--in reviewers and those of the panel, thereby potentially restricting the full range of reviewer. We understand that this is really a Division--wide issue, and hope that this process can continue to be managed with great care (as the PD is doing), perhaps by continuing to ensure that the number of expert mail--in reviewers be more than the number of panel members serving as reviewers of a proposal.

We found that most of the mail--in reviews provided substantial comments that described the rationale behind the final rankings. We believe that such detailed comments prove to be very helpful to the panels and to the PD, particularly in those cases where the different mail--in reviewers arrive at significantly different ranking for the same proposal. This was evident from both the panel summaries and from the PD's review analyses. There were even times when the PD made interpretive and evaluative judgments along the lines, "Prof. X's review read more like "Excellent" rather than Good/Very good". We are very impressed with the detailed review analyses by the PD. This was especially important for the (relatively few) cases where his decision differed slightly from that of the panel. We felt that he spelt out his rationale very clearly, and took the responsibility very seriously. As already remarked above, we were especially impressed by the PD's effort to communicate in detail the reasoning for declined proposals, or for renewal proposals that were funded at significantly lower levels.

Broader impact on society forms an important part of NSF proposals. Typically, these include open houses, visits to local area schools, community lectures, and mentoring of women and minorities in STEM areas, among other things. Wider reaching are appearances by NSF scientists on TV and in other media explaining science to the interested public, and authorship of popular science books. We were especially struck by Williams' (1652066/Williams, CAREER award) after-school program covering various aspects of every-day science experiences that reaches out to the local Native American population, and culminates in a trip to the Lederman Science Center at Fermilab for top ten performing students. There have been some very high profile successes of the outreach efforts supported by NSF in EPP Theory over the years. This includes David Kaplan's acclaimed 2014 movie *Particle Fever*, and the TheoryNet program directed by Tom Taylor and Brent Nelson that allows university professors to visit classrooms and pair up with high school teachers for one or two years.

Broader impact clearly forms a significant portion of the NSF CAREER awards. Indeed, as the PD says, "NSF's CAREER grants are not research excellence prizes. Instead, they are designed as an alternative funding mechanism targeted to those unique individuals who feel a special calling to engage in major educational/outreach activity during an early pre-tenured stage of their careers". We appreciate and recognize that outreach and education are an important part of NSF-sponsored activities, and we applaud the fantastic results of these efforts. Josh Ruderman's (1554858/Ruderman, CAREER award) setting up a science journalism competition to fund journalism students to visit HEP facilities and write about their experiences has resulted in science reaching a very broad audience, with expected impact continuing well beyond the lifetime of the award. Despite this we are concerned whether this strong emphasis on education and outreach at the early stage of a scientist's career, as is inherently part of the CAREER program, is beneficial to a young researcher. Also, another federal agency has an award with a very similar name that is a pure research excellence award, and these awards are viewed as "equivalent" by many university departments, with equivalent pressures to apply for them, even on those who have little desire, ability or expertise in public outreach at the early stage of their career. This question has a wider scope than our EPP Theory review.

4. INTEGRITY AND EFFICIENCY OF THE PROGRAM’S PROCESSES AND MANAGEMENT

The intrinsic interdisciplinary breadth and intellectual depth of the research areas covered within the EPP Theory program present unique management challenges. We commend the PD for his outstanding performance in handling these challenges and running a highly efficient and scrupulously cared-for program with great integrity. We find that under the PD’s guidance, and strong support of the division when necessary, EPP Theory has flourished even within the current tight funding climate and, as a result, it is well positioned to continue and indeed expand its leadership role in supporting U.S. research in theoretical elementary particle physics and cosmology.

A. QUALITY AND EFFECTIVENESS OF THE PROGRAM'S USE OF THE MERIT REVIEW PROCESS

The PD takes extraordinary care in ensuring that at multiple levels, the quality and effectiveness of the merit review process is maintained. We note that as the PD himself is a highly accomplished physicist, with impactful research ranging from formal theory to physics beyond the Standard Model to dark matter physics, he brings crucial breadth to the management of the review process. Not only does the PD know the field very well, he also knows the community well and is highly tuned to listening to the community as a whole. The PD thus clearly draws upon this considerable knowledge to select appropriate reviewers for proposals, constructing diverse and balanced review panels, and providing definitive assessments of the efficacy of the review process and panel discussions when making his final recommendations. He clearly values the integrity of the review/panel process and listens closely to the themes that emerge in the process when considering a specific proposal. His review analyses of proposals are detailed and meticulous reports, with careful reasoning provided for the underlying recommendations.

The PD is also to be commended for his very high degree of transparency in his communication with PIs, as well as proposal reviewers and panelists, as to the review process (and indeed, all relevant aspects of program management). This allows for a culture in which PIs can expect further helpful feedback from the PD if they request it, which ultimately is to great benefit for the community as a whole. The level of transparency and integrity provided in EPP Theory is an outstanding model for federal research funding agencies.

B. SELECTION OF REVIEWERS

The PD has consistently done an excellent job of selecting reviewers and panelists for the merit review process for EPP Theory. For the external reviews, the PD is meticulous in ensuring that the reviewers have sufficient expertise and a significant number of the reviews are at "arms-length". For the selection of panel members, there is clearly an emphasis on breadth and diversity of panelists in a wide variety of categories. The factors considered include the type of institution (private universities, public universities, national laboratories, and liberal arts colleges), the age/experience level of the panelist (junior as well as senior people, with the specific and well-motivated criterion that all panelists should have been successful in securing federal research funding at some point in their careers), and having representation from members of underrepresented groups (which include women of all races/ethnic backgrounds as well as racial/ethnic minorities). The PD is particularly to be commended for his emphasis on diversity and his awareness that given the relatively homogenous state of the field, there is a concern of over-burdening physicists in underrepresented groups.

C. MANAGEMENT OF THE PROGRAM UNDER REVIEW

As stated, the PD's management of the EPP Theory program is exceptional. He has guided the program to financial stability in a tight funding climate and in the wake of several fiscal challenges, which at times have been severe. They included untenable prior commitment levels at the beginning of his tenure and, more recently, a dramatic increase in the number of EPP Theory proposals that resulted from external shifts in the funding landscape (see Sec. 7B) as well as the dissolution of the Mathematical Physics (Math Phys) program at the NSF in FY2014. The removal of this program was logically poised to take a heavy toll on EPP Theory, as brought up in the 2015 COV report. As a result, the PD brought the issue to the division leadership, and the base budget of EPP Theory was augmented appropriately. We applaud the PD and the PHY division leadership for their positive and constructive response to this development.

We note that the continued increase in the numbers of proposals to EPP Theory has placed a heavy burden on the administrative management of EPP Theory. Given that this trend is likely to continue, it is important to consider constructive responses that are feasible given the current constraints. As mentioned in a previous section, one example cited is the PD's decision to "streamline" the review process in FY2018 to reduce the number of external reviews and instead require more than one member of the review panel to provide an explicit graded review for each proposal. The PD has expressed that this new model worked well, and that it will continue at least for the near future. This process does indeed reduce the administrative burden, and indeed provides for greater convergence between the reviewers and panelists, but this greater convergence is an expected outcome of such a model. We encourage the PD to continue to keep his watchful and critical eye on this process to ensure that the quality and integrity of the review process are maintained.

D. PORTFOLIO

The portfolio of research covered by EPP Theory is exceptionally diverse. The research areas covered include formal theory, mathematical physics, beyond Standard Model physics frameworks, collider physics, QCD, dark matter physics, and astrophysics/cosmology. The program also has great diversity in terms of institutions, including supporting a significant number of university theory programs at private and public institutions, as well as small colleges. The depth and scope of the research portfolio of EPP Theory speaks to the vitality and excitement of the field.

5. DIVERSITY AND INCLUSION

The EPP Theory program shares the goals of MPS and the NSF to broaden participation among women and underrepresented minority groups. On year-by-year basis, FY15 to FY18, there was 13%, 17% 22%, and 16% representation of women among (co-)PIs, and 11%, 8%, 3%, and 10% representation among underrepresented minority groups. These rates are consistent with the pool of proposals submitted to the program. Furthermore, the outreach aspects of EPP Theory have a significant component intended to invite and include women and underrepresented minorities into the scientific enterprise. We urge EPP Theory to continue these efforts on outreach, which we hope will increase the pool of researchers from under-represented groups, and to remain diligent in its quest to broaden participation.

6. RESPONSIVENESS TO 2015 COV

A primary concern of the 2015 COV was the dissolution of the Math Phys program within the PHY division. This disproportionately affected the HEP theory community, as many PIs in the "formal" side were either fully funded or co-funded within the Math Phys program. There is no question that this has "tightened" up the competition within EPP Theory, as proposals previously considered by Math Phys were added to EPP Theory. Although the dissolution of Math Phys continues to have aftershocks within the community, we believe that PHY has been fair in its re-allocations of Math Phys budget to enable continued support of the top work in this important subfield.

Another area of concern for the 2015 COV was the financial stress of the EPP Theory program. Presumably this concern was raised after seeing the extreme competition among excellent proposals for the limited resources available, and this is also what we are witnessing in this review. As was discussed within the 2015 COV, the issue for us is not if total allocated funds are appropriate, but rather if the funds that are available are awarded fairly to maximize science progress and the mission of the NSF. We conclude, as the 2015 COV did, that the EPP Theory program is doing very well in making wise award decisions that are informed by expert reviewers and panels whose results are communicated openly and thoughtfully to PIs.

The 2015 COV also echoed a concern from the 2012 COV regarding the ability to support graduate students adequately. This has been a perennial concern of all theoretical physics sub-disciplines, and we suppose it will remain a concern for some time to come. Among the many issues involved, a particularly wrenching concern is when there are funding changes to the relevant PI(s) midstream in a graduate student's career. Given the extreme competition for limited resources, there can be times when a new award does not receive as much funding as the previous award, leaving PI(s) with the difficult problem of not being able to fund a graduate student on the cusp of graduation. There are many subtleties associated with this problem, including the fact that some universities have resources to step in and fund the student and others do not, but there are circumstances where it is uncontroversially appropriate for EPP Theory to provide temporary relief to enable a student to finish, which it has at times. We consider this to be a part of the responsiveness to 2015 COV concerns, and we encourage EPP Theory to continue to retain limited resources in reserve for such circumstances, as it has been doing, and to continue to be sensitive to specific graduate student funding needs when issuing new awards.

7. CHALLENGES WITH EMERGING NEW OPPORTUNITIES

A. RESPONSIVENESS TO EXPERIMENTAL RESULTS and INTERDISCIPLINARY TRENDS

There has been significant progress in experimental particle physics and cosmology in recent years. In the particle collider physics arena, for example, the LHC has continued its operation of proton-proton collisions in the Run 2 phase from 2015-2018 at 13 TeV of center of mass energy, and it has collected approximately 140 fb^{-1} of integrated luminosity per experiment. Significant improvements of measurements of the Higgs boson properties and decays have resulted, although analyses of the latest data are still ongoing before results are released. Searches have continued for BSM physics, including extra Higgs boson states, superpartners, new forces, evidence for non-renormalizable operators, effects of small extra dimensions, non-standard flavor dynamics, etc. Although there are some potential signs of stress of the SM that are currently under investigation, there is as yet no evidence for results that cannot be accounted for by the SM. Nevertheless, we have learned much about what nature can and cannot tolerate, and we have learned that at least the vanilla versions of many pre-conceived BSM theories are either ruled out or have their parameter spaces significantly constrained. Meanwhile, other BSM theories are still consistent and largely unconstrained by the current level of experimental results, but will be probed in future runs of the LHC or at future colliders and experiments.

On the cosmological side a key issue has been to identify the nature of the dark matter in the universe. There is compelling evidence that dark matter must exist from a variety of sources, but there has been no experiment or observation not involving gravity that has provided information about its precise identity. These include many ground-based experiments looking for dark matter interactions with SM particles (nuclei, photons, etc.) and satellite/telescope experiments looking for dark matter annihilations or decays into SM particles (monochromatic photons, positrons, etc.). From these experiments we have learned much about what dark matter is not, which narrows the options of what dark matter could be.

Many other experiments are playing significant roles in shaping our understanding of nature and thus shaping the activities of EPP Theory. These include gravity wave detectors, electric dipole moment searches, pulsar timing arrays, B physics experiments, g-2 precision analysis, neutrino physics, nucleon decay and oscillation experiments, etc.

The fast-moving comprehensive nature of experimental research presents extraordinary opportunities and challenges to EPP Theory. A balance of expertise is needed between disciplinary theory work devoted to maximizing the impact of single experiments (e.g., higher-order calculations to observables) and more broad and inter-disciplinary theory work devoted to understanding the connections (and thus ultimate impacts) of a wide variety of experiments spanning neutrino detections, CMB measurements, gravity wave signals, high-energy collider searches, precision atomic physics measurements, and more. An important challenge is to ensure a proper balance between the narrow-focus theorists, the broader-focus "model-builders" and BSM phenomenologists, and the field theorists and string theorists with a more formal bent, all of whom are essential to progress. We note that formal developments within EPP Theory are having significant interdisciplinary impacts in pure and applied mathematics, and vice versa, and even within condensed matter physics.

The interdisciplinary breadth (cosmology, colliders, gravity, atomic physics, mathematics, etc.) and disciplinary depth (years of devoted "narrow" training to do higher-level computations or to understand specialized theory frameworks, etc.) of EPP Theory makes it particularly difficult on the review process. The number of external reviewers needed and the breadth of the panel required for full evaluation are particularly important challenges to the EPP Theory program and the community. The panel make-up is especially important since it is there where difficult narrow/broad sub-community discussions are most likely to arise and be resolved. We commend the PD for putting appropriate emphasis on outstanding reviewers and panels of sufficient breadth (and disciplinary depth) to make appropriate recommendations. Continued diligence on maintaining extensive external reviewing with a strong panel of broad expertise is paramount for the continuing success of EPP Theory, which displays profound interdisciplinary interests and which seeks to build on and revise theory paradigms that respond to the results of recent and near-future transformative experiments.

B. CHANGES IN THE BROADER FUNDING LANDSCAPE

The vitality of research challenges within EPP Theory has sparked much interest in the public and among prospective students. This interest is driven by the expectation that revolutionary new ideas will be needed to fix the recognized shortcomings in our current understanding of nature, and unforeseen novel directions will emerge as we explore this practically limitless frontier. The interest has led to an influx of private investment, which has been very much welcomed within the community. Investment and gifts have included individual awards programs, privately funded centers at universities, and organizations that fund researchers or research proposals similar in scope to federal agencies that have traditionally funded EPP Theory research. This has drawn the attention of universities and is partly why the subfield continues to grow and be a primary interest among students wishing to pursue physics.

Changes in the funding landscape have led to new opportunities and challenges for the EPP Theory program at the NSF. EPP Theory is experiencing a significant increase in the number of proposals among university-based researchers requesting federal research support due to the interest and growth of the field at universities, and also due to the recognition that the NSF is a leader in welcoming bottom-up curiosity-driven (as opposed to programmatic) university theory research, the primary driver of innovation in theoretical research. On the one hand, the increased number of proposals signifies tighter competition. On the other hand, the threshold level for funding outstanding theoretical work was already very high, with many excellent proposals going without funding. This puts stress and strain on the EPP Theory program within the division and also on PIs who are doing excellent work without financial support. To this issue, we encourage close communication with other federal funding agencies and private foundations in order to exploit for scientific gain emerging opportunities to fund high-value theory.

It is not within the purview of this report to question funding levels for programs, but rather to provide feedback informing of changes in the community and how the NSF may most effectively utilize their resources to promote an outstanding and balanced research portfolio. As such, we have been very satisfied with the management and approach to funding within EPP Theory, which has maintained its curiosity-driven stance, and has enabled support of high-impact theory research (e.g., see research highlights section) that would not otherwise be captured by top-down "calls". There have been recent perceptions of NSF increasing its leadership in supporting creative new developments in theoretical physics. We encourage EPP Theory to maintain this leadership position in this fast-changing external landscape.

C. Elementary Particle Physics Experiment and the Large Hadron Collider

Introduction

The Experimental Particle Physics (EPP) and Large Hadron Collider (LHC) programs support experimental, often accelerator-based, particle physics research that measures the properties and interactions of elementary particles, and explores the highest energy frontier interactions in search of evidence for possible new particles and forces that would lead to breakthroughs not only in our understanding of particle physics, but also of cosmology and astrophysics. Progress in EPP and the LHC is closely coupled with advances in accelerator, computing, and detector technology and has driven important progress in those areas. The EPP experiments have been an excellent training ground for students and postdoctoral scientists, who have gained important expertise in forefront technology and data analysis with many possible applications beyond particle physics research, to the benefit of society as a whole.

EPP has made a high priority of supporting the Large Hadron Collider (LHC) program, including the research of university groups on the ATLAS, CMS, and LHCb experiments, as well as the operations and upgrades of these experiments. Support of the LHC experiments upgrade program towards the High Luminosity phase of the LHC (HL-LHC) is well underway via a MREFC process. With the HL-LHC, we will enter a new phase of research, where precision measurements of the properties of the recently-discovered Higgs boson— a particle unlike any humankind has ever seen— will shed light on its role in giving mass to matter, and on its interactions with the other standard model particles. These measurements could also provide clues to where new particles and interactions might be found beyond the standard model of particle physics. In this new era, the Higgs boson can be used as a tool for probing possible new interactions or discovering new particles, and searches for dark matter candidates take center stage. In addition, searches that could lead to an evidence for supersymmetry, or extra dimensions and other new paradigms, are intensified.

EPP also has had major contributions to the U.S. neutrino program, with strong support for the short- and long-baseline programs, which are providing precision neutrino interaction measurements as well as the development of new technologies. The current U.S. neutrino program is centered on the Short-Baseline (SBN) program at Fermilab, where NSF physicists are continuing to take on leadership roles in all aspects of the experiments. The DUNE program will become the flagship U.S. on-shore particle physics program starting in the mid-2020's. It holds the promise of making key precision measurements of the oscillation parameters associated with neutrino mixing and has the capability to perform a definitive search for CP violation in the neutrino sector. In addition, the very large DUNE detectors will expand the sensitivity to supernova detection and baryon number violating processes such as proton decay and n-nbar oscillations.

Scientific Highlights

The four-year cycle of this COV review has been an eventful one for particle physics. Some of the science highlights include:

- The **liquid argon detector technology has come to fruition** with both the MicroBooNE experiment and the R&D for the future DUNE experiment that is being accomplished through the ProtoDUNE program.
- The **oscillation parameters associated with 3--neutrino mixing** are now measured with good precision and some early information on the mass hierarchy and neutrino sector CP violation is starting to become available.
- **Higgs couplings to third generation quarks and leptons**: The LHC is a Higgs factory. Couplings to 3^{d} generation were established in the past 2 years by the LHC collaborations, with the observation of the Higgs to bb, Higgs to tt, and ttH. This was confirmation that the boson that was discovered in 2012 at the LHC is the standard model Higgs boson (H particle) that was searched for. The H particle is now also extensively used as a tool for new physics searches.
- **Discovery of pentaquark states:** In 2015 LHCb collaboration reported the observation of pentaquark-charmonium states decaying into a J/ψ meson and a proton. Proposed as exotic particles by Gell-Mann in his 1964 paper on the quark model, these pentaquarks— hadrons consisting of five quarks— are now observed. Following up in early 2019, the LHCb collaboration announced the discovery of a new narrow pentaquark resonance, having a more complex structure consisting of two narrow overlapping peaks, similar to that of a baryon plus meson, but with lower total mass.
- **Discovery of CP violation in charm particle decays:** The LHCb collaboration recently presented the first observation of CP violation in charm particle decays. Differences in the properties of matter and antimatter, arising from CP violation, had been observed in the past using the decays of K and B mesons, i.e. of particles that contain strange or bottom quarks, but never before in the charm sector.

I. Quality and Effectiveness of the Merit Review Process

1. Appropriate Review Methods

The merit review process is based on both ad hoc reviews, which are solicited from the un-conflicted peer community and submitted in written format, and on review panel discussions and reviews, which take place in person and are informed in part by the ad hoc reviews, as well as by the submitted proposals and the panel interactions. The combination of ad hoc reviews and panel deliberations brings together complementary merit review methods and information which can be used by the program Directors to make final funding decisions. For example, the ad hoc reviews generally go into more of the proposal details, while the panel reviews enable comparative assessments amongst the proposals.

Observations from the e-‐jacket sample reviews:

It is noted that having enough ad hoc reviewers for the larger, multi-‐PI/multi-‐project proposals is essential, so that sufficient expertise is obtained on the broader range of topics. In addition, in the case of the larger LHC experiments, it could be important to utilize ad hoc reviewers from within the same experiment who are un-‐conflicted, but who could comment on the importance or impact of particular areas of contributions.

A weakness of the ad hoc reviews is that sometimes they appear to be superficial, and they are inconsistent in depth. For the most part, the reviews were very detailed, but in some instances, multiple ad hoc reviews missed clear issues and shortcomings that were surfaced in the panel discussions. Since in one example, even four ad-‐hoc reviewers failed to spot a particularly problematic statement in an ad hoc review, EPP should ensure that clear guidelines and expectations for the reviews are given, and perhaps further training of the reviewers would be beneficial. We suggest it could be useful to create a template that might more strongly encourage reviewers to address each review criterion clearly, rather than only sending them a list of criteria that could be taken only as a suggestion. In addition, in some cases the ad hoc reviews lacked substantive comments to explain the assessment of the proposals. For example, statements of support for groups that are known or have a strong history within experiments may be made by reviewers in spite of missing evidence in the review that the group's current and proposed roles remain substantial and relevant.

Panel discussions appear to be extremely helpful, as they are (a) based on consensus, (b) can draw on closer experience with PI's contribution to one experiment, and (c) can provide broader expertise, such as amongst hardware, computing, and analysis efforts. For the panel reviews, it is important to continue to maintain a good balance of expertise, which is done well currently.

In evaluating a group proposal, assessing the impact of individual PIs is important, and good practice. However, keeping in mind that a good balance of efforts must be achieved at the level of each group, and that individual PIs should be allowed to specialize in certain areas, care should be taken not to move toward a rating system for individuals that would require PIs to abandon their expertise in favor of breadth.

It is important that PIs carefully report and describe their past and proposed activities, including places where their efforts might be supported by different agencies/programs. It is noted that the program directors do recommend to PIs who have, or anticipate having, concurrent sources of support (including but not limited to grants from other agencies or private foundations, and/or laboratory appointments), that they clearly explain the relationship between the proposed activities and other funded or pending activities.

It is commended that particular attention has been directed to fostering the development of the research program of junior PIs, and it is noted that the reviews are generally supportive of the efforts and leadership development of postdoctoral researchers as well.

Site visits, which were recommended by the 2015 COV as a complementary method of review, did not appear to be implemented. This COV panel is divided on the question as to whether or not this is important. Reasoning that they aren't necessary: the panel system can do comparative reviews while a site review will only review a few groups in a non-comparative way. Smaller groups might be at a disadvantage. Reasoning that they can be an important tool: There is value in having program Directors visit sites periodically (~6-7 years) to get a sense of facilities and individual PI efforts, as well as to meet new PIs, but care should be taken not to visit/favor only larger, well-established groups.

2. Merit Review Criteria

Both the ad hoc reviewers and the panel are charged to address both the Intellectual Merit and Broader Impact criteria for the proposals. Most of the ad hoc reviews did address both the Intellectual Merit and Broader Impact criteria, but some reviews had little assessment of Broader Impacts. Perhaps this could be more strongly emphasized through an explicit form such as a template. The panel reviews consistently addressed both criteria.

While the documenting of Broader Impacts is usually done properly within the proposals themselves, for a few proposals it seemed to be an afterthought or to be describing a completed activity as something that is still coming in the future. We recommend that Broader Impact criteria continue to be carefully reviewed, and that the PIs continue to be encouraged to integrate and document their *current* activities in this area as well as any outcomes they have achieved.

For the panel reviews, it is important that the basis and details that led to the ranking and overall assessment is documented in the report in order to give the proposers important feedback on the strengths and deficiencies along with the procedure for the assessment.

The program Director reviews were very thorough, and it was clear that they synthesized the information from the ad hoc and panel reviews to make a judgement on funding. Of course, a funding decision for any given proposal extends beyond merit reviews, and brings funding availability into consideration, together with the overall merit and priority assessment. The panel rankings appear to be one of the prime inputs for the funding decision, which is appropriate.

3. Individual Reviewers Comments

The ad hoc reviews were generally found to be helpful in that reviewers could go into more depth on the proposal, but the comments were sometimes inconsistent, or insufficient in depth and knowledge. It is noted, however, that in general the evaluation of the RUI proposals (proposals from institutions with only undergraduate programs), often done by RUI PI peers, tended to be careful and thoughtful.

In the 2018 EPP report, after which four RUI programs were supported, it was proposed that RUIs may suffer in the reviews of the Intellectual Merits, which often get mixed ratings from reviewers. Since PIs at these institutions often have high teaching loads, methods of encouraging effective cooperation and collaboration between them would be beneficial *(see also section 4)*. In some cases there were ad hoc reviewers that could geographically attest to the potential impact of the application, and these reviews were extremely helpful.

Two previous comments by the 2015 EPP COV report appear to have been addressed: 1) ad hoc reviewers have curtailed their proposal summarization text and mainly now provide text on evaluation and assessment and 2) some panel members are now included as ad hoc reviewers. Both of these modifications appear to be beneficial to the review process.

It is recommended that the ad hoc reviewers be strongly encouraged to give sufficient detail so that the proponents can understand the basis for the rating they are given. The perceived strengths and deficiencies of the proposed program should be clearly stated by the reviewers so that PIs receive important feedback and so that the process is as transparent as possible.

4. Panel Summaries and Consensus

The panel summaries were clear and concise. The "review analysis" notes from the program director, which were very helpful, show that the deliberations of the panel are careful and quite thorough. The panel reports typically state the facts and assessments of the work plan, and record what considerations were used in making their final rating and ranking for a proposal. Nevertheless, some reports, especially those ranked in the "good" category, are short on details as to how the final assessments were made. These details on the assessment should be explicitly documented in the panel report so that they can become important inputs for the PIs to see how the process worked, and what they can improve on in the future. It is important for the panel to justify the logic used for final overall ranking so that the proponents can understand the process and the feedback on their proposal.

In the 2018 EPP report, the NSF made a careful analysis of the geographical distribution of reviewers and grants awarded. Since the geographical distribution was less balanced than the demographic distribution, it was suggested that EPP encourage the development of a programmatic strategy to foster cooperation amongst smaller groups, which are more isolated regionally, in ways that lead to more effective contributions. Additionally, extra care should be given to support the travel of panelist from regions which are farther away from NSF sites, where panel reviews are typically held.

5. Documentation for award decision in the Jacket

The award decision documentation has been generally well done: program director analysis reports were very thorough. It is useful to have the details regarding the many steps of the review and reporting, and the process of reaching a final decision which is typically built on consensus.
The jacket documentation of their analysis provides a rationale for the decision more clearly than the ad hoc reviews and the panel summary, and gives an excellent summary of the components and logic that went into the final decision.

The award decisions for almost all of the proposals were very consistent with the ad hoc and panel reviews and reports. With limited funding, the program Directors cannot fund all proposals, and thus they use the panel rankings to help determine which proposals to fund. Other information from the reports and assessments is brought in to adjudicate proposals near the ranking cutoff and for determining the funding allocation for successful proposals with respect to the requested funding. This method is very appropriate, and allows the program Directors to tailor the program to take the best advantage of the available funding.

Information about the proponent's previous funding source may also enter into the decision, such as when a PI is proposing to move to NSF funding from some other source. There may be good reasons for the PI's move request, since NSF has different mandates than DOE associated with Broader Impacts, especially with respect to mentoring underrepresented group. These types of situations and the diversity information should be part of developing the final decision on funding.

6. Documentation to the PI for award decision

The PI has access to all of the ad hoc reviews and the panel reviews with reviewers' names redacted. This is important information for the proponents in conveying the strengths and weaknesses of the proposal and also in giving descriptions of how the ratings were reached. For this reason, it is critical that the ad hoc review reports and panel review reports lay the basis for the ratings and what were the strengths and deficiencies of the proposal.

The panel review reports which are given to the PIs should be detailed enough to contain the elements that led to the final recommendation on funding support and funding levels. This was typically true for the EPP proposals but there were deficiencies in a few panel reports with respect to providing this information.

In addition, since some lead PIs do not always share feedback and guidance from the agency openly, it would be helpful if the final information that is transmitted from the program directors to the PIs (e.g. regarding re-budgeting) is made available to **all** PIs on a multi-PI grant, so that all proponents are given feedback in a way that allows them to react and optimize their submission on the next iteration.

II. Selection of Reviewers

Reviewers are selected based on their expertise and relevant knowledge in the areas represented by the received proposals. EPP has about 30-40% of proposals that are in 3-year cycles in any given year, and these are anticipated in advance. Thus, many relevant, unconflicted reviewers can be selected in advance for the panel and ad hoc reviews. The balance are selected based on new or previously unfunded received proposals. EPP attempts to obtain reviewers whose expertise can be in the same or similar areas. In addition, based on the previous COV recommendations, EPP selects some members of the chosen panel reviewers to serve as ad hoc reviewers for the proposals in that cycle as well, as was recommended by the 2015 COV. EPP is able to find new reviewers through previously successfully-funded PIs, through rotating IPAs who know different people in particle physics, and through word of mouth.

The subpanel finds that the reviewers generally have the expertise and experience to judge the proposals fairly. The program directors also select reviewers from relevant types of institutions such as some number of reviewers from RUIs. The subpanel notes that for proposals associated with large collaboration experiments, it is important to have some members of the experiment as reviewers in order to be able to assess the impact of the proposed work. Of course, care should be taken to avoid conflict of interest issues.

III. Program Management

1. Management of the program

The EPP and LHC program is fortunate to be managed by knowledgeable, interactive, and conscientious directors: Saul Gonzalez, Randall Ruchti, Mark Coles. It is clear that they work very hard to help facilitate the scientific endeavors of an extremely broad and talented pool of physicists. These PIs are attempting extraordinary experiments with cutting-edge research techniques and exciting ideas, some of which could be transformational for our understanding of particle physics. The program directors go out of their way to understand the research that is being proposed and to find ways to accomplish ambitious scientific goals and priorities.

A major accomplishment of the program directors during this period is the successful **MREFC** process for the HL-LHC, at the level of $150M shared between ATLAS and CMS, which has required close coordination with the DOE and the LHC experiments by Mark Coles, together with Randy Ruchti and Saul Gonzalez. Large-scale funding at the MREFC level is necessary for NSF to play significant and visible leadership roles in the HL-LHC, which has been set as the highest U.S. priority in HEP by the 2014 P5 report. We congratulate EPP on this important success.

Another important milestone has been achieved by the program directors in moving towards NSF support for the DUNE neutrino experiment project, which is also one of the highest priorities in the P5 report, and which will anchor the on-shore U.S. particle physics program for the next decade and beyond. In 2018, EPP funded a multi-institute collaborative design grant for $1.62M for completing the engineering and prototyping of the Anode Plane Assemblies (APAs) for the multi-kiloton DUNE liquid argon detector. Work funded through this collaborative grant from NSF, which includes PIs with base grants from both DOE and NSF, led to the completion of the APA fabrication design, thus achieving an important milestone for DUNE. This sort of program has the promise of providing a path for NSF to partner with DOE in realizing the DUNE experiment and its physics program, and we congratulate EPP on this critical step as well.

The subpanel notes the importance of having Saul Gonzalez back from his OSTP assignment, so that EPP now has a permanent NSF staff member back on the program management team. While he was away, Mark Coles was able to provide some continuity, but the subpanel notes that IPAs Jim Shank and Brian Meadows have left since the last COV. The addition of IPA Randall Ruchti has been a tremendous benefit, given his knowledge and interactivity, and we hope that Ruchti's assignment can be extended to the extent possible since continuity is important. The panel notes the importance also of documenting reasoning behind funding decisions and program strategies, as well as responses to COV discussions and recommendations, to help maintain institutional memory, needed due to periodic IPA turnover.

2a. Responsiveness to Emerging Research Opportunities

The subpanel finds that the program directors actively seek opportunities to fund emerging programs, for example EDMs, which are inter-division/agency ones, and can be promoted to NSF priority programs such as the "Windows to the Universe" Big Idea program.

EPP has also had a strong role in emerging detector technology and electronics advances associated with the LHC and neutrino programs. As an example, EPP has provided important support for the development of liquid argon TPC detectors for neutrino experiments, from the initiation of the ArgoNeuT experiment to the current DUNE detector program.

2b. Responsiveness to Emerging Educational Opportunities

NSF has provided strong support for research opportunities for undergraduate students that give them unique training and exposure to the methods and technology of doing science. The NSF Research Experience for Undergraduates (REU), Research in Undergraduate Institutions (RUI) and Research Opportunity Awards (ROA) programs all provide funding to support these types of activities. In EPP, a large fraction of the funded groups have either direct or associated activities with these programs.

3. Responsiveness to Priorities

The EPP subpanel finds the EPP program to be very well managed, and the priorities have remained in line with P5 report priorities within the EPP domain: HL--LHC upgrades and U.S.--based long--baseline neutrino effort (DUNE). For example, following P5, the MPS Advisory Committee Subpanel report[3] strongly recommended (p.5) that the MREFC process would be needed to allow NSF to play significant and visible roles in the HL--LHC. This is underway currently as has been discussed. NSF PI groups are also currently playing leading roles in detector development and other new initiatives for the high--luminosity upgrade of the LHC. In addition, the MPSAC report (p.6) recommended that after the LBNF was created, NSF should evaluate how it might participate in the long--baseline neutrino program. EPP has responded appropriately with the collaborative grant supporting DUNE R&D, which has had very successful outcomes.

We note that the MPSAC findings state (p.4) that the health of the field depends on the strength of the base research program (PI--driven research awards), and that in participating in priority projects, *"Careful consideration must be given to proceeding with these projects to ensure that they do not create an unacceptable negative impact on PI--driven research awards."* It was suggested that a reasonable total investment in R&D for future projects and for ongoing operations at facilities remain about one third of the EPP budget, distributed amongst projects of different scales. The EPP subpanel recommends that this figure of merit continue to be monitored in order to maintain the well--being of the important base research program.

4. EPP Responses to the 2015 COV Comments and Recommendations

2015 Recommendation 1) *This committee recognizes the importance of the extensive P5 process in setting the directions for our field, and encourages the alignment of EPP priorities consistent with these recommendations, while being open to innovation.*

EPP Response: The general NSF response to the P5 report has been excellent, as detailed in section 3 above. The MPSAC report (2015) strongly recommended (p. 9) that the MREFC process would allow NSF to play significant and visible role in the HL--LHC, which was the highest priority. The MREFC process is underway currently. NSF groups are currently also playing leading roles in detector development and other new initiatives for the high--luminosity upgrade of the LHC. In addition, DUNE was the second highest P5 priority, and the program planning during this review period included critical collaborative grant funding to facilitate R&D for DUNE construction, in addition to PI support in the form of Institutional and CAREER grants.

2015 Recommendation 2) *The COV recommends that the coordination with the DOE in the planning and execution of major projects be continued in the future.*

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[3] *"Implementing the Particle Physics Project Prioritization Panel (P5) Recommendations, Report of the MPSAC P5 Subcommittee", led by Y.K. Kim, January 201*5

Response: Coordination by EPP with DOE (and with other NSF areas) appears to be active. For the LHC upgrades, DOE and NSF are taking on complementary projects that fit within the achievable funds. Mark Coles is overseeing coordination with DOE for the LHC Operations and for the HL-LHC upgrades. Another example of coordination with DOE is the approval of the NSF collaborative grant among DOE and NSF physicists to do the design and engineering for constructing the DUNE anode plane assemblies (APAs). The result from this grant was critical first step for NSF's involvement in aspects of DUNE construction.

2015 Recommendation 3) *The COV encourages the EPP to maintain flexibility to allocate resources where needed to support the computing needs of the investigators.*

Response: The scope and context of the recommendation itself was not clear. Usually small items of computing equipment are either covered by university faculty development funds or they are entered in base grant proposals (laptops, small Tier 3s) . Larger computing resources are part of operations grants (e.g. larger Tier 3s and Tier 2s for LHC computing operations, or equivalent analysis centers). In general, computing needs seem to be supported. Of note, for FY2019 and future, there is also the IRIS initiative.

2015 Recommendation 4) *The COV recommends that in the ad hoc review the evaluation of the scientific merit and broader impact is kept separate from any summary of the proposal, which could be added in a separate, optional section. The COV also recommends an appropriate balance between the weight given to the intellectual merits and broader impact criteria be maintained in evaluating proposals.*

Response: It appears in the jackets that this has been implemented and the summary section in most cases is kept distinct from the questions of Intellectual Merit and Broader Impact.

2015 Recommendation 5) *The COV recommends site visits to be resumed when necessary, particularly for larger grants with multiple PI's, despite the budget constraints.*

Response: (*Covered also in section I.1)* Site visits have not been implemented as a result of the 2015 COV recommendations. This 2019 EPP subpanel is divided on the question as to whether or not this is important. Reasoning that they aren't necessary: site reviews do not provide comparative review of all proposals, smaller groups that do not receive a site review might be at a disadvantage. Also, with groups now so distributed geographically and with much of the work being done at multiple sites, the work at the home institution is just one component of a given program. Reasoning that they can be an important tool: There is value in having program Directors visit sites periodically (~6-7 years) to get a sense of facilities and individual PI efforts, as well as to meet new PIs, but care should be taken not to visit/favor only larger, well-established groups.

To be noted that also the following suggestion from the 2015 COV report was implemented:

1) *"The program directors might consider requiring additional supplemental documentation where PIs self--report their efforts in order to aid in proposal review."* The program directors added instructions for additional information for all proposals, that addresses this case. From nsf 18-564, the relevant language is:

   "For PIs who have or anticipate having concurrent sources of support during the performance of the proposed activities (including but not limited to grants from other agencies or private foundations, and/or laboratory appointments), proposals should clearly explain the relationship between the proposed activities and other funded or pending activities. The proposal should articulate the nature of commitments (such as deliverables, specific projects) associated with other sources of support. This information should be presented in the Current/Pending Support section. [Note that the FastLane web interface for Current/Pending Support is not adequate for providing this information. A separate Current/Pending Support file upload will be needed.] The proposal review process will include an assessment of the proposers' ability to carry out the proposed research in light of these commitments. PIs who have applied to more than one agency with very similar proposals will be expected to withdraw all other applications should one of these proposals be funded."

   We comment that clarity of instructions and feedback is very important, from/to the PIs, in cases of concurrent sources of support.

IV. Program Portfolio

Program Details

The EPP and LHC programs successfully work with other programs within NSF and the DOE Office of High Energy Physics to sustain research areas in particle physics as well as to prepare and align the program with the recommendations of the Particle Physics Project Prioritization Panel (P5)[4]. The world-wide nature of particle physics research is exemplified by the current distribution of EPP-supported groups (12 ATLAS, 15 CMS, 4 LHCb) working in energy forefront research at the LHC, and 13 groups in the area of neutrino physics. In terms of PIs, the program components most recently are: 78% LHC, 15% Neutrino and 7% Other; and by Funding Distribution: 75% LHC, 20% Neutrino and 5% Other. In 2018, the EPP program had a budget of $20.5M to support the university program. In 2018, $14.79M were budgeted for Detectors and Development funding and $16M for Operations.

[4] *"Building for Discovery: Strategic Plan for U.S. Particle Physics in the Global Context", a report of the P5 Subpanel of the High Energy Physics Advisory Panel, led by S. Ritz, May 2014.*

Program goals from the 2018 review:

The primary emphasis of EPP is to support university-based research with base program grants, MRI and cooperative agreements, and Midscale and MREFC processes. There are four components to this currently, aligned with the P5 science drivers:

- Supporting the research of the LHC experiments, including ATLAS, CMS and LHCb, as well as providing support for the scientific research personnel for the ATLAS and CMS Phase I and high-luminosity upgrades, and the LHCb upgrade.
- Support for Neutrino experiments at Fermilab (including Short Baseline and NUMI Programs) as well as new developments for DUNE (through support for ProtoDUNE and DUNE LAr Instrumentation Developments).
- Support for flavor physics, through experimentation at Belle-II, and CERN/NA62.
- Support for precision measurements co-funded in collaboration with AMO and Nuclear Physics programs.

EPP gives high priority to LHC operations, LS2 and Run 3, and the HL-LHC. Operations Support and M&O-A and M&O-B support for ATLAS and CMS are provided through Cooperative Agreements managed by Stonybrook University for ATLAS, and by Princeton University for CMS. For LHCb such support has been provided through EPP base grants to the LHCb groups, but this is planned to change in 2019 to match more closely what has been done in ATLAS and CMS

NSF support of the operations of the LHC experiments (ATLAS, CMS, LHCb) and the Phase I upgrades have been very successful. The panel notes that the MREFC process for supporting high-luminosity upgrades appears to be well implemented.

The EPP program's support for neutrino experiments centers around strengthening the Main Injector-based programs and short-baseline programs at Fermilab (MINERvA, LArIAT, MicroBooNE, NOvA, and SBND), and initiating involvement in the DUNE experimental program through leadership in LAr detector technology R&D for ProtoDUNE and DUNE. Additionally, EPP provides support for neutrino physics studies at CERN (ProtoDUNE) and at T2K (NuPRISM).

Example Implementations of Program Goals: Neutrino Physics

Over the 2015 to 2018 time period, EPP awards provided important support for the MicroBooNE experiment, which is one of the first operating, large scale liquid argon time projection chamber (LArTPC) neutrino detector. The MicroBooNE experiment has been taking data since 2015 and is expecting first results addressing the MiniBooNE low energy event excess at the end of 2019. In 2013, a near LArTPC detector experiment, SBND, was proposed to augment MicroBooNE by directly measuring backgrounds and constraining uncertainties associated with neutrino flux and cross sections. The SBND experiment was approved by Fermilab and, in 2014, NSF provided a $560k MRI award to construct the anode plane assemblies (APAs) for SBND. This MRI award, along with two CAREER grants partially associated with SBND, has allowed several young faculty members to take leadership roles in the experiment, with one being elected as co-spokesperson for the SBND experiment and the other developing a continuous readout for SBND that can be applied to DUNE. Beyond these MRI and CAREER awards, EPP has provided base grant support for SBND collaborators including an equipment grant to pay for the LArTPC readout electronics. These highly leveraged funds have allowed NSF supported physicists to fully participate in SBND, which will be able to provide data and physics analyses for their postdocs and students who are also working on the longer term DUNE experiment.

The next step in the US on-shore neutrino program will be the DUNE long-baseline oscillation program. This program will use four 10 kton LArTPC detectors located in the Homestake mine in South Dakota to detect neutrinos from the Fermilab neutrino beam, which starts 1300 km away. DUNE will make precision measurements of the oscillation parameters associated with neutrino mixing including making a definitive search for CP violation in the neutrino sector. CP violation may be a key ingredient for explaining the matter to antimatter asymmetry in the universe. In addition, the large DUNE detectors will have improved sensitivity for supernova detection and baryon number violating processes such as proton decay and n-nbar oscillations.

The NSF EPP program is providing very important funding support for NSF university physicists to take active and leadership roles in the DUNE experiment including one being the overall experiment spokesperson. NSF physicists are also playing major roles in the ProtoDUNE program at CERN, which is an important test of a DUNE detector prototype using actual test beam particles. As stated previously, a NSF collaborative design grant, which included PIs with base grants from both DOE and NSF, provided important support for completing the engineering and prototyping of the APAs for the DUNE detectors. Results from this grant could be a critical first step for constructing the APA planes at NSF-supported U.S. universities. In addition, EPP is supporting work on the design and construction of triggering and data selection through base grant and CAREER award funding. All of this funding support for DUNE activities is making it possible for NSF supported universities to take important roles in the DUNE design and construction by partnering with DOE in realizing the DUNE experiment and its physics program.

Example Implementations of Program Goals: LHC[5]

The 2013 Community Plan endorsed the HL-LHC upgrade as the highest priority large near-term initiative, and the following 2014 P5 report likewise identified the HL-LHC upgrades as the highest priority project for the particle physics community. Since then, periodic community statements[6] continue to emphasize the high priority of the HL-LHC upgrades. The HL-LHC will produce data at nearly an order of magnitude higher rate than the LHC. The substantial increase in luminosity will pose major technical challenges for the ATLAS and CMS detectors. The NSF MPSAC thus

[5] Source: "**High Luminosity Upgrades to the ATLAS and CMS Detectors at the Large Hadron Collider ",** PHY EPP/LHC Program Office

[6] See e.g. the 2018 joint statement from the APS Division of Particles and Fields, the Fermilab Users' Executive Committee, and the U.S. LHC Users Association: https://www.usparticlephysics.org/wp-content/uploads/2018/02/US-Particle-Physics-Strategy-2018.pdf

recommended that the NSF respond by participating in the upgrades at the MREFC level to enable meaningful participation by NSF PIs in building the future HL-LHC research program.

In 2015, NSF signed a Cooperation Agreement with CERN which defined a framework by which scientists sponsored by either entity can participate in projects of mutual interest. That same year, the NSF Director authorized the Conceptual Design of the ATLAS and CMS upgraded detectors, and NSF implemented an "Experiments Protocol II" with DOE and CERN, indicating the intent to participate in the construction of technologically advanced detector upgrades and to facilitate the continued participation of the U.S. community in the HL-LHC program. In mid 2016 the CERN Council approved the HL-LHC project.

The NSF's MREFC scope was based on the information provided by ATLAS and CMS to the PHY division on the broad technical requirements that would be needed to enable them to achieve their science goals. The specific components that each detector collaboration proposed for MREFC funding were selected carefully to leverage extensive experience, expertise, and interest of NSF-supported institutions and physicists. The components were also selected from those that could be uniquely identified as NSF contributions, and that could be accommodated on the appropriate timescale for both MREFC funding, and for the technical needs of the international upgrade program at CERN.

ATLAS and CMS successfully completed a CDR in spring 2016, and with PHY division's plan for continued oversight, the NSF Director authorized design activities in fall 2016 with an MREFC budget not to exceed $150M. This resulted in the CMS and ATLAS PDR's in late 2017 and early 2018. Evaluated in the areas of Management, Cost/Schedule/Risk, and Technical, the review panel concluded that each upgrade can be confidently executed within the $75M budget cap (per experiment), and that the schedules are adequate, teams are highly experienced, and risks are well-understood. In April 2018, the Facilities Readiness Panel (FRP) confirmed that the Preliminary Design phase activities were accomplished successfully, and that the upgrades were sufficiently developed to proceed to the Final Design Phase. The MPS Advisory Committee met and endorsed the continued advancement of the project in April 2018 as well. This story represents a major success for the NSF EPP program.

1. Portfolio Balance

Over the FY15 to FY18 time period, there were a total of 237 grant proposals to EPP programs with 77 funded (32.5%). From our inspection of the proposal jackets, this funding percentage seemed appropriate given the constraints on the total EPP funding allocations. The average duration of the grants were 3.1 years with a median (average) award size of $174K ($320K). (The average award size is distorted by a few large awards and by some grants receiving all funding in the first year.) There is a reasonable spread of support among the EPP program components with the PI count distribution being: 44% CMS, 26% ATLAS, 8% LHCb, 15% Neutrino and 7% Other and with the funding level distribution being: 40% CMS, 25% ATLAS, 10% LHCb, 20%

Neutrino and 5% Other. These distributions were assessed to be in line with the priorities in the field and strengths of the supported PIs and groups.

2. Award Duration and Size

The duration of the typical award is currently three years, although it can be as high as five years. For EPP, three years seems to be the right time scale for evaluating and carrying out proposed projects without having to extrapolate too far into the future as to what directions should be taken and what personnel are needed. In addition, this allows the evaluation of changes in ideas and progress in the field, as well as the evaluation of funding trends. We recommend that this cycle length be maintained on average, but with continued flexibility to grant shorter or longer term awards as deemed appropriate.

Given the overall funding levels, the size of an award per PI is often not appropriate to cover the scope of the project. It is difficult to fund all salaries typical of a group/PI (which should include summer salary, one postdoc, 1‐2 graduate students, and some travel). There are many variations on the levels, however. We note that the median and average of the funding‐per‐PI levels can be quite different, although we weren't able to assess the reasons for this.

Particular care should be given to cases of concurrent sources of support to make sure PIs can carry out their proposed research plan. Clarity of instructions and feedback from the review process is important.

3‐‐4. Support for innovative and Interdisciplinary or Multidisciplinary projects

EPP awards have supported research on novel detector technology, on cutting‐edge data analysis techniques, on precision measurements that will define our knowledge for the next generation, and on discovery science that has the potential to change our understanding of particles and forces that make up our universe.

Joint projects with DOE include LHC operations and upgrades as well as R&D and construction projects associated with the DUNE program. Joint projects with Nuclear Physics include some areas of neutrino physics, and heavy ion physics through LHC support.

5‐‐6. Portfolio Geographic and Institutional Balance

EPP has a distribution of support from the east to the west coast of the United States. Areas which could use outreach include the southern states, particular southeastern states, and the middle states.

In the 2018 EPP report, the NSF made a careful analysis of the geographical distribution of reviewers and grants awarded. Since the geographical distribution was less balanced than the demographic distribution, it was suggested that EPP encourage the development of a programmatic strategy to foster cooperation amongst smaller groups, which are more isolated regionally, in ways that lead to more effective contributions. *(See also section I.4)*

The EPP program supports the efforts of RUIs (institutions with only undergraduate programs), and young investigators proposals (either to the base program or to the CAREER). The EPP program conscientiously supports diversity at all stages of the review process.

7. Early Career Support

Support for first-time junior PIs has been an important part of the NSF portfolio. In spite of many challenges in the EPP funding profiles, the program Directors have supported funding for early career investigators. EPP has success rates sometimes as low as 25% for proposals, and turns away excellent, fund-if-possible proposals due to the tight funding envelopes, but the Program Directors have tried to support young PIs on single PI grants or multi-PI institutional grant awards, and not rely only on CAREER grants, which have a very low success rate. (Typically ~70-80 CAREER proposals are received across the PHY division in a given year, and of these ~ 5-10 are EPP related. Typically one EPP CAREER award is made per year, with a total of 9 awards active in the period reviewed.)

During the FY15 --- FY18 period, about 25% of the funded proposals in PHY went to first-time junior faculty. For the EPP program, this was also supplemented by four CAREER grant awards two associated with the LHC and two associated with Neutrinos.

2019 EPP COV Subpanel Recommendations

1) The subpanel recommends that the ad hoc reviews give sufficient detail so that the proponents can understand the basis for the rating they are given. The perceived strengths and deficiencies of the proposed program should be clearly stated by the reviewers so that PIs receive important feedback and so that the process is as transparent as possible.

2) The subpanel recommends that the EPP panel review reports are detailed enough to contain the elements that led to the final recommendation on the funding support and proposal ranking. From our readings, most proposals received sufficient feedback but we found some proposals with clear deficiencies.

3) The subpanel recommends that the final information transmitted from the program directors to the PI's (e.g. regarding re-budgeting and individual PI feedback) is made available to **<u>all</u>** PI's on a multi-PI or multi-institution grant, so that all proponents are given feedback that allows them to react and optimize any future submissions.

4) The subpanel recommends that for large experiments such as the LHC, EPP should obtain ad hoc reviews from peers within the same experiment who don't have a

conflict of interest. (This already occurs for the panels.) Such reviewers would be able to give better insight on the impact and importance of the current or proposed program as well as on the PI's contributions. These details can provide important input to the panel deliberations, but may not be available from a reviewer outside of the experiment.

5) There was no written EPP response to the previous 2015 COV, even while most recommendations were addressed. It is recommended that EPP prepare a written response for this 2019 COV report that can be reviewed by the next COV as to what was planned and then implemented. This might also help with explaining the context of the requests and responses.

D. Gravitational Physics / LIGO

Successes

- A broad yet balanced portfolio of theoretical and experimental research on gravity, all "under one roof."
- LIGO did it! Growth in gravitational wave field is being managed well.
- An initiative to improve measurements of Newton's constant *G* stimulates the community of table-top experimenters.
- The PD is being an active steward of the GW community's planning for a next generation of gravitational wave detectors.

Recommendations relevant to the whole Gravitational Physics program

- Continue the healthy union of gravitational experiment and theory in one program.
- Maintain the good balance between LIGO and non-LIGO research.
- Keep finding co-funding (Federal and private), and find other ways to expand the gravity program.
- Use some new funds to support postdocs.

Recommendations relevant to gravitational wave research

- Ensure that the mechanisms for sharing of LIGO data are working well.
- Continue stewardship of 3rd generation gravitational wave detectors.
- Review the LSC, and give it guidance in how to best serve the scientific community in the post-discovery era.
- At an appropriate time, consider calling together a "blue ribbon panel" to advise on the best organizational structure for the future of multi-messenger astronomy.

Narrative

Admirable features of the Gravitational Physics program

This program is well managed. Orchestrating the reviews and panels and responding to the panel and reviewer recommendations have all been done commendably. **We commend the PD for finding a legal mechanism to include members of the LIGO Scientific Collaboration in reviews of LIGO related proposals. This involved clarification of CoI rules that had in prior years prevented the assignment of reviewers with appropriate expertise.** Also, the PD sought expert reviewer input from far and wide including astronomy/astrophysics, math, and non-gravitational theory to comment on proposals with overlap in those areas to complement the reviews from the gravity reviewers. The analysis provided by the PD appropriately considered the comments from the panel summary and individual reviews, and the award/decline decision was made in a reasonable fashion and tailored to the particular recommendations of the reviewers. The gravity program and PD should be commended for being flexible, thoughtful, and creative in his responses to panel summaries and reviewer comments. We noted with approval his decisions to provide one year of funding for projects where further study was needed or where there were split reviews, subject to renewal. We also approved of cases of his funding only a subset of activities, based on reviewer feedback.

The Gravitational Physics program has paid careful attention to proposals from new investigators. In the four most recent FYs, 24 new young investigators were funded, out of 38 proposals submitted -- a laudable success rate. The program supports inter- and multi-disciplinary projects such as multi-messenger gravitational astronomy, materials science for gravitational-wave detector mirror coatings research, computer science and citizen science. Cooperation and co-funding with AMO is strong in gravitational experiment. The interdisciplinary nature of the program is borne out in co-funding by AMOP, PIF, OMA, OAC, CISE, OIA and AST for gravity awards.

The awards have been adjusted in size given constraints on the overall budget and for the most part are appropriate to allow progress to occur for the funded projects. However many of the awards do not allow enough support for postdoctoral researchers where such support has been requested. There is a balance between the number of awards and the size of awards, and to allow meritorious awards from a larger subset of the community to be funded, often fewer postdocs are able to be supported by the projects. The PD has maintained a reasonable balance between award size and success rate. Postdoctoral researchers are a crucial component for the success of many of the projects proposed within the portfolio, and as this field is rapidly growing in the wake of the gravitational wave discoveries, the development of future researchers in the field depends on having methods of strong postdoctoral training. For example the NANOGRAV program has recently produced five new faculty members from its former postdocs.. This field is ripe for growth in its overall funding level; we feel that a substantial portion of any new funds be devoted to increasing the size of individual grants, to allow more grants to support postdoctoral researchers.

Regarding the balance of awards across disciplines and sub-disciplines, for the past 4-year period, the portfolio has had an average expenditure of $15.56M/yr, including theoretical gravitational physics $5.02M/yr (32%), gravitational physics experiment $1.73M/yr (11%), and LIGO research support $8.81M/yr (57%). This is separate from the expenditures on operations and maintenance and the LIGO A+ upgrade, which are not considered part of the same budget, averaging $40.3M/yr. The support for the LIGO-related research has steadily increased in the past 4 years, starting at $6.9M in 2015 and ending at $10.8M in 2018. Support for Gravitational experiment and theory has remained steadier: for theory from $5.59M in 2015 to $5.62M in 2018, and for experiment from $1.73M in 2015 to $1.72M in 2018.

As the gravitational physics program at NSF is the principal funding source for research in gravitational theory and experimental gravitation in the U.S., it is crucial to maintain a reasonable balance between theory and experimental funding for both LIGO-related and non-LIGO related research. LIGO has had tremendous recent success in the detection of gravitational waves, and continuing support for LIGO-related research and next generation detectors is crucial for the portfolio. In keeping with the NSF’s mission of fostering transformative high-impact high-risk research, it is equally important that research and development for new techniques for gravitational wave detection as well as other experimental gravity tests; we are happy to note that tests of Einstein’s equivalence principle, testing the gravitational inverse square law at short-distance, and improving the measurement of the Newton’s constant *G* are included as a balanced part of the portfolio. The efforts of the PD in this regard have been commendable and this should be maintained in the future. Another excellent aspect of the portfolio its tradition of integration of the theory program with the experimental program; this integration should be perpetuated.

In sum, the Gravitational Physics program has experienced dramatic growth over the past several decades, but it has stayed true to several admirable organizing principles:

Theoretical and experimental physics are supported together “under one roof” and are under the guidance of a highly respected gravitational physicist.

Even while growing LIGO “from scratch” and nurturing it as it grew to become a project with a large budget surrounded by a large collaboration, the Gravitational Physics program has, in parallel, nurtured high quality research on theoretical and experimental aspects of gravitation done in the more traditional single-investigator style.

The result has been a varied but tremendously successful portfolio of research. **We recommend that the Gravitational Physics program continue to be a single home for both theoretical and experimental research on gravitation,** and also **we recommend that the Gravitational Physics program continue to maintain a balance between research on gravitational waves and research on other aspects of gravitational physics.**

Celebrating and building on LIGO’s success

We would be remiss if we did not take this opportunity to recognize, with profound gratitude, the way NSF steadfastly supported the Laser Interferometer Gravitational-wave Observatory over many decades, from its tentative beginnings until it had truly opened a new window on the universe, ushering in a new era of multi-messenger astronomy. There were all too many ways that this effort could have gone wrong. That it went right instead is truly a cause for celebration.

Now, we face the happy challenges of success. The gravitational wave research effort has grown dramatically. As NSF manages the growth of the current gravitational wave effort (see recommendation above), it also is looking forward to a next generation (so-called 3G) of gravitational wave detector(s). Specific features of that effort are discussed below.

Leadership across the range of gravitational physics

The Gravitational Physics program has wisely kept its attention directed equally on the non-gravitational wave aspects of gravitational physics. This is an essential part of its mission. For example, this program is the only source of Federal support for experimental gravitational physics, and it is also the only source of support for exploration of approaches to quantum gravity other than string theory (for example, loop quantum gravity.)

One recent example of intellectual leadership is the convening of an Ideas Lab to promote new approaches to the measurement of Newton’s constant, *G*. This effort holds the promise not only of dramatic improvement in the precision of our knowledge of *G*, but also of stimulating growth of new measurement techniques of much broader applicability.

Responding to challenging budgetary climate

The past four years have seen effectively flat budgets, even as the success of the Gravitational Physics portfolio has led to strong growth in the community. The Program Director has responded with care and creativity to this challenge. His creativity has been manifest in his ability to find sources of co-funding from a wide spectrum of sources (including other programs in PHY, the Physics Division itself, other parts of NSF, other agencies, and even a private foundation.)

Even if the overall budget situation were to improve, the pressure on Gravitational Physics program resources is sure to continue; the growth of interest in gravitational waves is enough to ensure this. Thus, **there is no substitute for growth in funds available to the Gravitational Physics program. Creatively combining regular budget, Federal co-funding, and private foundation support is essential.**

While growing the program overall, it is important that some of any new funds be used to redress the shrinkage of individual grants that has occurred over the years. In particular, it has become hard even for some very strong grant recipients to hire postdocs. **We urge that increasing the number of grantees able to hire postdocs be made one of the priorities for new funding.**

The NSF's stewardship of next-generation gravitational-wave interferometers

With the dramatic successes of LIGO and its international partners the gravitational wave window on the universe has been firmly opened and gravitational wave and electromagnetic multi-messenger astronomy has been demonstrated to be incredibly rich. The LIGO detectors have received PHY midscale support for an upgrade of the facilities to Advanced LIGO+ (A+), which will deepen our view of the universe by increasing the detection rate to better than once per day.

Currently, plans are being developed for Cosmic Explorer, the next generation of US-led ground based laser interferometers with ten times the baseline (40 km) and scaled up technology. Cosmic Explorer promises ten times the sensitivity of A+, enabling humanity to observe all stellar-mass black hole and neutron star mergers in the universe, and reaching back to cosmological periods before the first stars formed. While the scientific payoff of a next generation of observatories is clear, the path to their funding in the US is less clear. Here the gravitational wave community can benefit greatly (and already has) from stewardship by the NSF.

The Gravitational Physics Program Director, Pedro Marronetti, has been proactive in next-generation stewardship, including supporting the field to bring together both the gravitational-wave community and the international funding community through an international series of so-called DAWN meetings, of which there have been 5 annual meetings so far. Marronetti has also supported the formation of the Gravitational Wave Agencies Correspondents (GWAC) with representatives from international funding agencies with interest in future gravitational wave observatories. The DAWN meetings have nurtured the growth of both the US and international next-generation detector and science plans, including community reports and whitepapers; GWAC's interactions with the gravitational-wave community are providing much needed feedback toward developing realistic funding and scientific plans.

At DAWN V, Marronetti presented a clear timeline for how a large project, such as Cosmic Explorer, could go from its first NSF-funded planning study to eventual support via an MREFC. The NSF-funded Cosmic Explorer team and the LIGO lab are working together toward the delivery of the next step of this path: a horizon planning whitepaper. .

We wish to acknowledge the central importance of this; the NSF is playing a crucial role in the guidance of third generation gravitational wave science. Through this work the Physics Division can ensure that the United States remains at the forefront of the era of gravitational-wave discovery. Thus, although it may seem superfluous in light of the Program Director's personal leadership, **we recommend that the NSF's active stewardship of the 3G detector effort remain a high priority.**

As the field moves forward with planning for a hoped-for 3G detector, at some point it will prove necessary to ask the same kinds of questions for the 3G era as was done in the mid-1990's for the LIGO era. How should the core team be organized, what is the scope of work that the core team will carry out, and how will people outside the core team be able to make contributions? Lessons from LIGO will be instructive, but surely not prescriptive. A meta-lesson from the LIGO era might be that it is better if these questions are asked early, and asked of a wide community. We note with pleasure that questions like this are already being discussed at, for example, the DAWN meetings. Nevertheless, NSF might do well to repeat this much of LIGO history – **at the appropriate time, call together a "blue ribbon panel" to advise on the best form of organizational structure for this new era in multi-messenger astronomy.**

LIGO Scientific Collaboration

The success of LIGO depended on many things. NSF's vision and steadfastness have been essential factors. The insight, skill, and determination of the Nobel Prize-winning leaders was also key, of course. They built a tremendous team of scientists, engineers, and others at what became known as the LIGO Laboratory at Caltech and MIT.

Beyond these obvious leaders was a large group that also played an essential role in LIGO's success – the dozens of groups of scientists and students at universities around the country (as well as across the world), who organized their contributions to LIGO through the LIGO Scientific Collaboration (LSC.)

NSF had begun funding independent scientists doing LIGO-related research in the early 1990's. But by the middle of that decade it had realized that there needed to be a stronger way to integrate that work with the work of the core team at Caltech and MIT. Only with good coordination would the best possible science be done, with the broadest range of ideas contributed and the widest possible distribution of the benefits of the taxpayers' massive investment.

In 1996, NSF convened a "blue-ribbon panel" chaired by Boyce McDaniel to find the solution to the future use of LIGO. The outcome was twofold:

- re-organization of the team at Caltech and MIT as the LIGO Laboratory, with the responsibility to construct, commission, and operate the LIGO detectors, and
- organization of the group of both "inside" and "outside" scientists as the LIGO Scientific Collaboration, whose role was to "carry out the scientific program of LIGO."

In spite of the well-known difficulty of "herding cats", the LSC succeeded spectacularly well since its organizational meeting in 1997. The LSC was responsible for organizing and carrying out the analysis of LIGO's huge and precious dataset, resulting in the discovery of the black hole binary GW150914, the neutron star binary GW170817, and more examples of them that are now being discovered at an increasing rate as the LIGO interferometers are improved and run for longer periods.

The LSC was also responsible for organizing the research and the "visioning exercise" that led to the design of Advanced LIGO, the instrument that finally succeeded in discovering gravitational waves. Of course the LIGO Laboratory brought the idea of Advanced LIGO to fulfillment, but the role of the broader LSC must not be neglected.

NSF not only sponsored the creation of the LSC, but of course it has nurtured it as well, through a growing program of LIGO Research Support that today totals about $11M /yr. And yet there has always been something anomalous in the relationship. While NSF's Cooperative Agreement with the LIGO Laboratory strictly governs that essential relationship, NSF has no formal connection to the LSC at all. Instead, it has the usual kind of relationship with each individual NSF grantee among the members of the LSC.

What this means is that, while the NSF knows precisely how to review the LIGO Laboratory, it has (until quite recently) had no mechanism for reviewing the work of the LIGO Scientific Collaboration as a whole. That has been a problem, since the LSC has carried out functions essential for the success of LIGO, like analyzing its data and thus finding in that data the signals that constituted the discovery of gravitational waves.

Thus, we are tremendously heartened by the NSF's success in finding a mechanism by which it can interact with the LSC as a whole, and thus give it the kind of oversight that it deserves. NSF has approved the funding of a grant to the LSC that pays for the LIGO Fellows program (among other expenses of the LSC.) In its administration of this grant, NSF will be able to conduct reviews of the LSC and give it guidance. We commend this action, and **we recommend that the NSF use its new ability to review the LSC explicitly as a way to review and guide the organization.**

LIGO Open Data

Of course, a lot has happened since 1997. Not least has been a dramatic change in the model of data "ownership" that governs LIGO data. As originally organized, LIGO's data was proprietary to the LIGO Laboratory. In its Memoranda of Understanding with the member groups of the LSC, the Lab shared the right to analyze that data with the LSC. For a number of reasons that were eminently reasonable at the time, that arrangement seemed a good one; and in fact it was the arrangement under which the discovery work was carried out.

But in the meantime, the national conversation led to a new consensus that proprietary data from NSF-funded projects was a bad idea. In 2010, LIGO and NSF completed a new Data Management Plan that laid out a gradual transition from the original proprietary-data model to one where, on a negotiated time scale, LIGO data was shared with any interested scientist. By now, LIGO is in what is called the “open data era,” where each section of newly-collected data is analyzed privately by the LSC for a restricted period of time, after which it is shared freely.

LIGO has been able to move rapidly into the open data era, precisely because of the tremendous success of the efforts of the Lab and the LSC. Beginning with the first binary black hole discoveries, and burgeoning with the discovery of the first neutron star binary, the field of “multi-messenger astronomy” has exploded. Several “outside” groups are doing excellent science with released LIGO data.

This makes it timely for NSF to ask how well the mechanisms for the sharing of LIGO data are working. The Gravitational Wave Open Science Center (GWOSC) is the organization that prepares the data for sharing and that serves the broader community that uses it. Is GWOSC succeeded at its mission? The question can be asked both from the point of view of “outside users” as well as from “insiders.” Does GWOSC do a good job with the resources it has? Could it do a better job with additional resources, for example?

We were very encouraged to learn that the Gravitational Physics Program has been asking precisely these questions. **We recommend that the NSF help LIGO and the wider community to find the best ways to share LIGO data and use it to do high quality science; if more resources are required for this essential science-enabling activity to be done properly, we hope that NSF will find a way to provide them.**

Responsiveness to previous COV recommendations: Among the gravitational physics recommendations from the last round were, “we strongly encourage vigilance in maintaining thematic balance” [given a diverse theoretical and experimental program and the presence of major projects such as LIGO and NanoGrav] and “It would be good if the CAREER program allowed more flexibility to researchers to tailor their proposals to their talents allowing varying levels of commitment to outreach among the successful proposals.” The PD has indeed been vigilant about maintaining thematic balance. The issue concerning the CAREER program is an NSF-level question that was discussed by the full PHY COV at the 2019 meeting.

E. Integrative Activities in Physics

Introduction

The Integrative Activities in Physics (IAP) program plays a unique and critical role in enhancing the breadth – and in particular, the broader impacts – of the Division of Physics (PHY) as a whole. Historically, IAP has served as an incubator for new research activities that do not fit neatly into existing PHY programs (e.g., during the emergence of biophysics as a new sub-discipline), and it has the potential to fill this role again in the future. In FY15-18, IAP primarily supported projects that spanned existing sub-disciplines and focused on different aspects of research training, education, outreach, and/or broadening participation – all crucial aspects of ensuring the future vitality of physics and its contributions to the national science and technology enterprise.

The largest budgetary slice of the IAP pie belongs to the Research Experiences for Undergraduates (REU) sites, of which ~50 are (co-)funded by PHY in any given year. Non-REU proposals have been regularly funded in physics education research (if relevant to advanced undergraduate or graduate education), facilities-related outreach (including teacher development) efforts, and programming designed to broaden the participation of underrepresented groups in physics. (The latter effort is supplemented by IAP's management of dedicated PHY funding for broadening participation, which is deployed to make investments in individuals at critical early career stages.) A major new development has been IAP's funding of a multi-year proposal to support interdisciplinary research workshops (at the Kavli Institute for Theoretical Physics), resulting in an REU vs. non-REU balance that is more even than in the recent past.

As part of the COV process, a three-member subcommittee examined a selection of 22 "jackets" for proposals reviewed by IAP in the FY15-18 window; these jackets included funded and declined examples of all of the proposal types described above. The subcommittee also examined statistical aspects of funding rates for the IAP portfolio as a whole. The subcommittee's discussions were informed by a detailed program summary and multiple clarifying conversations with the current IAP program Director.

The remainder of this report addresses the four general COV prompts in the specific context of the IAP program. Because the challenges faced in reviewing REU and non-REU proposals are very different, we consider the first two sets of COV questions (concerning "Quality and effectiveness of merit review process" and "Selection of reviewers") for the two categories separately, before addressing the last two sets of COV questions (concerning "Management of the program" and "Portfolio of awards") as framed.

1. Review of REU proposals

Each year's REU proposals are reviewed by a single panel with 10–13 members, which produces panel summaries for all proposals and provides a natural basis for a context statement that can be shared with PIs. The COV subcommittee for IAP was generally impressed by the skill and thoughtfulness with which PHY REU review panels were constructed. FY15-18 panels were nicely balanced in nearly all respects, with good attention paid to demographic diversity, the involvement of faculty from predominantly undergraduate institutions (PUIs) and minority-serving institutions (MSIs), and appropriate expertise and qualifications.

There is, however, one dimension of balance that we see as requiring some adjustment. A major new development since the 2015 COV report has been the growing degree of coordination among the PIs of PHY-funded REU sites, and the establishment of a new "NSF Physics REU Leadership Group" (NPRLG), external to NSF, to strengthen and extend this coordination. Through one face-to-face meeting hosted by the American Physical Society (APS) and the ongoing work of the NPRLG, over the last three years, offer and response timelines have been harmonized across U.S.-based PHY REU sites, information about students accepting offers has been efficiently shared among REU directors, and progress has been made on a common assessment/evaluation instrument scaffolded on previous work by the Center for the Improvement of Mentored Experiences in Research (CIMER). These are very positive changes from the point of view of REU applicants, REU directors, and the Division of Physics, which will ultimately benefit from a stronger evidence base as it considers whether the positive impacts of REU programs on their participants justify an expansion of funding for the IAP program.

With this greater degree of coordination, however, comes the likelihood that new proposers will be at a greater disadvantage relative to incumbents because they will not have equal access to shared knowledge. This situation will be exacerbated by a high fraction of REU (Co-)PIs on the REU review panel, which averaged 73% in FY15-18. There is thus a risk that members of the same exclusive "club" of PIs could end up as judge and jury for out-of-the-loop first-timers, both defining the standards for PHY REU sites and determining whether new proposals meet those standards. To minimize the risk of such clubbiness and reduce barriers to entry, we recommend the following:

- **The IAP program Director should encourage the NPRLG to allow aspiring REU proposers to attend the PHY REU directors' meetings as guests.** This opportunity should be advertised (e.g., on the NPRLG website[7] hosted by the APS) so that it will become known to its target audience. To avoid having the directors' meeting overwhelmed with such guests, it would be reasonable for NSF or the NPRLG to offer travel support only in exceptional cases.
- **The IAP program Director should encourage the NPRLG to identify publicly which REU functions are already being coordinated (and, as appropriate, funded) across sites.** Program evaluation is now firmly in the

[7] https://www.aps.org/programs/education/undergrad/physicsreu/nprlg.cfm

"coordinated" category, and it is possible that recruiting, application platform, applicant/participant tracking, and/or other functions may shift into these categories in the future. Transparent communication here will save new proposers from, e.g., sinking time and money into the development of new assessment tools whose outputs cannot usefully be aggregated on a PHY-‐wide basis.

- **Between 1/3 and 1/2 of each year's REU review panel should *not* be affiliated with current PHY-‐funded REU sites or the NPRLG.** (This fraction should be closer to 1/2 if our first recommendation cannot be followed, i.e., if aspiring REU proposers are barred from PHY REU directors' meetings.) Possible categories of knowledgeable, non-‐incumbent reviewers might include "near miss" proposers from the previous year who did not re-‐propose.

We close this section with two comments inspired by the set of REU jackets we examined. First, it was clear that reviewers were sometimes more optimistic about the quality of the student experience at a particular REU site than they were impressed by the site's proposal per se. A genuine commitment to lowering barriers to new entrants, as discussed above, should enhance competitive pressure and ultimately elevate the quality of funded REU proposals overall. Second, in the case of one year's panel, it appeared that the same general, constructively critical comment was included in nearly all panel summaries – undoubtedly useful feedback for each proposer and (aspiring) site, but by definition not determinative of funding outcome. Such feedback might in the future be folded into the context statement for a given REU panel or contextualized in program Director comments, rather than retained without comment in individual proposals' panel summaries.

2. Review of non-‐REU proposals

The non-‐REU proposals submitted to IAP represent one of the stiffest challenges for the review process across all of the Division of Physics: these proposals vary widely in scale, structure, objective, and balance of intellectual merit vs. broader impacts. While all such FY15-‐18 proposals were subjected to ad hoc review by reviewers with appropriate expertise and qualifications – with similar proposals sometimes reviewed by the same ad hoc reviewers – it is our conviction that the advantages of a panel review encompassing all of the non-‐REU proposal types IAP currently receives (as occurs for similarly heterogeneous sets of proposals in, e.g., the Division of Mathematical Sciences) would outweigh the drawbacks:

- Proposals reviewed by panels can be more reliably tensioned against each other, illuminating strengths and weaknesses that would not have been as obvious to ad hoc reviewers considering only 1-‐2 proposals apiece.
- Panelists learn to become better reviewers as they are exposed to each other's responses to the same proposals. This type of education does not happen with ad hoc reviewing, but our sense (from the uneven level of specificity among reviews of the same proposals) is that it would be beneficial to the IAP reviewer pool.

- Panel reviewers (unlike ad hoc reviewers) are compensated for their efforts and can therefore be expected to undertake more thorough preparation for their work – e.g., training on implicit bias and the interpretation of "broader impacts," as highlighted in the IAP section of the 2015 COV report.
- For IAP at this juncture, the different sets of non-REU proposals being received – most of which focus on education and outreach at some level – would not appear to require an unmanageably large breadth of expertise among panelists.
- IAP is currently supporting several large education/outreach projects with long records of continuous funding but not always clear demonstrations of impact. For such projects, high-quality evaluation is *especially* crucial in allowing IAP to assess whether they are meeting high-level NSF objectives and continuing to deliver value. We believe that panel reviews comparing new and renewal proposals for such large projects against each other (and against other proposals) are more likely to strengthen the quality of project evaluation plans than ad hoc reviews, which can sometimes lapse into generic, "too big to fail" style feedback.

In view of these considerations, we recommend the following:

- **Each year, IAP should convene an integrative, multidisciplinary panel to review all non--REU proposals** (apart from small conference proposals that can be reviewed internally). Ad hoc reviews can be used as needed to provide supplemental breadth of expertise (e.g., for multi-year proposals to support research workshops that span the subdisciplines of physics).
- **IAP should consider adjusting future award lengths for its large, long--standing education/outreach projects so as to bring their renewal years into alignment.** Synchronized renewal cycles would better allow the tensioning of similar proposals against each other, with the goal of strengthening programmatic evaluation.

3. Management of the IAP program

The COV subcommittee for IAP was impressed by the insight and thoughtfulness with which this multi-faceted program is being run. Our assessment is strongly informed by IAP's management of funds for broadening participation (BP) within PHY, which centers on an internal solicitation to other PHY program Directors inviting nominations of "difference-making" investments in individual proposers at critical early career junctures. We strongly endorse the continued use of this approach (as opposed to supporting research proposals because they happen to be outreach-heavy), and offer three supportive recommendations:

- **The language of the internal BP solicitation and a rubric for evaluating the resulting nominations should be formalized.** Our motivation in making this suggestion is to ensure that the current nuanced approach to deploying BP funds is fully documented and continued.

- **Commitments of BP funds should be documented and their impacts evaluated more formally than is being done at present.** Better tracking of these investments will help clarify their strategic value to PHY and make their continued success more assured.
- **For individual investigators whose proposals are supported in part with BP funds, NSF should suppress the BP designation in communications with the PIs and in all public listings.** Making BP support public has the potential to stigmatize its recipients, exposing members of underrepresented groups to "you only got that grant because you're X" comments from colleagues and exacerbating imposter syndrome. These negative effects are completely counter to the goal of broadening participation within physics.

We also endorse IAP's continued commitment to obtaining co-funding, which was favorably noted in the 2015 COV report. One respect in which we see an opportunity for improvement is IAP's role in *stimulating* new interdisciplinary proposals from the physics community, leading to a final recommendation:

- **PHY should convene one or more workshops (or alternatively, events in the "Ideas Lab" format) focused on areas of direct relevance to IAP – e.g., burgeoning interdisciplinary areas of physics, broadening participation in physics, or strategies for educating the next generation of physicists.** These would spark a new set of IAP-targeted proposals and boost the number of newly funded PIs. There could be pressure to broaden such events to the scale of MPS or NSF as a whole; we would advise resisting such pressure, in the interest of not diluting a focus on the discipline-specific challenges physics faces and the discipline-specific tools that might be brought to bear on them.

The subcommittee examined whether funding rates across the entire IAP portfolio correlate with PI race or gender and found that for FY15–18, the mean funding rate for women PIs (48%) was slightly lower than for men (53%). While this comparison comes with substantial year-to-year fluctuations, it is reversed from the pattern for the Division as a whole (per Table 6 of the PHY report to the COV); we therefore encourage IAP to track demographic trends in proposal outcomes going forward. We did not find statistically significant evidence of a difference in funding rate for under-represented minority (URM) PIs relative to other PIs.

4. Portfolio of IAP awards

The IAP award portfolio is traditionally dominated by awards to REU sites. Key questions for any assessment of programmatic balance are therefore (i) how many REU sites PHY should (co-)fund,
and (ii) whether REU award sizes are commensurate with project scope and duration. For (i), anecdotal evidence from program Director site visits suggests that student demand for physics REU programs is high, but applicant tracking is not yet advanced enough to make this statement quantitative. We suggest that IAP continue to discuss with the NPRLG ways to capture information about the REU applicant pool while protecting student privacy. For (ii), although PHY operates within the constraints of

the same REU solicitation 19-582 as all other NSF divisions, we would encourage cross-division discussion of expanding the budget allowed for administrative support before the next solicitation is released. The present "up to one month of salary for the PI, or distributed among the PI and other research mentors" allowance may not be enough to incentivize a research-active physicist to serve as PI *and* compensate support staff who handle a large fraction of administrative duties. A slightly more generous policy here (one month of PI summer salary and one month of administrative support staff salary?) might be helpful in stimulating REU proposals from institutions that are not resource-rich. An additional comment concerning REU awards is that IAP should be alert to situations in which it makes more sense to fund coordinated multiple-site efforts (e.g., in the areas like program evaluation, applicant review, and recruiting noted above) rather than piecemeal components of individual awards. Such efforts might be funded using standalone awards or supplements to a set of existing site awards.

The non-REU portion of IAP's award portfolio includes a variety of projects, most of which are broadly related to education, outreach, and/or broadening participation. We specifically endorse IAP's continued consideration of physics education research proposals that address issues salient for advanced undergraduate and graduate school physics learning, where EHR has less interest and reviewing capacity. (We note that this is currently one of the smallest slices of IAP's portfolio.) For other proposal types, the points made above have several implications for portfolio balance. First, panel review of large education/outreach focused proposals should lead to a set of funded projects with evaluation protocols that are stronger than at present. Improvements in this area will (eventually) deliver a more robust evidence base for assessing these projects' long-term strategic value. Second,
we view IAP's funding of a multi-year proposal to support conferences and workshops that span the subdisciplines of physics as an intriguing new development. Future reviews of proposals of this type across PHY might be usefully coordinated with each other (e.g., using a panel or overlapping ad hoc reviewers), either inside or outside of IAP. Finally, we note that the IAP program occupies a special place in PHY, touching all programs and having a truly unique potential to transform the field. By utilizing NSF's convening power, IAP can catalyze ideas that impact the breadth and health of physics as a whole. We believe that PHY funding of one or more IAP-led workshops or Ideas Lab events should lead to a larger number of seed proposals (with greater potential for innovative and transformative follow-up) than is currently represented in the portfolio. The net effect will be an IAP program that shines even more brightly as a jewel within the Division of Physics than it does today.

F. Nuclear Physics, Theory and Experiment

The physics of nuclei is at the heart of matter (Decadal Study, National Research Council, 2013). Nuclei connect a vast range of distance scales: they emerge from the fundamental properties of quarks and gluons, and they are the fundamental constituents that shape and power the emergent properties of stars. Nuclei connect all the forces: they are powerful laboratories for exploring the strong, weak, electromagnetic forces – as well as possible new ones – and they account for nearly all of the gravitational mass of the visible universe. And nuclei connect fundamental research to science that serves society: they make powerful energy sources, provide unique tracers to identify and age-date materials, and the sensitive instrumentation invented to study them has a wide range of practical applications.

The NSF Nuclear Physics programs – Theory and Experiment – fund focused research from the broad frontier areas of the field (Long Range Plan, Nuclear Science Advisory Committee, 2015). These include the properties and structure of nuclei, tested with a variety of probes; nuclear reactions, ranging from single-nucleon to heavy ion collisions; inputs for astrophysics; and tests of neutrinos and fundamental symmetries. The vibrant research programs are made richer by their interdisciplinary overlap with astrophysics, atomic physics, elementary particle physics, gravitational physics, and more.

In the following, we report on our review of the NSF Nuclear Physics programs, covering both Theory and Experiment (treating them together except as noted), as part of the 2019 Physics Committee of Visitors. We begin with our key conclusions and our review processes, then present our reviews of the merit-review process, the selection of reviewers, the management of the program, and the resulting portfolio of awards.

1. Summary of Findings and Recommendations

A. Observation: *The NSF Nuclear Physics programs fund outstanding research.*

Projects supported by Nuclear Physics address fundamental topics in the nature of visible matter and its interactions. Work in Experiment is done at national accelerator facilities, underground laboratories, and university facilities. Research in Hadrons and QCD is focused on understanding how the properties of protons, neutrons, nuclei, and matter were formed as the universe cooled and how they arise from the interactions of quarks and gluons. Research in Nuclear Astrophysics, Structure, and Reactions probes the nature of the strong and weak interactions, reveals the processes that create the elements, and leads to a wealth of applications that benefit society. Research in Nuclear Precision Measurements uses nuclei, neutrinos, and nuclear-physics techniques to probe the most basic laws of nature (e.g., fundamental symmetries) and to test for new physics. Work in Theory is an essential partner to that in Experiment: it predicts, interprets, and connects. This work includes substantial computational efforts, e.g., ranging from lattice QCD to nuclear structure to supernova explosions.

Projects supported by Nuclear Physics also address interdisciplinary topics. There are emerging opportunities for outstanding cross-cutting science where nuclear physics expertise is critical.

- In astrophysics and gravitational physics, these include the physics of core-collapse supernovae, which produce bursts of neutrinos, and the physics of thermonuclear supernovae, a standard candle in astronomy, for which the value of the Hubble constant now disagrees with that from the cosmic microwave background. The detection of binary neutron star mergers with LIGO has led to multi-messenger signals that depend on the properties of nuclear matter and may indicate the formation of heavy elements through the r-process.

- In particle physics, these include the physics of neutrinos, ranging from their interactions to mixing to other possible properties. In tests of neutrino mixing, a major roadblock is the poorly understood neutrino-nucleus cross sections. The problem is acute in the MeV—GeV regime, where nuclear structure matters. Tests of the Majorana versus Dirac nature of neutrinos through neutrinoless double beta decay also depend strongly on nuclear structure. Tests of fundamental symmetries and new physics, including the direct detection of dark matter, also depend on nuclear physics.

- In quantum information science, nuclear physics expertise in strongly coupled many body systems, as well as computational techniques, have much intellectual overlap with scientific progress needed to develop quantum computing.

The NSF Nuclear Physics programs can contribute significantly to these forefront areas, through funding single investigators and small groups that can push transformative ideas and provide outstanding student and postdoc training. Theory is especially important for opening new areas and for connecting topics. The present Theory program is undersized for maximizing the benefits of the Experiment program.

<u>Recommendation:</u> *The NSF Nuclear Physics programs have outstanding new scientific opportunities that could be realized with new investments, especially in Theory. An upcoming transition in the NSCL stewardship associated with an increased DOE role is planned to have an associated transition of funds and PIs to the Experiment program, which we endorse. This could also provide a critical opportunity to benefit the Theory program, which we strongly encourage.*

B. <u>Observation:</u> *The strength of the NSF Nuclear Physics portfolio is enhanced by excellent community processes.*

A special strength of the nuclear physics community is its robust processes for defining and backing coherent research priorities for the field. In addition to ad-hoc white papers, key documents are the Decadal Studies (driven by the National Academies) and the Long-

Range Plans (driven by NSAC, the Nuclear Science Advisory Committee, which is charged by the NSF and DOE jointly). An obvious consequence is that Nuclear Physics reviewers are aware of these priorities when they rank proposals. A more subtle consequence is that the community's culture values researchers being well informed about and appreciative of the full scope of the field. These facts lead to thoughtful considerations of proposals by full panels, which improves the quality of the reviews.

Recommendation: *Maintaining these community driven processes is essential for the quality and vitality of the portfolio.*

C. Observation: *The strength of the NSF Nuclear Physics portfolio is enhanced by excellent management.*

Theory and Experiment each have permanent-staff program directors (Experiment also has a temporary staff rotator). This structure is beneficial for maintaining institutional knowledge and for finding creative ways to support outstanding science. The reviews are carried out in the usual structure of ad-hoc reviews followed by panel reviews followed by a thorough and well-documented program-director review. In addition, the program directors are given wide latitude to make and defend their decisions, and this is also essential. In addition to these structural points, the particular program directors have shown outstanding judgment and are assets for both NSF and the nuclear-physics community. As part of building the excellence of their programs, they have worked to broaden participation, which we strongly support.

Recommendation: *Maintaining the staffing and processes in NSF Nuclear Physics is essential for the quality and vitality of its portfolio.*

For Physics-wide issues, we strongly support the recommendations of the overall COV report. We highlight the importance of the following: program directors having the freedom and support to do their jobs well, support of a Physics Division mid-scale instrumentation program, as well as efforts to broaden participation, all of which enable innovative science.

2. Description of COV Review Process

The Nuclear Physics COV review process followed the procedures and rules of the general Physics COV review process, as described in the overall report. The Physics Division supplied substantial information on its operations over the past four years, as well as the 2015, 2012, and prior Physics COV reports and responses. We also reviewed the response of Nuclear Physics to the recommendations of the 2015 COV subcommittee, finding their responses and actions appropriate.

For Nuclear Physics specifically, we were provided additional information by the program directors Bogdan Mihaila (Theory; he also covers Physics at the Information Frontier) and Allena Opper and Jim Thomas (both Experiment). We were also given access to many proposals and associated documents through the eJacket system, where these proposals were selected by the program directors to provide a representative sampling. In total, we reviewed 23 Theory and 27 Experiment jackets. Each jacket was reviewed by at least two members of our four-person subcommittee. Careful attention was paid to managing conflicts of interest.

In addition to substantial email discussions, our subcommittee held two video conferences (including the program directors for parts of these meetings) prior to our in-person meeting in Washington in June 2019. The program directors were extraordinarily helpful and forthcoming at providing factual information and explaining their perspectives.

All decisions of our subcommittee, and the text of this report, reflect our consensus opinions.

3. Merit Review Process

The review process in nuclear physics is very thorough, using input from a large number and wide range of reviewers, both ad-hoc and as members of panels, as well as assessments by the program directors.

Documentation

Before review begins, the provided documentation tells reviewers what to expect. Some of this documentation is well developed, e.g., examples of broader impacts. Other parts could be better developed, e.g., examples of expectations for data-management plans, postdoc-mentoring plans, and broadening participation. It would be helpful to develop and collect these and other materials into one obvious place on the NSF Physics webpage, perhaps called something like "Understanding Proposal Review," and advertise it well to the community. (The information Under "Merit Review" is useful but is not specific enough.) This should also include the documentation provided to reviewers, e.g., the discussion of the intellectual-merit and broader-impacts criteria and the five questions to be used to evaluate each. Much of the information above is already developed; it should be completed and collected. More generally, the organization of the NSF Physics and subsidiary webpages could be substantially improved.

There is evidence that some reviewers studied the instructions carefully. Some quote the five questions in their reports and answer those. But there is also evidence that some reviewers have not read all instructions. For a few years, Theory participated in a pilot where proposers provided only qualitative budgets, listing zeros in the budget sheets. Some reviewers were mystified by this and discussed it in their reviews, despite having been told about it in advance. (Importantly, they then set this point aside and focused on the proposal content.) It might be that having a clearly organized collection of information, to which reviewers are directed, would help. This must be streamlined so that readers do not tune out.

If it is not done already, it might also be helpful to include the text of the five review questions in the web form seen by reviewers, perhaps even already inserted in the text boxes, so that the reviewer has the option to answer the quoted questions in order (we saw some reviews like this). But if the reviewer prefers to edit out the questions and give free-form responses, that is fine. Being reminded of the questions may help improve their reviews.

Structure and Quality of the Review Process

The review process has three components.

First, individual reviews, some written by ad-hoc reviewers and some by panel reviewers. Typically, there are 3—6 individual reviews obtained for single-investigator proposals, with more for larger proposals. (For the mail-in reviews, the response rate is about 80%, which is an important sign of the community's support.) These include two or three in-depth individual reviews by panel members. This is a relatively large number, usually in excess of the NSF requirement of three independent reviews, but it enhances the quality of the process. Most individual reviews were substantive, some were excellent, and some could have been more useful. To obtain inputs that cover a wide variety of perspectives, the program directors must continually seek new reviewers, and the quality of their reviews is not known in advance.

For large proposals, there may also be a site visit by a panel charged by NSF to closely review the group and submit a written report; those that we reviewed were thorough and well-considered.

Second, panel reviews, typically with 9—10 members for Theory and 13 or more members for Experiment (depending on the balance between small- and large-scale proposals). Again, this relatively large number is crucially important to the quality of the process. The panel reviews were excellent. The members are carefully chosen, nearly all with prior experience of writing mail-in reviews for this program. It appears that panels worked together very well as groups, coming to consensus and preparing high-quality summary reviews of every proposal.

Third, program-director reviews. A valuable element is the mix among the program directors of experienced permanent staff and research-active temporary staff. The review summaries prepared by the program directors are outstanding. They detail and reflect on all the prior input, adding nuances and framing the reviews in the context of the whole program and its goals. These detailed notes are important for thorough assessment and to provide institutional memory, which is important for talking to PIs, onboarding new program directors, and so on.

In all steps of the reviews, both the intellectual-merit and broader-impacts criteria were appropriately addressed and weighted. As discussed above, the five questions used to assess these could be better employed. In addition, the process could be improved with more common understandings of expectations for broader impacts (and the distinction to broadening participation), data management, and postdoc mentoring.

It could be asked if international reviewers fully appreciate the importance of the NSF's broader-impacts criterion. In the cases we examined, the answer was largely yes, though improvements could be made, for example through the points above about improving documentation. For panel reviewers, their knowledge about this criterion is increased due to seeing many proposals and discussing them with other panel members.

While the ad-hoc reviews generally reach consistent conclusions about the overall merit of a proposal, this is not always the case and there appears to be more of a range of opinion in the Theory proposals. Because the panel reviews are comparative in nature and consensus-driven, they can make clear recommendations, even when the ad-hoc reviews do not. Further, in cases where the panel's ranking does not appear to agree with the ad-hoc reviews, the reasoning is clearly described and well-considered. The independent assessment by the program director provides an additional layer of context and provides the rationale for the funding decision.

The three-part structure, the care taken in selecting reviewers, and the commitment of the program directors combine to make proposal reviews thorough, less susceptible to noise, and of very high quality. The depth of the discussions and the wisdom of the decisions increased at every step in the process, and this is sourced by the amount and quality of the inputs.

Overall, the review process used in Nuclear Physics seems to represent a best practice, based on our experience reviewing proposals for many funding agencies.

<u>Other Aspects of the Review Process</u>

Some proposals are co-reviewed, which is important for building interdisciplinary science, as new topics emerge at the boundaries of traditional disciplines. Co-review may be requested by the proposer but, typically, this decision is led by the program directors, which is preferred, because the program directors are better informed of possible opportunities and which proposals might be competitive for them. We saw examples of proposals that underwent scientific co-review by other programs in Physics and by other Divisions in the MPS Directorate. Importantly, proposals that are reviewed by multiple panels are not subject to double jeopardy: if the other program does not fund a proposal, that does not prejudice consideration in the main program. Overall, we found that siloing does not seem to be a problem, due to the NSF Physics philosophy of focusing on the science.

The nuclear physics program directors take good advantage of co-funding opportunities that further NSF goals and increase the impact of awards. These other avenues include the Integrative Activities in Physics and the Physics at the Information Frontier programs in Physics, the Office of Multidisciplinary Activities in MPS, and the Office of Advanced Cyberinfrastructure in CISE. Co-funding is typically decided by the cognizant program directors.

There are special programs to provide seed funding to develop proof-of-concept evidence for high-risk/high-reward proposals, including RAISE (formerly INSPIRE), RAPID, and EAGER. We reviewed one such example. This proposal, while considered interesting by Program A, was thought by reviewers to really belong in Program B, but also vice versa. Ultimately, following review by the program directors, it was funded through a combination of three sources. We think this was a wise choice to start what may ultimately become important science. It shows the importance of expert and proactive program directors.

Funded awards are subject to annual review by program directors. Generally, the annual reports prepared by PIs were detailed and informative, though there is considerable variation in the nature and length of the responses. In the cases we examined, all for continued annual funding, the program directors made appropriate choices. The NSF instructions for preparing annual reviews could be considerably better, and the distinctions between different questions made clearer.

There is a new requirement for PIs to post publications to the NSF Public Access Repository. This process generally works, but the documentation could be better. For example, should one submit the last PDF sent to the journal or the PDF from the journal web page? It appears that authors have decided the latter is correct, but it is not clear if the journals find this acceptable. If they do, then it would be better for authors if the process of getting the PDFs could be automated once authors supply the DOIs. At the very least, the system should be changed to accept regular PDFs and convert them to PDF/A files, so that PIs do not have to do this by hand.

Communicating Reviews to Proposers

After review is completed and concurred by Division management, the program directors inform proposers of the decision. Proposers are given the text (minus necessary redactions) of the individual reviews, the panel summary, and a context statement on that year's funding and review information. The information supplied is detailed and can help proposers develop better proposals. In addition, the program directors are available for further discussions with proposers, which is very important.

Collecting Data to Inform Broadening Participation

Broadening participation is an important goal for the NSF and our community. Assessment of progress begins with attempting to collect adequate data. Prior COV reports recommended steps to improve this. When a proposal is submitted, the PI and CoIs can fill out a personal demographic questionnaire. Answering this is voluntary, and it is valid to select to not answer individual questions. We gather that these response rates are relatively high, though we were not given data. Gathering data on students and postdocs is done differently. When the PI files an annual report, the participants are emailed a personal demographic questionnaire. We gather that the response rates are relatively low. At present, NSF Physics sends PIs an email explaining the importance of these questionnaires, but only at the start of the award. We recommend that this email be re-sent when each annual report is filed. The PI can share this with students and postdocs. Last, the questions should be made more inclusive, for example by providing additional options for the question about gender.

4. Selection of Reviewers

The expertise of reviewers was well matched to their tasks. For the mail-in individual reviews, the reviewers were specialists who were appropriately chosen. The panels more often had people with broad expertise. The panel members chosen to write individual reviews, though sometimes less specialized than the mail-in reviewers, were also appropriately chosen. The overall compositions of the panels were well designed to cover the suites of proposals at hand in complementary but overlapping ways. The Theory panels included experimentalists and vice versa, which is also a good choice.

The assignment of reviewers and the choices of panel members have a good amount of rotation to ensure a mix of perspectives and to avoid overburdening individuals. This helps to educate the program directors on developments in the field and to educate the community on how the proposal process functions. It helps that the community has a commitment to and a continuing tradition of planning directions for future investments in the field as a whole (e.g., the Decadal Studies and the Long-Range Plans).

We saw that the program directors put considerable effort into managing conflicts of interest. Correctly choosing people with the right expertise – but avoiding conflicts of interest – requires a good knowledge of the field and continuing efforts to build on it. Many obvious conflicts of interest are noted through materials the proposers provide, but the less obvious ones require this knowledge. When panels are convened to consider many proposals, some conflicts of interest are inevitable. The program directors carefully managed those, with reviewers having no access to certain proposals and leaving the call or room for any discussion of them.

The diversity of a panel is an important ingredient for its success, as the synthesis of different perspectives leads to deeper, more objective consideration. The program directors also put considerable effort into making the panels diverse on many axes on the technical side: subject area, proposer professional seniority, level and type of research activity, type of institution and geographic location, and more. In addition, the members were chosen to represent diversity in personal characteristics, including gender and race/ethnicity. This is an important part of broadening participation in the field and in fostering excellence in the panel's work. The panels we examined had fractions of women that ranged from 20% to 40% (the fraction of women in the APS Division of Nuclear

Physics is about 15%). Under-represented minorities constituted between 0% and 15% of panel members, as compared to about 6% nationally.

5. Management of the Program

The Theory and Experiment programs in Nuclear Physics are run separately, but in close coordination, and with very similar approaches. The Experimental program is subdivided into Nucleon and Hadron QCD, Nuclear Astrophysics, Structure, and Reactions (including accelerator facilities at Florida State University and the University of Notre Dame), Nuclear Precision Measurements, and the National Superconducting Cyclotron Laboratory (NSCL).

Prior to FY15, Nuclear Astrophysics was part of a joint program with Particle Astrophysics; we view the move to the Nuclear Physics program as appropriate. As part of that, support for neutrinoless double beta decay was moved to Nuclear Physics Experiment along with the 4-year historical average funding for the program. Since then, Experiment has supported a balanced portfolio of the leading experiments via base grants and made a significant investment of mid-scale funding in the LEGEND-200 experiment.

The funded topics are summarized in reports such as the Decadal Study and the Long Range Plan. Those documents drive the strategies of the field, and the NSF Nuclear Physics programs are guided, though not bound, as new developments arise, by those. In addition, the Nuclear Physics programs are guided by NSF priorities. Of NSF's 2016 Big Ideas, Nuclear Physics is well connected to the Research Ideas Windows on the Universe, Quantum Leap, and Harnessing the Data Revolution, as well as all of the Process Ideas.

While the Department of Energy's Office of Nuclear Physics supports the majority of the national research program in nuclear physics, NSF is a critical partner through its investment in individual investigators, university groups, instrumentation, and university-based laboratories. The PIs supported by the Nuclear Physics programs are scientific leaders in their respective areas, and their programs provide a significant and fertile training ground for the next generation. As noted in the 2015 Long Range Plan, the two programs work in partnership "to maximize the scientific and societal impact of federal spending" in the field, guided by the community-driven planning process.

The program directors have extensive duties in managing the review of proposals, the progress of ongoing awards, staying current on the science to maintain the quality of the program, and more. The program-director workload is high, but the work is getting done because of the quality of the staff. Maintaining this is crucial. The program directors must have adequate staff to help with clerical work and must have adequate time and funds to travel to conferences and universities to keep current and to look ahead to see emerging topics and scientists.

Budget pressure is significant. The funding success rates are approximately 43% for Theory and 38% for Experiment (both averaged over 4 years), which may sound relatively high. However, these programs fund PIs, not projects, so a given researcher would have at most one NSF award at a time, with some exceptions. Reviews apply significant scrutiny to the other commitments of proposers. We found that many strong proposals were declined. As a further point, many awards are undersized relative to needs. In the Theory program, there is very little support for postdocs through the Individual Investigator program, typically < 1 FTE in total for the entire program. This generational gap is unhealthy for the long-term viability of the field.

The program directors have been innovative at working to address these concerns through co-funding opportunities. Additionally, the new Theory Hub program specifically targets support for networks of postdocs around a thematic area, which is helpful, but not sufficient, to overcome the impact of underfunding.

A key part of the program is training junior scientists to become the leaders of tomorrow. An overall theme of the Nuclear Physics programs is supporting the whole cycle of research: from undergraduates, to graduates, to postdocs, to faculty, to the next generations they train, including those who join the non-academic workforce, bringing valuable skills. Nuclear Physics has very strong programs for undergraduates. The Conference Experience for Undergraduates program is outstanding, is a pride of the community, and attracts talent to the field. This is partnered by strong Research at Undergraduate Institutions awards. Graduate training is similarly a high priority. As noted, the situation for postdocs in Theory, while still dire, has been partially addressed. Budgetary pressure in Experiment has also led to a reduction in support for postdocs and graduate students. This trend is somewhat countered on the DOE side through their support for facilities and thus does not affect the overall health of the field in the same way as seen in Theory. Nonetheless, this is still worrisome and points to the need for better demographic information when looking at overall program goals. Finally, through supporting university research with strong broader impacts, the Nuclear Physics programs help enrich education for non-researchers throughout the nation, from classroom teaching to informal education to the public through outreach.

6. Resulting Portfolio of Awards

The Theory program has a budget of about $4M/year and typically supports about 36 individual-investigator awards at a given time. In addition, there is presently one Theory Hub. The Experiment program has a budget of about $18M/year; separately, there is about $24M/year for one large facility, the National Superconducting Cyclotron Laboratory (NSCL) at Michigan State University. The remaining portion supports about 60 investigator awards at a given time, along with two smaller facilities, at Florida State University and at the University of Notre Dame, and a few larger grants to groups of PIs at a given institution. The Nuclear programs are enhanced by separate funding for the Physics Frontier Center JINA-CEE (Joint Institute for Nuclear Astrophysics – Center for the Evolution of the Elements). The timing of the funding cycles for the facilities, group grants, and potential mid-scale awards impacts the number of investigator proposals awarded in a given year.

National Superconducting Cyclotron Laboratory (NSCL)

The NSCL is a national user facility based at Michigan State University, currently supported through an NSF cooperative agreement. At present, about 1,000 researchers are members of the NSCL users' group. Two linked superconducting cyclotrons, the K500 and K1200, combined with the A-1900 fragment separator, create a unique capability for producing rare isotope beams for nuclear structure, nuclear astrophysics, and fundamental symmetries. The NSCL also hosts a broad program in applied physics focusing on medical physics, nuclear forensics, and isotope harvesting. In 2017, the laboratory commissioned the ReA3, which provides accelerated beams in the energy range 0.3 – 6 MeV/u. The NSCL hosts a state-of-the-art suite of detector systems, including the GRETINA gamma-ray tracking array consisting of 28 highly segmented coaxial germanium crystals, which is the first stage of the full GRETA detector system. NSF's support of NSCL includes not only operation of the facility but also the research portfolio of the Michigan State University nuclear scientists along with their students and postdocs.

In FY 2022, it is expected that NSCL will transition to the U.S. Department of Energy managed Facility for Rare Isotope Beams (FRIB). Much of the NSCL's infrastructure will be incorporated into FRIB. NSF operations and maintenance (O&M) support for the NSCL will cease when the facility becomes FRIB, and the transition is being managed by a Joint Oversight Group to allow for a smooth handoff. Retention of the O&M creates an opportunity for investment in other broad and pressing priorities within the Physics Division, such as mid-scale instrumentation and added support for Theory. The research portion of NSCL budget will transition to stewardship within the Experiment program, and NSCL/MSU faculty will compete for these resources along with the rest of the community through the investigator-initiated review process. We view this transition plan as appropriate and good stewardship of these resources for what has been a longstanding and major scientific investment.

The Nuclear Science Laboratory (NSL) at Notre Dame University performs world-leading research in the areas of experimental nuclear structure and nuclear astrophysics, fundamental symmetries, and applied nuclear physics. Continued improvements in accelerators, detectors, and related infrastructure have maintained the position of the NSL as one of the centers for experimental nuclear physics in the U.S. Recently, the NSL has initiated the CASPAR underground accelerator (located at SURF), which is the only underground accelerator in the United States. Nuclear structure research at the NSL examines a wide range of topics, including their impact on our understanding of the r-process. Applied nuclear physics is a new program focusing on the application of accelerator mass spectrometry to problems in geophysics and environmental science.

The John D. Fox superconducting accelerator laboratory at Florida State University operates a two-stage accelerator comprised of a 9MV Super-FN tandem van de Graaff accelerator and a superconducting linear accelerator. The in-flight radioactive-beam facility RESOLUT produces beams of interest for nuclear-astrophysics studies. The commissioning of the "super" Enge spectrometer, obtained from Yale, provides unique capabilities for high-resolution spectroscopy using stable and radioactive beams. In addition, the active-target detector ANASEN (developed in collaboration with Louisiana State University) allows for measurements of (p,p), (d,p) and (α,p) reactions using radioactive beams at both FSU and the NSCL.

Mid-scale instrumentation

The nature of nuclear physics experiments makes them particularly well suited for the mid-scale funds made available at the division level. This creates an avenue for NSF PIs to have a strong, and often leadership, role in some of the most exciting projects that require a higher level of technical or instrumentation support than would normally fit within an investigator award. The projects that have been funded have been well vetted both for scientific merit and technical readiness and have strong management plans.

Program Balance

The awards in the Theory and Experiment programs are each well balanced among major directions of the field, subject to the expected year-to-year fluctuations. This balance is not an accident, and it is appropriately guided by the panels and the program Directors. The Theory and Experiment program areas are well matched to each other. While the program directors respect and follow the proposal-driven process of NSF, they are also attentive to the community's priorities as articulated in the 2013 Decadal Survey and the 2015 Long Range Plan.

In addition to examining subject-area balance, we also considered the distributions of awards on other aspects, including proposer professional seniority, level and type of research activity, type of institution and geographic location, and others. We found these to be well balanced.

Broadening Participation

On the personal diversity of the proposers, an important aspect of broadening participation in our field, we also found the Nuclear Physics portfolio to generally reflect the diversity of the field. For the PIs of awarded proposals, the fraction of women is about 18% for Experiment and 16% for Theory (averaged over four years). This is comparable to the fraction of women faculty in physics departments nationally. (For comparison, the fraction of women in the membership of the APS Division of Nuclear Physics is presently about 15%). About 7% of the declined proposals were from women PIs. For the Nuclear Physics programs, the award data for PIs at Historically Black Colleges and Universities (HBCUs) was provided as a proxy for minority participation, but the data are too sparse to draw conclusions. We found that the program directors have been proactive in taking advantage of programs from both within and outside the Physics Division to support NSF's overall goals of increasing participation from under-represented groups.

Cross-Disciplinary Activities

As noted in the 2015 COV Report, the Division has been using the frame of an overall science portfolio rather than a series of individual programs. The Nuclear Physics program directors have been able to leverage this philosophy by co-funding proposals with other programs within Physics and with other divisions such as Chemistry and Astronomy, or with funds from the MPS Office of Multidisciplinary Activities or Integrative Activities in Physics, augmenting the budget portfolio by about 10%.

Program Highlights

- Nuclear Astrophysics, Structure, and Reactions: Nucleosynthesis in stellar explosions provides information about the explosion mechanism and provides key information relevant to the origin of the elements and the chemical evolution of the galaxy. About half of the elements beyond iron are synthesized in the r-process and modeling the r-process requires a wealth of nuclear structure and reaction information, the latter including a knowledge on (n,γ) rates. Recently, the ^{59}Ni(n,γ)^{60}Ni was measured at the NSCL using the SuN detector and a beam of (radioactive) ^{59}Ni. The precision of the reaction rate was improved by an order of magnitude, into the range needed to constrain r-process models. This work is particularly timely given the recent observation of r-process production in the neutron-star merger, GW170817 and highlights the role of nuclear physics in the era of gravitational-wave astronomy.

  Most deformed nuclei are axially symmetric, but triaxial shapes are expected (and observed) in some regions of the nuclear chart. These shapes can be manifested through excited bands that can be associated with wobbling or chirality, the latter being commonly observed. Measurements of the ^{123}Sb(^{16}O,4n)^{135}Pr have shown the first evidence for wobbling in the A = 130 region: a sequence of intraband ⊗I = 1, E2 transitions in ^{135}Pr.

- Hadrons and QCD: Two regimes are critically important for connecting the fundamental interactions of quarks and gluons to the emergent properties of nucleons, nuclei, and other bound states. First, how a hot soup of quarks and gluons condenses (or hadronizes) into bound states. Characterization of this liquid-like quark-gluon plasma phase of the early universe is carried out through its creation in relativistic heavy ion collisions followed by measurement of photons, leptons, and reformed hadrons as they escape the plasma. Second, for cold matter, understanding observables such as mass, charge, and spin in systems that range from perturbative to nonperturbative. Research supported by the Experiment program includes the first strong evidence of gluon polarization in the proton, determination of the anti-quark momentum distributions in the proton in the non-perturbative regime, and searches for low-lying exotic bound states that require explicit involvement of gluon quanta for them to exist.

  An especially pressing topic is the charge radius of the proton. The value obtained from spectroscopic muonic-hydrogen experiments is significantly smaller than that determined from spectroscopic measurements with regular hydrogen and from early measurements of elastic electron-proton interactions. New measurements of the latter are split. The Experiment program is providing support to two new experiments, with different techniques, to resolve this puzzle. The “Proton Radius” measurement, or PRad, in Hall B at Jefferson Laboratory, uses electron-proton scattering but with detection of the outgoing proton, rather than the electron, allowing measurement of a range of distance scales in one setting. Preliminary results (APS DNP meeting, November 2018) seem to indicate agreement with the muon data. The PRad collaboration is supported by both NSF and DOE, and the gaseous hydrogen target for the experiment was funded from an NSF MRI award in FY12. MUSE, a Physics Division mid-scale funded project, will use mixed muon and electron beams of either charge (mu+, e+, mu-, e-) at the Paul Scherrer Institute in Switzerland to make precise measurements of the lepton-proton elastic scattering cross sections, with the goal to extract the proton charge radius for each probe under the same experimental conditions. Construction is now complete and data collection will begin in November 2019. Several collaboration members are supported through Experiment in addition to the mid-scale project funding.

- Nuclear Precision Measurements: Like the proton radius puzzle, for the anomalous magnetic moment of the muon there is experimental disagreement with the Standard Model that may indicate new physics. The Muon g-2 experiment at Fermilab will greatly improve precision. This requires measurement of the muon precession frequency in a precisely known magnetic field to an uncertainty of better than 70 ppb. The first muon beam was injected into the storage ring for an engineering run in 2017, and enough muons were observed to see muon precession. The first physics run took place in 2018, and analysis is ongoing.

  The physics of neutrino mass represents yet another window onto physics beyond the Standard Model. The fundamental nature of the neutrino mass is unknown, and the search for the rare process of neutrinoless double beta decay is the most promising experimental approach to the problem. The Experiment program supports collaborators on a number of neutrinoless double beta decay experiments employing different detector technologies, including EXO-200, CUORE, and the Majorana Demonstrator. The FY18 investment of mid-scale funding in the LEGEND-200 experiment will ensure continued US participation in a leading search for years to come.

- Theory: An important development is a new program, the Focused Research Hubs for Theoretical Physics. One has been awarded, the "Network for Neutrinos, Nuclear Astrophysics, and Symmetries (N3AS)," with its research program described publicly as "N3AS research focuses on theory issues in neutrino and nuclear astrophysics; dense matter including the modeling of supernovae, neutron stars, and neutron star mergers; nucleosynthesis; and dark matter." The network involves 16 faculty at 11 institutions. The funds are used to support postdocs (3 hired in 2017, 4 hired in 2018) who work two years at one institution and then one year at one of the three lead institutions. This is an important development for Theory in terms of both science and training. In the future, additional Hubs and topics are expected, which we support.

  There was also increased success in CAREER awards for young faculty. In the four years we considered, there were 9 such awards. These span a wide range of topics, including the astrophysics of supernovae, neutron stars, and neutron-star mergers; lattice QCD to probe hadronic physics from parton distribution functions to nucleons to the quark-gluon plasma; and nuclear structure and reactions, from few-body to many-body. These topics connect well to those of the major programs in Experiment.

- Broader Impacts: A frequently highlighted effort is the longstanding and successful "Conference Experiences for Undergraduates" (CEU) in which undergraduate students participate in the APS Division of Nuclear Physics annual meeting. They are provided with targeted programming, encouraged to attend the meeting presentations, required to present a poster, and invited to a graduate school recruiting and information session. Annual participation continues to grow, now approaching 200 students. The program was highlighted in a 2018 article in Physics Today (https://doi.org/10.1063/PT.3.3858).

  As a more technical broader impact, the mid-scale project LEGEND-200 (L-200) aims to push the frontiers of sensitivity in germanium detection of neutrinoless double beta decay through pursuit of a 200-kg scale experiment. L-200 is based on combining the techniques developed by prior experiments to both produce ultra-clean components and to suppress background through immersion of the detectors in a liquid argon bath. Potential societal benefits include advances in environmental monitoring, improved methods of radioactive dating, highly sensitive monitoring of reactors, biological tracers at very low activity, and advances in occupational health monitoring.

7. Coda

The physics of nuclei provides a powerful laboratory for ever-more-precise measurements of the nature of matter and the known forces. It is also essential for tests of neutrinos and new physics, as well as for revealing the processes that shape the majesty of the visible universe. The NSF Nuclear Physics programs fund outstanding research at these frontiers. Key to this are both the excellent community processes and the excellent management of the program.

G. Particle Astrophysics

Introduction

The particle astrophysics program supports a variety of investigations that sit at the boundary of physics and astrophysics. The program is divided into three sub-areas: cosmic phenomena covering studies of astrophysical sources of gamma-rays, cosmic-rays and neutrinos (excluding IceCube); underground physics which includes studies of solar, atmospheric and reactor neutrinos, neutrino mass measurements and direct dark matter detection experiments; and IceCube Research Support which supports the IceCube experiment at the south pole.

To evaluate the program, the CoV subpanel was provided with a report of the particle astrophysics program for FY2015-16-17-18. On June 5, the PA program directors and the PA CoV subcommittee held a virtual meeting to discuss the particle astrophysics program; and the panel reviewed 26 proposal jackets (15 awards and 11 declines). The panel met for 2 days for an in person meeting on June 20-21 to discuss and write the report.

The program directors have successfully managed the particle astrophysics program. The COV subcommittee was particularly struck when we were informed that an evolving decision has been made to move from managing the program towards program breadth (make sure as many groups who could be funded would be funded even at reduced levels) to managing the program toward projects with the highest quality (make sure quality proposals are well supported towards desired outcomes often resulting in fewer overall approved proposals). What struck the COV subcommittee was that the program directors were willing to be flexible in their own metric of successful management. This example shows active reflection by the program directors on the overall management. An argument could be made for either metric – it took some positive self-reflection to make such a change with the end goal being better overall and thoughtful management of the program.

Staffing of the NSF particle astrophysics program is adequate, but with little margin given the other responsibilities of the program directors outside the particle astrophysics program. Since the field of particle astrophysics is growing, program office staffing may become an issue in the future.

Since the last COV report, the particle astrophysics program has implemented mechanisms to hold comparative reviews of long-duration operating experiments and added language to the solicitations to request information on expected project lifetime and milestones. The program office has also introduced mechanisms such as collaboration matrices (which list the contributions of individual university groups in the collaboration) and site visits to avoid duplication. The project has also improved the technical implementation of the “asynchronous” review process. The panel commends the program office for introducing these initiatives and recommends that they continue to evaluate methods to effectively evaluate proposals coming from individual investigators in large collaborations.

We feel the PA program directors do a laudable job of reducing funding for lower priority aspects of excellent proposals in order to appropriately fund the very good proposals. The distribution of award durations and use of creative extensions to extend award by up to two years is appropriate and a good fit for the particle astrophysics program.

The panel was pleased to see an increasing fraction of young (<10 years since PhD) PIs successfully proposing to the particle astrophysical program. This is indicative of a healthy, growing scientific field. We note that the program has faced significant funding pressure due to changing priorities with partner funding agencies. In particular, the withdrawal of DOE from cosmic-ray and gamma-ray experiments, combined with the increasing size and cost of new facilities is placing significant funding burdens on the program. In the absence of additional funding, this will necessarily lead to an erosion of US leadership in some of these important areas.

Finally, we note that due to their interdisciplinary nature, projects within the particle astrophysics program are ranked by two separate extensive community processes- the decadal survey which focuses on astrophysics prioritization and P5 which focuses on high energy physics priorities. NSF will need to appropriately balance priorities and recommendations between the two.

The integrity and efficacy of processes used to solicit, review, recommend, and document proposal actions:

The panel members were impressed with all aspects of the design and implementation of the proposal process. Program directors make thoughtful and well-documented decisions despite some difficult circumstances.

Reviews of proposals consistently included substantial discussion from which the expertise of the reviewers was evident. The program directors have composed committees with appropriate attention to diversity that cuts many ways (personal characteristics of the committee members, affiliation of the committee members (i.e. size and scope of institution from which reviewers come from) and, to a degree, the specific backgrounds of reviewers. Processes designed to ensure unbiased discussions (such as conflict of interest evaluations, etc.) are rigorously and consistently followed.

The documentation of the proposal review is complete and the review analysis documents are comprehensive with full consideration of ad hoc reviews, discussion in the panel, and all other relevant factors. We consistently found value added review analysis by the program directors in consideration of both merit criteria. We found that post review communication with the PI, as recommended by the previous CoV, is happening at an adequate level.

Since the last CoV report in 2015, the in-person reviews are now handled in two separate panels: Particle Astrophysics underground projects + IceCube (covering underground experiments and IceCube) and Particle Astrophysics Cosmic Processes (covering gamma-ray, cosmic-ray and neutrino experiments). This transition makes sense and allows the discussion of the proposals under review to be more focused and more efficient.

We especially appreciate the flexible approach that the program directors take to supporting large collaborations that have more PIs than can be funded. For large collaborations at multiple institutions, the standard review process might not fully take into account connections between different proposals, or project prioritization. We recognize the use of an analysis matrix in large collaborations like VERITAS and IceCube to deal with the avoidance of duplication and to identify collaboration priorities. The program directors engage directly with collaboration leadership and individual PIs to determine the appropriate funding model on a case-by-case basis e.g. single PI with multiple sub-awards or collaborative proposals with multiple PIs.

The previous CoV panel supported asynchronous reviews, which allows reviewers to read and provide feedback on proposals prior to the face-to-face meeting. This initiative had just started prior to the previous CoV review and they recommended enabling better technology to manage the process. This approach is now routinely used and significantly improves the panel review process by starting the discussion earlier. The interaction panel system is now working well and earlier technical issues appear to have been addressed.

We encourage the PA program to continue to identify and advertise opportunities for their communities. The Windows On the Universe big idea is an excellent example of a foundation level opportunity that the program directors have leveraged to benefit the particle astrophysics community.

The quality and significance of the results of the Division's programmatic investments:

The particle astrophysics program supports world-leading facilities in gamma-ray, cosmic-ray and neutrino astrophysics. These investments include world-class facilities using well established techniques and guaranteed science return such as VERITAS, HAWC, AUGER, IceCube and Telescope Array. The program also supports innovative projects such as ARIANNA and ARA that have the potential to open an entirely new ultra-high-energy window on the Universe. The science return has been exceptionally high. Of particular note is the increasing synergy and joint studies between the projects that combine results from multiple collaborations. An example of this has been the high-resolution studies by VERITAS of new gamma-ray excesses seen by HAWC.

The particle astrophysics program has made high quality investments in the area of dark matter detection with an appropriate balance between development of high-risk/high-reward novel techniques and existing experiments that are producing world-leading results. The program supports detectors using noble liquids, salts and crystals, bubble chambers with spin-dependent targets, and several initiatives suitable if the dark matter particle turns out to be light. This breadth is appropriate as there is a large range of possibilities as to the particle nature of dark matter. In addition, several funded activities have a strong component towards novel detector development where a breakthrough could result in an enabling technology for mounting a larger future effort.

The particle astrophysics program is providing support towards two leading experiments in cosmic microwave background (CMB) research. The next generation CMB experiment (CMB-S4) will focus on definitive measurements of the power spectrum separated into polarization components. The two supported efforts include the current state of the art CMB experiment in polarization and the leading terrestrial experiment that will have to scale in size and scope and have polarization sensitivity to reach its science goals.

The PA program supports a diverse set of neutrino experiments at reactors and underground in order to measure well the remaining parameters in the three-neutrino paradigm and search for rare sources or new neutrino reactions. New measurements of solar, supernova and geo-neutrinos can be used to reveal information about the physical properties of the source.

The relationship between award decisions, program goals, and Foundation-wide programs and strategic goals:

The Windows on the Universe (WoU) initiative is an example of the particle astrophysics program connecting directly to a foundation-level program. The Windows on the Universe big idea - combining gravitational wave, neutrino and electromagnetic data to explore the universe in a powerful new way - is an excellent match to the particle astrophysics program. Neutrinos are one of the new messengers for astrophysical studies. Gamma-ray observations have been an integral part of every multimessenger observation to date: gamma-ray lines and neutrinos from the nearby supernova SN1987a, a gamma-ray burst and gravitational waves from merging neutron stars in GW170817, and identification of a flaring gamma-ray blazar from the location of high energy neutrinos.

The particle astrophysics program provides the neutrino and gamma-ray observations, and the WoU initiative provides the necessary cross program (to gravitational waves) and cross division connections (to astronomy) to fully pursue this new area. The implementation of the WoU program, allowing relevant proposals received through existing mechanisms to be augmented and connected together in a common goal, makes the whole of these efforts be greater than the sum of the parts.

H. Physics Frontier Centers and Physics at the Information Frontier

H1. Physics Frontier Centers

Introduction
The Physics Frontiers Centers (PFC) program supports university-based centers, enabling transformational advances in a range of research areas at the forefront of physics. Importantly, the intent of the PFCs is to provide resources and support activities not normally possible through grants to single investigators or small groups. Research themes of PFCs may fall under any sub-fields of physics within the Division of Physics (atomic, molecular, optical, plasma, elementary particle, nuclear, astro-, gravitational, and biological physics); the program encourages interdisciplinary projects between these physics areas and other disciples, provided the majority of the activities fall in one of the areas in the Division of Physics. NSF's program solicitation states that successful proposals for PFCs must demonstrate (1) the potential for a profound advance in physics; (2) creative, substantive activities aimed at enhancing education, diversity, and public outreach; (3) potential for broader impacts and benefits to society; (4) a value-added rationale that justifies a center- or institute-like approach. The last review criterion here is an additional component required of PFC proposals, compared to other NSF awards.

The PFC program is ably managed by co-directors Jean Cottam Allen and Kathleen McCloud. The program directors split their time between this and other programs: Dr. Allen is also Acting Deputy Division Director and co-manages the Particle Astrophysics – Experiment program, among other program responsibilities; Dr. McCloud manages the Integrative Activities in Physics program, among other program responsibilities.

PFC awards are made for five years, with the option to extend funding for one year. The PFC program operates on a three-year cycle; recently, open competitions have been held every three years. At the end of the award, PFCs can re-compete for another award, with no limit on the number of terms. Conversely, there is no set expectation that funding will continue for a funded center when an award expires. In the case that a renewal proposal is unsuccessful, phase-out funding can be provided (at a reduced level) for up to two years.

The first PFC competition was held in FY2001, resulting in four PFC awards. The most recent competitions were held in FY2014 and FY2017. In the FY2017 competition, two existing centers were phased out, and one new center was awarded.

The PFC program is a very important one for NSF, as it highlights extremely high-profile, cutting-edge science. The management of the PFC program by the program directors is truly excellent and they have successfully maintained a high level of quality and novelty of program activities. As noted also by the 2015 COV, the program directors have been remarkably successful at securing co-funding for the PFCs from other divisions across NSF.

General Overview
The PFC program currently (FY2019) funds 9 Centers. The current list is as follows.

Started or renewed in 2017 or 2018:

- Institute for Quantum Information and Matter (IQIM), Caltech
- JILA Physics Frontier Center, University of Colorado, Boulder/NIST
- Center for Ultracold Atoms (CUA), Harvard/MIT
- Center for the Physics of Biological Function (CPBF), Princeton

Started or renewed in 2014 or 2015:

- North American Nanohertz Observatory for Gravitational Waves (NANOGrav), University of Wisconsin, Milwaukee
- Center for the Physics of Living Cells (CPLC), University of Illinois Urbana–Champaign
- Center for Theoretical Biological Physics (CTBP), Rice University
- Physics Frontier Center at the Joint Quantum Institute (PFC@JQI), University of Maryland, College Park
- Joint Institute for Nuclear Astrophysics—Center for the Evolution of the Elements (JINA–CEE), Michigan State University

In addition, there are two centers that were recently phased out:

- Kavli Institute for Cosmological Physics (KICP) Physics Frontier Center, University of Chicago (phased out in 2019)
- Kavli Institute for Theoretical Physics (KITP), University of California, Santa Barbara (phased out in 2018, supported as of 3/2018 through IAP with co-funding from DMR and AST)

The total budget for the PFC awards, broken down by funding source and fiscal year, is shown in the table below.

Funding Source	**FY2015**	**FY2016**	**FY2017**	**FY2018**
PFC program	$21.3M	$21.3M	$21.3M	$16.6M
Co-funding	$6.68M	$4.66M	$8.48M	$3.98M
Total	**$28.0M**	**$26.0M**	**$29.8M**	**$20.6M**

There are two notable elements in the data here. The first is, as noted before, an extraordinary range of co-funding partnerships, both within and beyond the Directorate for Mathematical and Physical Sciences (including OMA; MPS/DMR; AST; CHE; BIO/MCB; IOS; CISE/CCF; and GEO/PL. This represents an outstanding effort by the PFC program directors.

The second notable element in the budget data is a substantial (and, evidently, permanent) drop in the direct budget to the PFC program. The likely immediate outcome of this funding change is that the number of funded centers in the ongoing PFC competition will drop to 3, compared to 4 in the FY2017 competition. The direct funding to PFC in FY2015-7 amounted to about 8% of the total physics budget, dropping to a bit above 5% in FY2018 (though some of this is explained by a "bump" in infrastructure funding). The 2015 COV also noted the 8% figure in past years, and that this amount is consistent with recommendations of past COVs that this fraction be kept at less than 10%, although the 2015 COV itself declined to recommend a particular amount. The question of an optimum allocation of funds to the PFC program relative to NSF as a whole is a difficult one to pinpoint exactly; nevertheless, given the immense impact of the PFCs and the success of the PFC program, it is difficult to see the FY2018 change as a positive development. Clearly, any further decrease in funding would profoundly jeopardize the success of the program.

From the PFC budget and the relatively small number of awards, it is clear that the PFC awards represent large financial commitments. In our opinion the size of the awards is justified: it is an important incentive to attract proposals from top PIs, despite the fierce competition and large workload in proposing and managing a center. The large awards also enable important center-level capabilities that would otherwise not be possible, such as hiring a full-time outreach coordinator. Given the decrease in the PFC's total budget, it seems to us that the better coping strategy is to reduce the number of, rather than the size of, the awards.

Review Process: Integrity and Effectiveness

In the published program guidelines, the stated review criteria for PFC proposals are (1) the potential for a profound advance in physics; (2) creative, substantive activities aimed at enhancing education, diversity, and public outreach; (3) potential for broader impacts, e.g., impacts on other field(s) and benefits to society; (4) a synergy or value-added rationale that justifies a center- or institute-like approach. These criteria were also communicated to ad hoc reviewers and review panels.

The first stage in the review process is a preliminary proposal, which is rated by ad hoc reviewers and then a review panel. Successful preproposals are invited to submit full proposals. In the period FY2015-FY2018, preproposals were considered by 15 panelists; 44% of the preproposals led to invitations for full proposals. Several preproposals were returned without review.

The second stage in the review process is the full proposal, a long and detailed proposal compared to single-investigator proposals. The full proposal is again rated by ad hoc reviewers, with reviewer ratings being integrated directly by the program directors; no panel is involved at this stage. Successful awards at this stage are invited for a reverse site visit. In FY2015-FY2018, after consideration by 91 ad hoc reviewers, 83% of the full proposals were invited for reverse site visits.

In the final stage of the review process, the reverse site visit, PIs pitch the center to a review panel over a two-day period. A useful aspect of the reverse site visit is that PIs have a chance to respond to issues raised in ad hoc reviews, and on the second day the PIs answer questions and concerns raised by the panel. In FY2015-FY2018, the reverse site visits were considered by a 15-member panel, leading to 4 awards (3 renewals and 1 new award).

Note that at all stages in the process, other program directors in fields relevant to the proposal also contribute their analysis of the panel reviews, which can be an important deciding factor for the success of proposals. Importantly, as noted above other program directors can contribute partial funding for a center award. These funding commitments are strong indicators to the PFC program directors of the merits of the proposal in a particular field.

The funded centers are also carefully overseen through site-visit reviews. These include mid-cycle reviews and reviews for one-year funding extensions at the end of an award, to bridge center support until the next competition. In FY2017 and 2018 there were 5 mid-cycle site visits, involving 18 total panelists. The panel reviews were positive, and all of them recommended continued funding. In FY2016 there were 6 site-visit reviews (for 5 one-year supplements, and an additional review of one center), involving 28 total panelists. All of the centers requesting supplements received funding, but with some at a reduced level as a result of the panel reviews.

We considered in detail 15 jackets, comprising 5 preliminary proposals, 8 full proposals, and 2 supplement proposals from the FY2017 competition. Panel summaries were impressively detailed in their analysis and rationale.

Similarly, the review analyses from the program directors were impressively detailed and carefully reasoned, thoroughly documenting the rationale for the success or failure of the proposals. In two jackets where the program directors did not strictly follow the panel's recommended proposal ranking, they were particularly careful to note their reasoning, corroborating their opinions with those of directors from other programs. In one jacket, the directors allowed a proposal to go forward from the preliminary stage, though the panel recommended against this; this decision was vindicated by the good performance and ultimate success of the proposal in the final selection process. In the second jacket, the directors made a hard decision to decline a proposal with serious issues, despite a positive panel recommendation. We were pleased to see the leadership shown by the program directors in these cases, and we agreed that their decisions were carefully considered and soundly reasoned. The program directors clearly took the panel recommendations seriously, but still showed good initiative by synthesizing all available information and coming to independent conclusions in the process of making difficult decisions to allocate scarce resources.

Conflicts of interest were consistently and carefully identified and documented in review analyses and diary notes. The conflicts were resolved through recusals of panelists or ad hoc reviewers, or through consultation with the Office of General Counsel and the determination that apparent conflicts were not disqualifying.

The review and oversight processes in the PFC program represent an enormous workload for reviewers, panelists, PIs, and program directors. This effort is justified, however, given that each funded PFC constitutes a large commitment of resources. We feel that the program directors do an admirable job of running a thorough, intense, fair, and efficient selection process, and that the site-visit process is likewise excellent in ensuring high-quality output from the supported centers.

Intellectual Merit
The Physics Frontier Centers have produced the very highest level of groundbreaking physics in areas that require the concerted effort brought by the center-level organization.

<u>Center for the Physics of Biological Function (CPBF)</u>
CPBF is focused on collective behavior in biological systems, their underlying general principles and emergent properties they exhibit. Researchers in the CPBF have developed new approaches to larger neural populations. They have developed phenomenological coarse-graining procedures and applied them to recordings from a population of 1000+ cells in the hippocampus. They found evidence of scaling in both static and dynamic quantities; exponents are reproducible across mice, in some cases to the second decimal place. These results suggest that the collective behavior of the network is described by a non-trivial fixed point.

> Meshulam et al., Collective Behavior of Place and Non-place Neurons in the Hippocampal Network, *Neuron* **96**, 1178-1191.e4 (2017).

<u>Center for the Physics of Living Cells (CPLC)</u>
CPLC aims to develop a quantitative, physical description of living cells using a combination of theory, computation and experiment. Recent work involved inserting QM/MM into their NAMD molecular dynamics software. Applying this approach to a key reaction that sets the genetic code revealed subatomic details for this essential step of life.

> Melo et al., NAMD goes quantum: an integrative suite for hybrid simulations. *Nat. Methods* **15**, 351-354 (2018).

Another CPLC team found that the environment is crucial for determining how bacteria adapt when their swimming speed and population growth rate are restricted by a trade-off. When nutrients are plentiful, E. coli populations evolve to spread faster by swimming more quickly despite growing more slowly. In contrast, if nutrients are scarcer, the bacteria evolve to grow more quickly and swim more slowly. Next-generation sequencing identified single mutations that changed both swimming speed and growth rate by modifying negative regulatory activity in the cell.

> Shih et al., Biophysical constraints determine the selection of phenotypic fluctuations during directed evolution, *Physical Biology* **15**, 065003 (2018).

Center for Theoretical Biological Physics (CTBP)
Recent work at the CTBP has succeeded in extending the theoretical and computational physics to an ever-widening array of processes of living systems. Three articles in PNAS involved the prediction of folding and dynamics conformations of eukaryotic chromosomes

PNAS **113**, 12168-12173 (2016), *PNAS* **114**, 12126-12131 (2017); *PNAS* **115** 7753-7758 (2018).

In addition, CTBP researchers have interrogated the relationships between cytoskeleton and connected structures at the dendritic spine and the creation and storage of memories of past activity.

Chen et al., *PNAS* **113**, 5006 (2016).

Center for Ultracold Atoms (CUA)
The CUA addresses how microscale physics determines the macroscale behavior of quantum many body systems. A breakthrough made in the Ni group allows single particle control of ultracold molecules in optical tweezers, combining two atoms in a single controlled reaction to form one molecule.

Science **360**, 900 (2018).

CUA researchers also demonstrated a new method for creating controlled many-body quantum matter that combines deterministically prepared, reconfigurable arrays of individually trapped cold atoms with strong, coherent interactions enabled by excitation to Rydberg states.

Science **354**, 1024 (2016); *Nature* **551**, 579 (2017).

They have also prepared high-fidelity entanglement between two atoms, establishing neutral atoms as a competitive platform for quantum information processing.

PRL **121**, 123603 (2018).

Institute for Quantum Information and Matter (IQIM)
IQIM is devoted to advancing the entanglement frontier through the combined efforts of theorists and experimentalists. They have developed superconducting metamaterials that may play an important role in future quantum computing.

Mirhosseini et al, Superconducting metamaterials for waveguide quantum electrodynamics, *Nat. Comm.* **9**, 3706 (2018).

IQIM researchers have also made progress in speeding up quantum algorithms for semdefinite programming.

> Brandão and Svore, Quantum speed--ups for semidefinite programming, *Foundations of Computer Science (FOCS), 2017 IEEE 58th Annual Symposium on*, pp. 415--426. Brandão et al, Quantum SDP solvers: large speed--ups, optimality, and applications to quantum learning, arXiv:1710,02581 (2017).

<u>JILA Physics Frontier Center</u>

JILA explores quantum matter, correlated states and out of equilibrium dynamics of quantum systems. JILA researchers have succeeded in creating and harnessing quantum entanglement with gases of strontium atoms in an optical cavity.

> Norica et al., Cavity--mediated collective spin--exchange interactions in a strontium superradiant laser, *Science* **361**, 259 (2018).

Other JILA researchers demonstrated with very high precision that the shape of the electron is round.

> Cairncross et al. A precision measurement of the electron's electric dipole moment using trapped molecular ions, *PRL* **119**, 153001 (2017).

<u>Joint Institute for Nuclear Astrophysics—Center for the Evolution of the Elements (JINA--CEE)</u>

The JINA multi--institutional, multidisciplinary center that brings together scientists working in experimental and theoretical nuclear physics, astronomy, astrophysics, and computational physics to address fundamental questions about the cosmos.

In their quest to define the origin of heavy elements, JINA researchers showed that the neutron star merger rate and amount of ejected material inferred from GW170817 are compatible with neutron star mergers being the primary site for the origin of r--process nuclei.

> Côté, B. et al., *ApJ* **855**, 2 (2018).

<u>North American Nanohertz Observatory for Gravitational Waves (NANOGrav)</u>

The NANOGrav Physics Frontier Center focuses on a transformational fundamental physics experiment: the direct detection and characterization of low--frequency (nano--Hz) gravitational waves (GWs).

The NanoGrav 11--year data release and a corresponding limit on the GW stochastic background provides meaningful constraints on the processes through which galaxies merge. They showed that either 1) the emission of gravitational waves is not the only process through which the black holes at galactic cores are losing energy, 2) that mergers occur less frequently than we thought, or 3) that black holes at the centers of galaxies are less massive than previously thought.

Astrophys. J. Suppl. **235**, 37 (2018); *Astrophys. J.* **859**, 47 (2018).

<u>Physics Frontier Center at Joint Quantum Institute (PFC@JQI)</u>

The Physics Frontier Center at the Joint Quantum Institute (PFC@JQI) addresses quantum coherent phenomena, long a central feature of Atomic, Molecular and Optical (AMO) and more recently of Condensed Matter (CM) physics, in direct connection with quantum information science (QIS).

JQI researchers discovered how the qubit magnets organize as different phases form, dynamics that the researchers say are nearly impossible to calculate using conventional means when there are so many interactions.

Zhang et al., Observation of a many-body dynamical phase transition with a 53-qubit quantum simulator, *Nature* **551**, 601 (2017).

Other JQI researchers explored topological pathways for single photons.

Sabyasachi Barik et al., A topological quantum optics interface, *Science* **359**, 666 (2018).

Broader Impacts

Education and outreach initiatives are a major PFC activity, and all of the existing PFCs have shown creativity and enthusiasm in these efforts. Novel curricula at the undergraduate level, hands on experiences for all levels from K-12 to postdocs, and interactions with high school teachers contribute to a broad education portfolio for the PFC program. One example of a particularly useful and impactful education effort is the recurring CPLC summer school. Students from all over the US and the world can participate and are taught cutting-edge experimental and computational biophysical techniques. Not only does this activity enhance the training, career and confidence of the student participants, but it also provides unique teaching experiences for the CPLC teaching assistants in the workshop. This in turn helps them in the pursuit of careers in biophysics. Production by IQIM of two highly professional films, including Hollywood stars and Stephen Hawking, *Anyone Can Quantum* and *Quantum is Calling*, strikes the COV as a particularly novel and inspiring outreach activity that introduced quantum concepts such as entanglements, super-position and teleportation to the general public. IQIM also produced highly popular animated Youtube movies. Many of the centers have strong outreach programs targeting K–12 students and their teachers to increase interest in physics, generally.

In terms of increasing diversity in Physics, it remains difficult to obtain a measure of the participation of URMs in the Centers as either graduate students or postdocs. Each PFC is required to have a diversity plan, but the contours and strategies of these plans were not always explicitly provided in progress reports, and it was not always clear from the reports what the milestones are and whether they are being met. Thus, the overall diversity effort in the PFCs was difficult to judge.

A recommendation to address this last point is that the program directors remain vigilant about reporting of diversity efforts by the PFCs, so that progress in this area can be documented and highlighted.

Recommendation of Prior COVs

A recommendation shared by the two most recent COVs is the commission of a broader review of the PFCs and the PFC program. The 2012 COV stated

> We recommend that the NSF charge an appropriate high-level body, possibly the National Academy of Sciences, to conduct a retrospective review of the PFCs, outside of the context of a funding competition for renewal and new starts.

while the 2015 COV agreed, saying

> Our charge was to evaluate process, and in that regard the program comes through with flying colors. However, there is much more to the story. We believe that the Center program would benefit from a dedicated comprehensive review by a high-level body with the time, access and expertise to evaluate the PFC program. One would like independent confirmation that the PFCs add value in a way that individual investigator grants do not. Are the claims of synergy justified? And if they are, should the fraction of the Physics Division budget be increased? These are questions we were not equipped to address, but clearly need answering.

In response to these recommendations, the Physics Division charged the Advisory Committee for Mathematical and Physical Sciences (MPSAC) to convene a subcommittee to evaluate the PFC Program. The subcommittee was charged with conducting an independent assessment of the PFC program as a whole, rather than performing detailed evaluations of individual centers. The subcommittee is identifying strengths and weaknesses of the PFC program, which can be used by the Physics Division to develop and enhance the program. The subcommittee is not reviewing the review and selection process for proposals nor the funding levels, as these are the purview of the COV (despite the comments noted above by the 2015 COV). The review has been ongoing during the past year; as the final report is due to MPSAC in June 2019, it will not be available in time for this COV to review. Nevertheless, we are happy to see this response to the recommendations of prior COVs, and we believe the structure and scope of the review is appropriate and will be beneficial to the program's future.

Recommendations

The PFC program is an excellent, tremendously successful, and well-run program; thus, the COV is not recommending major changes. It is clear that the PFCs consistently produce groundbreaking scientific discoveries, and they are phenomenal in terms of broader impact.

Although we will not be able to comment on the MPSAC subcommittee's review of the program, we think that idea of the review is a good one, and suggest that the PFC program conduct this review again at some time in the future.

We also reiterate that the reduction in funding level for the PFC that began in FY2018 is a disappointing development. We suggest maintaining the size of PFC awards, even though this will translate into a reduction in the number of awards. As a minor mitigation, the PFC PDs may consider limiting even further the number proposals that move on beyond the preliminary stage.

The 2015 COV commented that "It is important for the Division to remain vigilant so that the Centers do not become entitlements, unfairly leveraging their history and momentum in competitions." We believe that the PFC program's selection process has been successful in this regard, and we encourage the Division's continued vigilance.

H2. Physics at the Information Frontier (PIF)

The Program Director, Bogdan Mihaila, devotes 50% of his time to the PIF program and the remainder of his time to the Theoretical Nuclear Physics program.

Program Synopsis

To support the use of computation for advancement of physics, the NSF Physics Division has long supported a Physics at the Information Frontier (PIF) program. PIF addresses compelling scientific goals relevant to disciplines within the purview of the Physics Division by supporting the development of enabling capabilities through computational advances. The program acknowledges the rapid convergence of Big Data and High-Performance Computing and advances research in algorithm development, efficient use of novel architectures, and community-building activities for computational and data-enabled science.

The Physics at the Information Frontier (PIF) program focuses on studies relevant to disciplines supported by the Physics Division, while encouraging broader impacts on other disciplines. Disciplines within the purview of the Physics Division include: atomic, molecular, optical, plasma, elementary particle, nuclear, gravitational and biological physics, and particle astrophysics. In addition, PIF supports the development of tools and infrastructure that provide rapid, secure, and efficient access to physics data via heterogeneous or distributed computing resources and networks. Examples include development of reliable digital preservation, access, integration, and curation capabilities associated with data from Physics Division experimental facilities and the tools for data handling needed to maximize the scientific payoff.

PIF is the PHY representative in the Computational and Data-Enabled Science and Engineering (CDS&E) program, which crosses multiple Divisions within the Directorate for Mathematical and Physical Sciences (MPS), the Directorate for

Engineering (ENG), and the Office of Advanced Cyberinfrastructure (OAC) in the Directorate for Computer and Information Science and Engineering (CISE).

PIF was conceived as a program in Computational Physics, informed by a PHY workshop entitled, Computation as a Tool for Discovery in Physics, held at NSF in September, 2001. Later on, PIF added a Quantum Information Science (QIS) component. Examples of activities supported by PIF include:

1. Development of novel algorithms, including machine learning, to enable new science in the fields of study within the purview of the Physics Division.
2. Development of novel algorithms to take advantage of state-of-the-art computational architectures, including GPUs, MICs, Playstations etc.
3. Development of novel algorithms for non-grid analysis of data or virtual organization software associated with LIGO and LHC.

PIF co-reviews and co-funds projects with the Cyberinfrastructure for Sustained Scientific Innovation (CSSI) program in OAC. PIF supports and participates in several major activities that demonstrate intellectual excellence and positively impact the physics community and society at large. Here are two examples:

1) Ideas Lab: Practical Fully-Connected Quantum Computer Challenge (PFCQC)

Ideas Labs are intensive meetings that bring together multiple diverse perspectives to focus on finding innovative cross-disciplinary solutions to grand challenge problems. The goal is that bringing together researchers from diverse scientific backgrounds to generate fresh perspectives and innovative approaches on the design and fabrication of quantum devices and processors and the implementation of quantum information processing algorithms. The ultimate goal of the Ideas Lab on Quantum Computing is to facilitate the development and operation of a *practical*-scale quantum computer that will enable the solution of science problems that are currently beyond the reach of modern high-performance computing applications on classical computers.

This Ideas Lab was organized by the Division of Physics (PHY/PIF) in MPS, the Division of Computing and Communication Foundations (CCF) in CISE, and the Division of Electrical, Communications and Cyber Systems (ECCS) in the Directorate for Engineering (ENG). The Ideas Lab resulted in a $15M total award over 5 years to a team lead by Duke University for the "Software-Tailored Architecture for Quantum co-design (STAQ)" project (http://staq.pratt.duke.edu), which involves 9 researchers from 7 institutions.

2. IRIS--HEP: Institute for Research and Innovation in Software for High--Energy Physics, iris-hep.org

IRIS-HEP is a Software Infrastructure for Sustained Innovation (S2I2) Institute led by Princeton University. The institute is co-funded by OAC and PHY/PIF and supports 21 researchers from 17 institutions at a level of $25M total over 5 years. This project is a perfect example of the convergence of HPC and Big Data analytics. It involves experts in physics, computer science, data science, applied mathematics, and software engineering. IRIS-HEP requires multi-agency coordination, especially with the Department of Energy and international research centers. The integration of algorithms, software development, distributed computing, distributed data organization, management, and access (DOMA) enables progress on a critical scientific application that complements the NSF MREFC for the High Luminosity upgrade at the LHC. One of the objective of IRIS-HEP is to tackle challenging problems and provide new solutions that can be transferred to other scientific domains.

IRIS-HEP is the result of a bottom-up approach supported by a conceptualization project co-funded also by OAC and PHY/PIF in FY2015, see s2i2-hep.org. Two reports were produced by the community:

- A Roadmap for HEP Software and Computing R&D for the 2020s (Community White Paper) --- arXiv 1712.06982
- Strategic Plan for a Scientific Software Innovation Institute (S2I2) for High Energy Physics --- arXiv 1712.06592

Budget

The PIF/CP budget was steady, slightly oscillating around $6.5 mil, see Fig. 1. In FY2016-FY2017, PIF had two funding allocations: the Computational Physics allocation and a direct PIF allocation. Between the two funding allocations, the budget for PIF is approximately $6.5M/year. In order to simplify administration and promote transparency the two lines were gradually combined. As of FY2018, PIF has one funding allocation.

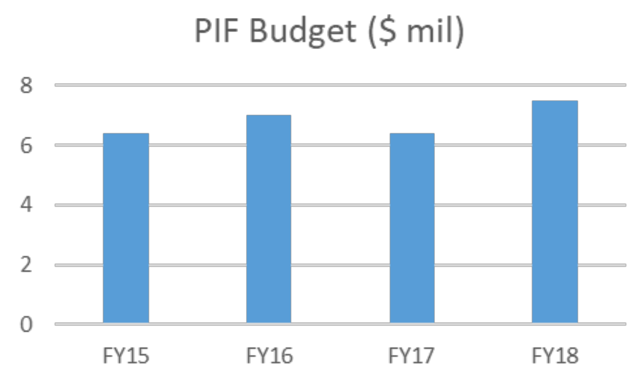


Figure 1. Total budget allocations to the Physics at the Informational Frontier program.

Review Process

The program management is excellent. The well thought-out review process involves multiple steps. After the review criteria are communicated to review panels and ad hoc reviewers, successful preproposals are invited to submit full proposals. Due to the cross cutting nature of the Physics at the Information Frontier Program, it is appropriate to gather proposals from multiple sources. One is a direct (Computational Physics) program solicitation, the other via referral from the other PHY program directors. The proposals are then reviewed in a consistent PIF process of mail in-reviews followed by a panel review of the proposals. In order to ensure comprehensive expertise on the panel, the PIF program director works closely with the other PHY program directors to select a team of qualified reviewers who can span all areas of expertise. The PHY program directors are invited to take place in the entire process, including observing the panel deliberations, discussing the ranking and participating in funding decisions.

In FY2015‑FY2016, 35 distinct proposals involving 50 institutions were considered by 14 panelists, leading to 8 awards involving 11 institutions.

In FY2017‑FY2018, PIF organized the PFCQC Ideas Lab with the participation of ECCS in ENG and CCF in CISE. In the first stage, 41 pre‑proposals from 27 institutions were received and evaluated by a Selection Virtual Panel composed of four panelists with expertise in theoretical and experimental physics, engineering, and computer science. As a result, in the second stage, 24 invitees from 19 institutions participated in the Ideas Lab Workshop held at the Santa Fe Institute. Seven mentors facilitated the workshop proceedings and the formation of four teams that developed distinct proposals. Three of the four teams were invited to submit full proposals. In the third stage, the three full proposals were reviewed by a panel of seven experts, resulting in one award.

The COV considered in detail 24 jackets, comprising 19 proposals submitted to the PIF: Computational Physics program solicitation or CDS&E programs, 3 Ideas Lab preliminary proposals, and 2 full Ideas Lab proposals.

The review analyses from the program director documented the rationale for the success or failure of the proposals. In some cases, the program directors decided to go against a panel recommendation. These decisions are well justified and provide sufficient evidence that the process was transparent. The program director made decisions that took into account a complex set of requirements, both scientific and financial. The treatment of conflict of interest situations was appropriate, with much care displayed in selecting reviewers during all steps of the process.

Intellectual Merit

The program Director, Bogdan Mihaila, is an outstanding physicist and provides intellectual leadership to strategy development in scientific research. PIF interacts with programs across the Division, Directorate, and the Foundation to promote inter‑disciplinary research that leads to cutting edge solutions to cyberinfrastructure challenges facing PHY communities. PIF is the PHY representative in a variety of NSF‑wide crosscutting activities related to computing, such as the CDS&E meta‑program, and the CSSI and CyberTraining programs in CISE. The program provides intellectual leadership in formulating the PHY/MPS/NSF response to national initiatives, such as the National Strategic Computing Initiative (NSCI), the National Quantum Initiative (NQI), and promotes the PHY community interests with respect to the Reproducibility and Replicability and the Public Access Policies for Federally Funded Research. PIF supports community‑building activities for computational and data‑enabled science.

Broader Impacts

PIF promotes the NSF goals of increasing and broadening participation and diversity. The program supports workshops and summer schools that facilitate the training and networking of junior researchers (students and postdocs) in computational methods on advanced computing architectures. High-performance computing and data analytics methods are introduced in the context of specific scientific applications relevant to the PHY communities. Lectures are accompanied by problem sessions and hands-on activities on the actual machines. Online sharing of workshop materials and recorded presentations on dedicated websites is strongly encouraged.

Recommendation of Prior COVs

The 2012 COV recommended that the designated PIF/CP representative is attached to one of the subcommittees. That recommendation was implemented in 2015 and the same model was followed this year, i.e. by establishing the PFC/PIF subcommittee.

Recommendations of the Current COV

We congratulate PIF for embracing and promoting the “convergence” approach of forming teams with expertise in physics, computer science, and mathematics to address critical scientific challenges facing the PHY community. We encourage them to continue to apply it to large-scale projects, e.g. the Scalable Cyberinfrastructure for Multi-Messenger Astrophysics (SCMMI).

We commend the PIF program for establishing a close working relationship with similar programs across NSF and in particular, with the Office of Advanced Cyberinfrastructure in CISE. This allows PIF to leverage resources and most importantly expertise at the cutting edge and the bleeding edge of computing. Such efforts should be amplified moving forward.

It is critical that PHY and NSF maintain the high professional quality of their program directors as thought leaders in Physics. We encourage PHY to continue to bring in outstanding scientists that have a profound understanding of the impact physics can make on science and society.

With the increasing number and level of programmatic activities, we trust that PHY will find the best approach to maintaining the intellectual curiosity and productivity of the Program Directors and avoid overloading them.

I. Physics of Living Systems

A. Definition of the Program

The Physics of Living Systems (PoLS) program supports a research portfolio where physics-based approaches are used to study the biology of living organisms. The program encourages projects that require the skills and training of physicists and that can often be carried out within physics departments, but, at the same time, have strong biological relevance. PoLS extensively collaborates with the MCB/BIO and IOS/BIO divisions of the NSF to identify grants that can be jointly considered and/or supported.

Each year, the PoLS program typically runs two panels to review individual proposals. The panels are divided by the scale of enquiry. One panel focuses on molecular to cellular phenomena, topics related to the MCB/BIO division of the NSF. The other panel focuses on systems, organismal, and collective phenomena, topics related to IOS/BIO. A common theme is that proposals must shed light on the unique properties of living systems, and not on properties shared by living and nonliving matter (e.g., elasticity, viscosity) that might be studied as Materials Science or Soft Condensed Matter.

The collaboration between MCB/BIO, IOS/BIO and PoLS runs both ways. Proposals made initially to the biological divisions with a strong physics component can be supported by PoLS and vice-versa. This has the positive effect of raising the quality of funded proposals. The best science submitted across the NSF is identified and supported, not the best applicants to a particular program.

Collaboration between PoLS and the BIO divisions extends to all mechanisms. Substantial support is given to CAREER grants for early investigators and NSF-wide initiatives with a biophysical theme (e.g., BRAIN Initiative, NeuroNex, Rules of Life). PoLS works closely with Program Directors from biological divisions to support these cross-cutting, highly interdisciplinary efforts.

Although not directly within the PoLS program, another significant success of the collaboration between the MCB/BIO, IOS/BIO and PHY divisions includes their support of several Physics Frontiers Centers that foster research and education at the interface between the life and physical sciences. The fact that three PFCs have biological themes attests to the successful collaboration between the PHY and BIO divisions.

PoLS actively looks for new frontiers where physicists might facilitate biological research. A notable example is the effort to identify and promote a "Physics of Cancer" to determine whether theoretical approaches, not common to modern cancer research, might be effective. Although the diversity of research projects is impressive, additional areas at the interface between the physical and life sciences might continue to be identified (see Suggestions below).

In the last few years, the PoLS has also successfully launched an innovative graduate student network. Several institutions in the USA (Harvard, Yale, Rice, UIUC, Georgia Tech, and others) have been funded to create networks of research collaboration driven by graduate students at these universities. In the first phase, most funding appears to have successfully fostered collaborations between labs at individual universities, but the future might involve more collaboration between institutions and with international partners (see Suggestions below).

B. Integrity and Efficiency of the Process and Management of the Program

1. <u>Effectiveness of the Review Process</u>

The program typically holds two review panels per year. Each panel composed of 12-15 members reviews ~50 single investigator grants. One panel focuses on molecular to cellular phenomena. The other focuses on systems, organisms, and collective behaviors.

Ad hoc external reviews are not solicited for single investigator grants. All of these grants are reviewed by 2-3 panelists. Diverse panelists are selected for the breadth of their biological expertise. A mix is sought of young investigators (often recent CAREER awardees) as well as more experienced members to achieve viewpoint diversity. Panelists are also frequently rotated on and off to bring fresh perspectives. The results of panel discussions are shared with Program Directors from the BIO divisions to better understand differences of opinion and to better understand how the PHY and BIO divisions can work together to encourage and fund the best science.

CAREER proposals are not decided in regular panels, but by direct consultation with Program Directors from MCB/BIO and IOS/BIO on the basis of external reviews. Because CAREER proposals tend to be particularly cross-cutting and innovative, they do not easily fall within the scope of the two regular panels. Also, the nature of CAREER proposals understandably makes them difficult to review along with regular proposals. The challenge of reviewing and considering CAREER proposals with integrity and high standards is met by recruiting expert external reviews for each submission and achieving agreement between multiple Program Directors from physics and biology.

The PoLS Graduate Student Network grants span all levels of biology, and each is reviewed by both panels. Thus, six reviews are collected, which guarantees rigor and depth. The activities of the PoLS Graduate Student Network are also regularly reviewed by Program Directors from the BIO divisions.

All representative jackets made available to the COV had between 2 and 3 written reviews by panel members. The panel discussion consisted of an initial classification of the proposals followed by a final classification into four categories: high priority, medium priority, low priority and non-competitive. The first and last of these contained the lowest numbers (sometimes none) of proposals. The decision to place a proposal into high priority was arrived at only after all the proposals had been thoroughly discussed.

Thorough analysis showed that all reviews were adequately detailed and clear, describing strengths and weaknesses in Intellectual Merit and Broader Impacts. Whether or not proposals were funded, reviews constituted useful feedback for applicants. The panel summaries were generally brief but conveyed the sense of the discussion and provided insight into the final decision. The Program Director synthesized review materials and factors that contributed to the final decision. This latter document was not released to the PIs, but the Program Director carefully considered whether future and sufficiently revised proposals might be funded. The Program Director played an admirably active role in helping scientists, particularly women and minorities, who had trouble getting initial grants find the collaborations and mentoring that might be needed to be successful in the future.

The intersection between the physical and life sciences is broad and occurs at all levels from molecular to cellular to systems to the behavior of individuals and collectives. It is impressive that this breadth is reflected in the funding portfolio without apparent compromise of quality in any area. All funded grants received robust support from the individual reviews of competent panelists and in the panel summaries.

In addition, the portfolio shows a diversity of theoretical and experimental approaches, an essential quality of the "physics" of living systems as opposed to more conventional biological approaches. The portfolio also has a distribution of scientists at all levels from early career scientists to senior investigators.

2. Broader Impacts

The portfolio succeeds in achieving geographical diversity of funded projects. The portfolio spans states and institutions in the Northeast, mid-Atlantic, South, Midwest, and West. Naturally, it is impossible to evenly cover most states, but the effort to obtain a geographical distribution is clear and successful.

In addition, several programs within the portfolio make an effort to create collaborative networks across these geographical areas. A mix of public and private research universities and collaborative networks that connect institutions at all levels will promote the dissemination of ideas and sharing of tools and technologies across the USA.

The CAREER investigators demonstrate a particularly rich diversity of creative and far-reaching outreach activities that will broaden participation of diverse groups at all levels. All single investigator grants have some elements of outreach and education, as well as the Physics Frontiers Centers that connect to biology.

A highlight (although not part of the PoLS program) is the PFC at Princeton and CUNY with a biological focus. This PFC makes an explicit link between an elite private university and a public university that serves a diverse population. This type of sharing and collaboration is important to build the broad and deep research community across the USA that will serve the national interest.

Another broader impact is to increase collaboration and cohesion of research networks across the USA and around the world. This is the explicit goal of the PoLS graduate student research network, a model of a Science Across Virtual Institutes (SAVI). Several research universities in the USA are currently funded (Harvard, Yale, Rice, Georgia Tech, and others). These institutions are encouraged to initiate research projects that involve the exchange of ideas and scientific collaboration. So far, the Graduate Student Networks have been successful in fostering collaboration between labs within universities or between nearby institutions (e.g., between Georgia Tech and Emory), but the ambition is to expand and grow these collaborative networks.

3. Portfolio of Awards and Management

The Program Director is responsible for the "living systems" theme of the division, and actively fosters new and innovative research at this interface. He spends considerable time encouraging collaboration and the exchange of ideas between biologists and physicists. These efforts have nucleated a number of successful research programs and collaborations.

The Program Director pays special attention to proposals from early investigators and underrepresented groups, and finds ways to encourage supporting these groups by strengthening their proposals by advice and collaboration from more established scientists. The success of this effort is evident in portfolio diversity.

The Program Director weights funding decisions toward "high-risk/high-reward", and is admirably willing to take risks in funding new areas that had not received attention, and not always funding safe and continuing research efforts.

The Program Director takes full advantage of NSF-wide initiatives. PoLS played an important role in the BRAIN Initiative by participating in an Idea Lab on Cracking the Olfactory Code. PoLS played a key role in the NeuroNexus program, and in Rules of Life, one of the "Big Ideas", by sponsoring an Ideas Lab on Synthetic Life.

The Program Director is deeply involved in the launch and successful management of the Graduate Student Network, by fostering collaboration with international partners, by frequent site visits, and attending annual meetings. He has high standards for the broader impacts and intellectual merit of the Graduate Student Network, and actively encourages participating institutions to keep up their efforts and improve their performance.

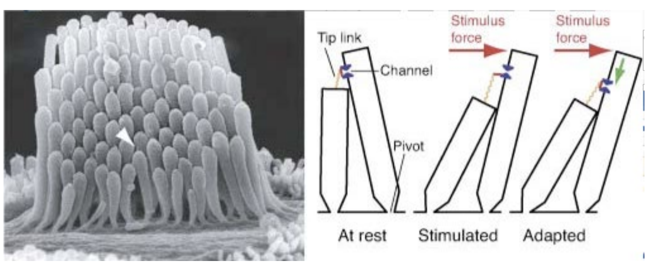
The Program Director is also singularly successful in bridging PoLS to the MCB/BIO and IOS/BIO divisions of the NSF. By encouraging and funding the best proposals across the physical and life sciences, the team of program directors across disciplines enhances the quality and amount of NSF funded science. Sharing support of single investigator projects that cut across the physical and life sciences has become a standard and admirable practice by this team. Also laudable is their explicit collaboration in reviewing and funding CAREER proposals. The three PFCs that connect to different areas of the life sciences testify to the collaboration between different NSF divisions.

C. Results of NSF Investments:

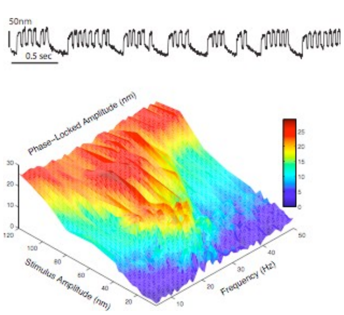
To illustrate the breadth of the PoLS portfolio, we highlight two funded projects.

<u>Project 1: Chaotic dynamics of inner ear hair cells, Dolores Bozovic, PI</u>

Nonlinear processes and active amplification have been shown to be key to the extreme sensitivity of audition. While many studies have explored the various potential mechanisms of amplification, the role of either deterministic or stochastic noise has received comparatively little attention. Hair cells of the inner ear are the biological sensors that detect displacements induced by air-borne or ground-borne vibrations and transduce them into electrical signals. Their responsiveness is crucially dependent on an active process that amplifies oscillations induced by the incoming sound. One of the signatures of the active process, hair cell bundles have been shown to exhibit limit cycle oscillations, spontaneous motion in the absence of any input. The PI hypothesizes that the innate motility exhibits chaotic behavior, and proposes to test how chaos impacts the sensitivity of detection.



Left: SEM image of a hair bundle. Right: Schematic diagram of two stereocilia connected by a tip link. Deflection of the bundle tenses the tip link, and opens the transduction channels.



Top: spontaneous hair bundle oscillation.
Bottom: Phase-locked component of the response. The bundles were deflected from 5-50 Hz, in 1 Hz increments. Stimulus was applied from 4-120 nm

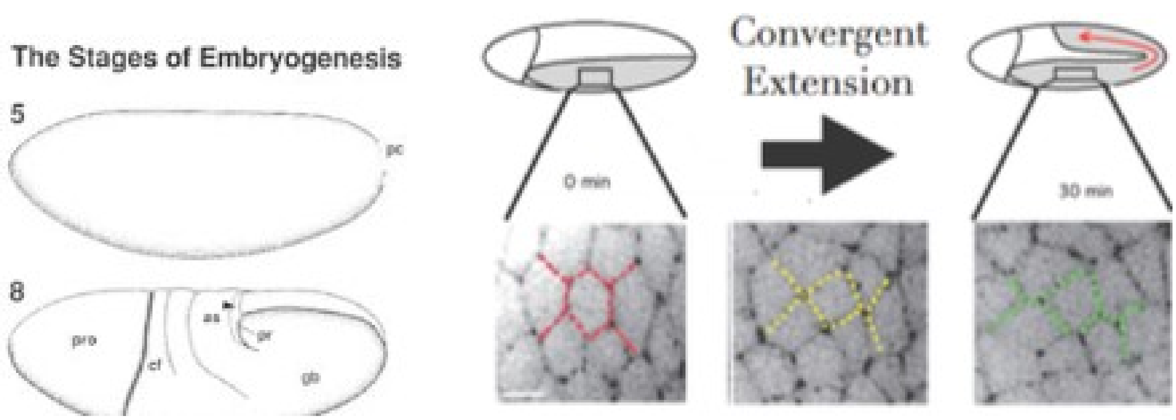
This project is exemplary in combining experiment and theory to explore the effects of chaos and noise on the sensitivity of detection by the auditory system. This study should have fundamental impacts in biomechanics and cellular modeling, but also on sensory neuroscience and the mechanisms of vertebrate hearing.

This project (PHY 1705139) from Dolores Bozovic of the University of California-Los Angeles was jointly funded by the Program of Physics of Living Systems in the Division of Physics, and the Neural Systems Cluster in the Division of IOS/BIO.

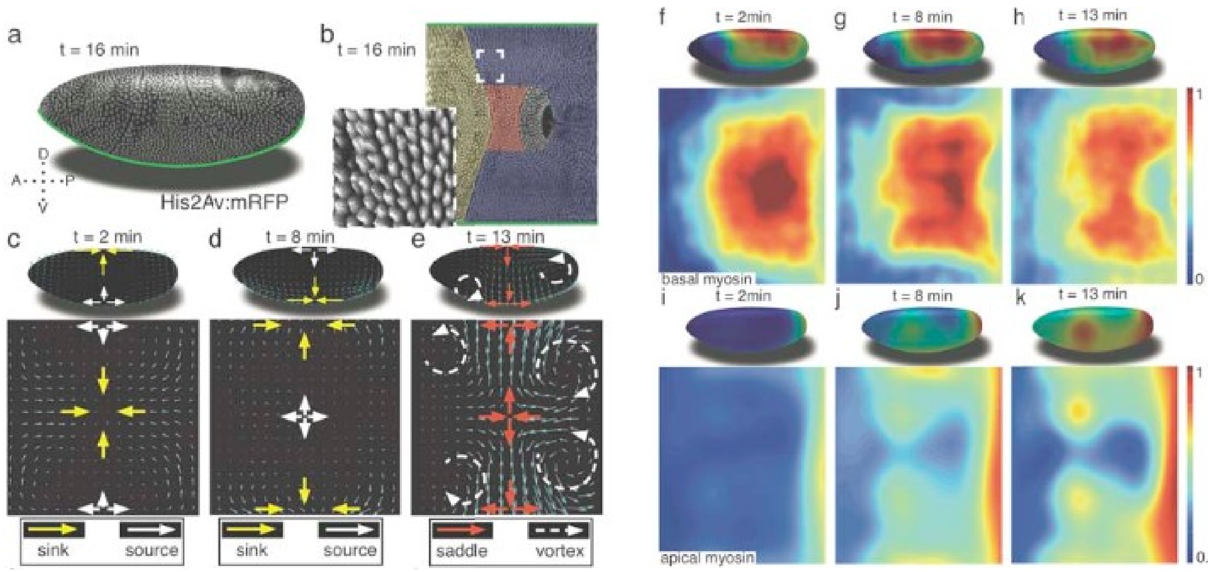
The PI was originally trained as a physicist (BA and PhD) in condensed matter physics before becoming a biophysicist/neuroscientist as a postdoc and independent investigator. Her successful career trajectory is paradigmatic of the next generation of researchers at the interface between the physical and life sciences. These career trajectories should continue to be encouraged and supported.

<u>Project 2: Mechanics and Mechanisms of Morphogenesis, Boris Shraiman, PI</u>

Morphogenesis, which is the process by which multi-cellular organisms acquire their "shape", is the ultimate challenge in the study of pattern formation. Morphogenesis executes a genetically encoded developmental program and much of the prior work has been focused on the pattern of gene expression and the molecular signals that control it. Yet because morphogenesis involves spatial rearrangement and flow of cells, physical interaction of cells within tissue plays an important role. This project will address the role of mechanical stress in coordinating cellular and sub-cellular processes with the global organization and dynamics of tissues. The proposed work will develop the Active Solid Model (ASM) of epithelial tissue mechanics describing its fluid-like ability to rearrange that coexists with the ability to support steady external stress.



Early morphogenetic flows in fly embryo rearrange the initial monolayer of cells to form the head region, internalize the ventral mesoderm (not visible in the figure) and generate germ-band extension (red arrow). The latter is often explained by convergent extension in the lateral ectoderm (here illustrated by rectangles) associated with anisotropic local rearrangement of cells via T1 processes (red to yellow and green).



SPIM imaging of morphogenetic flow (a-e) and of the basal (f-g) and apical (i-k) myosin distributions at 3 different times after cephalic furrow formation. Panels c-d) present velocity fields measured by PIV analysis and illustrate changes in the topology of the morphogenetic flow.

The model developed by the PI suggests that the anisotropy of myosin distribution, which is central to generation of convergent extension flow, may be a consequence of bi-stability arising from mechanical feedback. In this scenario, the effect of pair-rule gene expression boundaries is limited to providing an orientational bias to the mechanical feedback-driven symmetry breaking instability.

This project (PHY 1707973) from Boris Shraiman, an established investigator at UC Santa Barbara, was funded by the Physics of Living Systems, Division of Physics.

Before becoming interested in biological systems, the PI had a long and successful career in condensed matter theory. Enabling established investigators with powerful tools developed in the physical sciences to make inroads into the life sciences is highly laudable.

D. Summary, Comments, and Recommendations

The program fits the mission of the NSF in identifying and supporting our national priorities to innovate in cross-cutting research that will best serve the training needs of future generations.

1. The number of grants, success rate, and typical grant size has been roughly constant over the last decade. The quality of funded research and investigators has been consistently high. A number of funded researchers have become the

leaders of their fields. However, the costs of research have significantly risen whereas the amount of funding has not. Graduate student and postdoctoral salaries have increased substantially, and the costs of many experimental programs have increased with technology. Consequently, the impact of each award is diminished. Funding awards should, at the very least, keep up with inflation.

2. One gap is the lack of independent funding mechanisms for postdocs, which might have special relevance for the Physics of Living Systems. Individuals with PhDs in the physical sciences who want to switch to the life sciences will have a hard time getting postdoctoral fellowships from the NIH or other funding mechanisms that require substantial prior research experience in any subdiscipline. A robust program to fund postdocs, and even investigators at later stages of their careers, who want time and flexibility to pioneer new directions in the physics of living systems, would fill an important gap. We do not recommend reducing the size of any of the current programs to fund postdocs, but strongly recommend budget increases to allow the inclusion of such a funding program.

3. Missing areas of research in the portfolio include life in extreme environments, e.g., the possibility of detecting life on other planets. Life in extreme environments is an area of research that has been pursued at the biological and biochemical levels, but so far not with a view to physical principles. The Program Director should be vigilant for other such areas, and receive adequate support from the NSF to travel and interact with the broader scientific community to identify new opportunities.

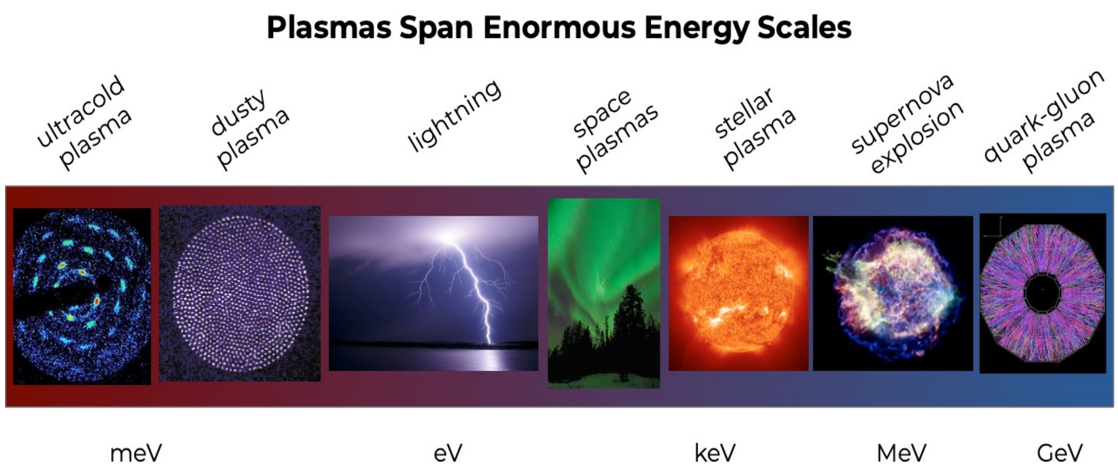
4. The PoLS Graduate Student Network is now launched with nodes at several major research universities. However, its continued success and relevance will require increased evidence of actual collaboration between nodes and/or broader impacts where the significant funding given to each node translates into expanded participation of networks of scientists at more institutions. Significant attention to fostering networks that expand outside the confines of each funded node will be required to argue for continuation or enlargement of this program.

J. Plasma Physics and Accelerator Science

Introduction

Plasma Physics is a study of matter and physical systems whose intrinsic properties are governed by collective interactions of large ensembles of free charged particles, for which self-consistent electromagnetic as well as (possibly) other fundamental forces play a fundamental role. For example, nearly all of the visible Universe is thought to consist of plasmas (e.g., stars). Yet, many dynamical aspects of the closest star to us (the Sun) remain poorly understood: the heating mechanism(s) of the solar corona, the nature of the 22-year solar magnetic reversal cycle, and the genesis of coronal mass ejections.

The underlying nonlinear physics of the collective plasma behavior makes plasmas ideal paradigms in the study of complex systems. Plasma physics has applications to space physics and astrophysics, materials science, applied mathematics and mathematical physics, magnetic & inertial fusion science, accelerator science (e.g., plasma-based accelerators), and many branches of engineering. In addition, one of the most spectacular features of laboratory and natural plasma systems involve the enormous energy scales they span: from ultracold plasmas at the meV scale, lightning discharges at the eV scale, to thermonuclear burning plasmas at the keV scale and extreme astrophysical events (e.g., supernova explosions) at the MeV scale and beyond.



The Plasma Physics program supports research that can be categorized by several broad, sometimes overlapping, sub-areas of the discipline, including: magnetized plasmas in the laboratory, space, and astrophysical environments; high energy density plasmas; low temperature plasmas; dusty, ultra-cold, and otherwise strongly coupled plasmas; non-neutral plasmas; and intense field-matter interaction in plasmas. The focus of the Plasma Physics program is to generate an understanding of the fundamental principles governing the physical behavior of a plasma via collective interactions of large ensembles of free charged particles, as well as to improve the basic understanding of the plasma state as needed for other areas or disciplines of science and engineering.

Some Plasma Physics-related activities are supported primarily by other NSF Programs. For example, proposals focused on the physical properties of individual or a small number of atoms or molecules, or optical physics, should be directed to the Atomic, Molecular, and Optical Physics Program within the Division of Physics. Proposals focused on understanding astrophysical systems should be directed to the Division of Astronomical Sciences. Proposals focused on understanding the Geospace environment or the Sun-Earth interactions should be directed to an appropriate program within the Geospace Section of the Division of Atmospheric and Geospace Sciences. Proposals focused on the development of new materials using plasmas should be directed to an appropriate program in the Division of Materials Research. Proposal focused on plasma-assisted manufacturing should be directed to the Division of Civil, Mechanical and Manufacturing Innovation. Finally, proposals focused on the use of plasmas for environmental and reaction engineering, environmental sustainability, combustion systems, or engineering of biomedical systems should be directed to an appropriate program within the Division of Chemical, Bioengineering, Environmental and Transport systems.

Plasma Physics Program and the NSF/DOE Partnership in Basic Plasma Science and Engineering

The majority of individual investigator proposals considered by the PHY Plasma Physics program are submitted to the NSF/DOE Partnership in Basic Plasma Science and Engineering solicitation, The National Science Foundation (NSF), with the participation of the Directorates for Engineering, Geosciences, and Mathematical and Physical Sciences, and the Department of Energy, Office of Science, Fusion Energy Sciences (DOE/SC/FES) have continued the joint Partnership in Basic Plasma Science and Engineering begun in FY1997 and renewed several times since.

As stated in the original solicitation (NSF 97-39), the goal of the Partnership is to enhance basic plasma science research and education in this broad, multidisciplinary field by coordinating efforts and combining resources of the two agencies. The current solicitation also encourages submission of proposals to perform basic plasma experiments at NSF and DOE supported user facilities, including facilities located at DOE national laboratories, designed to serve the needs of the broader plasma science and engineering community.

The NSF/DOE Partnership program in Plasma Physics is a unique program among the NSF divisions. During the FY15-FY18 period, the NSF contributed between $5M and $8M to the Partnership on the annual basis, matched approximately equally by DOE. The major research areas are low-temperature, non-neutral and dusty plasmas; turbulence and magnetic reconnection in laboratory and space plasmas; laser-plasma interactions; and high energy density plasmas. The NSF program emphasizes graduate education integrated within the research programs, and excludes research directly related to fusion plasmas. The Partnership funding is critical for the viability of discovery-based plasma research as a distinct area of intellectual inquiry within Physics, and for the training of the next generation of plasma physicists. The bulk of the funding is for single-PI research programs, with the exception of continuing shared support of $1.7M/yr for the Basic Plasma Science (user) Facility at UCLA.

We note that, in parallel with the current NSF/PHY COV, the DOE/FES program also undergoes regular COV reviews, which are submitted to the Fusion Energy Sciences Advisory Committee (https://science.osti.gov//media/fes/fesac/pdf/2018/FESAC_COV_Report_2018.pdf?la=en&hash=36C190517213645669265D91C0C96FE7DD3D98B2 for the latest COV conducted in 2018). Since the NSF/DOE Plasma Partnership proposal review is managed by NSF/PHY, the review of the Partnership is the responsibility of this COV.

Connections with NASA in the area of Plasma Physics have also generated co-funding opportunities. Because of its breadth of coverage, connections within the NSF Physics Division have also been fostered (e.g., with Particle Astrophysics).

During a workshop held in 2017 in celebration of the 20th anniversary of the Partnership, several observations were made in support of the NSF/DOE Partnership:

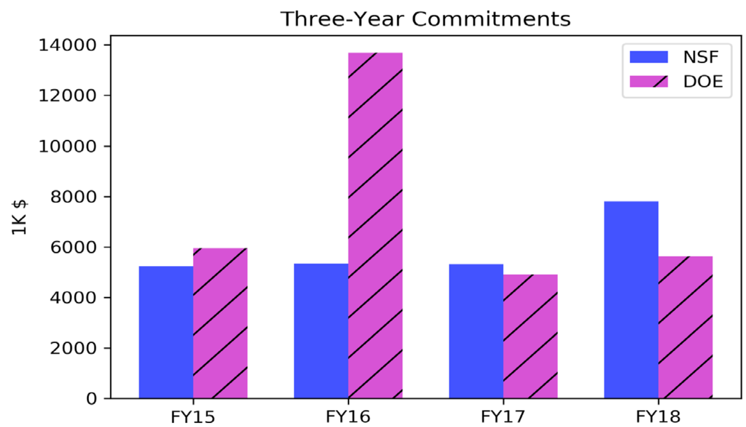
- The Partnership offers PIs an opportunity to focus on basic plasma science, whether in the form of theory or experiment. This emphasis allows PIs to explore scientific questions of fundamental importance and long-term impact, without having to respond to a specific agency or programmatic mandate.
- Similarly, the Partnership allows PIs a degree of intellectual flexibility. PIs are given the freedom to pursue research topics and to follow their scientific curiosity. It was remarked that the flexibility offered by the Partnership enabled the development of tools and techniques (e.g., in accelerator physics and low-temperature plasmas) that have led to new technologies that may have broad societal impact.
- The Partnership has had a significant impact on the training of the next generation of plasma scientists, including the funding support of both undergraduate and graduate students in their research.
- The Partnership has contributed to building a broad basic plasma science community that cuts across many different disciplines and impacts many fields.

The NSF/DOE MOU was renewed and the NSF/DOE Plasma Partnership solicitation was revised in FY16 to clarify and emphasize the physics-centric nature of the Partnership. While the MOU of the Partnership is scheduled to be renegotiated in two years, we note that the NSF/PHY Plasma Physics program exists independently of the Partnership. Hence, in the event of the dissolution of the Partnership, it is recommended that the NSF Plasma Physics program continue soliciting individual investigator proposals on an annual basis as any other PHY programs.

Statistics for the NSF/DOE Plasma Partnership for the FY15-FY18 Period

Each project/proposal in Plasma Physics considered by the NSF/DOE Partnership received a rigorous review, involving *ad-hoc* reviews (for depth) and at least 3 panel reviews (for breadth), which were discussed in one of three separate review panels (five panels in FY15) – Magnetized Plasmas, Low-Temperature Plasmas & Plasma-Material Interactions, and High-Energy-Density & Laser-Plasma Interactions – organized and managed by NSF, with input from and attendance by DOE/SC/FES Program Managers. In each review panel, the projects and proposals were ranked in one of four categories: Must Fund (MF), Fund if at all Possible (FIP), Funds if Funds Available (FIFA), and Do Not Fund (DNF).

Of the ranked MF and FIP projects and proposals in Plasma Physics during the FY15-FY18 period, 108 (21/34/24/29) projects and 129 (29/38/30/32) proposals received funding either in full or in part, which accounts for approximately 30% of the total number of proposals and projects. The total 3-year award commitments by the NSF and DOE/FES within the Partnership were, respectively, \$5,232k and \$5,945k in FY15, \$5,322k and \$13,686k in FY16, \$5,319k and \$4,907k in FY17, and \$7,811k and \$5,633k in FY18. The surge in DOE/SC/FES funding in FY16 was due to a one-time funding reassignment within DOE/FES. If we omit the award data for FY16, the subpanel was glad to see the share of the NSF funding through the NSF/DOE Partnership in Plasma Physics steadily rising.



A brief discussion took place among the members of the 2019 COV Plasma Physics subpanel with the Program Director regarding the possibility of accepting Plasma Physics proposals for a longer period (e.g., 5-year proposals). In general, we agreed that, while the 3-year proposal period should be standard, the Program Director should be given some flexibility in considering longer research proposal periods.

Accelerator Science Program

Following one recommendation included in the 2015 COV for the Accelerator Science program, the present 2019 COV Plasma Physics subpanel was charged to evaluate the review process for projects and proposals that were submitted to the Accelerator Science program during the FY15-FY17 period. The Accelerator Science program, which was initiated in FY14, supports studies of the basic physics associated with the unidirectional acceleration of non-zero mass particles. Some of the research topics covered by the Accelerator Science program include:

- Modeling and experimental control of beam dynamics
- Understanding and optimization of the initial distribution function for accelerator particle sources
- Novel methods for maximizing energy gain and/or minimizing angular spread and/or maximizing bunching of accelerated particle beams for a range of applications from high energy and nuclear physics, to medical applications, and to light sources
- New ideas for compact particle accelerators, including plasma-based acceleration.
- The study of materials properties of accelerating cavities
- Novel diagnostics for beams and particle sources

The scope of the Accelerator Science program supports and fosters research that exploits the educational and discovery potential of basic accelerator physics research at academic institutions. A key goal of the program is to seed and develop research efforts in fundamental accelerator science at colleges and universities that will enable transformational discoveries in this cross-cutting academic discipline.

Statistics for the Accelerator Science program for the FY15-FY17 Period

The Accelerator Science program received proposals in response to Physics Division-wide annual solicitations during the FY15-FY17 period. Each project/proposal considered by the Accelerator Science program received a rigorous review, involving *ad-hoc* reviews (for depth) and at least 3 panel reviews (for breadth) discussed during a single review panel. Of the ranked MF and FIP projects in Accelerator Science during the FY15-FY17 period, 36 (16/10/10) projects received funding either in full or in part: the total 3-year award commitments by the Accelerator Science program were $6,706k in FY15, $4,458k in FY16, and $5,605k in FY17, although nearly half of the FY17 award funds went to a single institution.

The demise of the Accelerator Science program in FY18

Due to decreased proposal pressure observed during the FY15-FY17 period, the Accelerator Science (AS) program within the NSF's Physics Division stopped soliciting proposals in FY18 (only one research award, a CAREER award, was made and a number of one-year supplement awards were made to PIs whose awards were due to expire with the goal of helping graduate students already working on NSF-funded projects).

A brief postmortem of this decision follows. First, we note that the AS program's initiation in FY14 coincided with the initiation of related Accelerator Stewardship program within DOE/SC/HEP. The NSF PHY/AS program and the DOE/SC/HEP Accelerator Stewardship program, as well as DOE/SC/HEP's General Accelerator R&D (GARD) program, have maintained close informal coordination. Second, there are significant overlaps between the NSF/PHY/AS program and the DOE/SC/HEP programs, which have comparative reviews and fund proposals through regular calls. In addition, DOE/SC/HEP also has a "general accelerator research R&D" (GARD) program, which supports accelerator science R&D for high energy physics applications at universities and national labs (BNL, Argonne, FermiLab, LBNL, SLAC).

Could we perhaps see a joint NSF/DOE AS Partnership?

The 2019 COV subpanel observed definite overlaps between the NSF Accelerator Science program with the DOE/SC/HEP portfolios. Unlike for the NSF/DOE partnership in plasma science, where only FES is involved, there are many possible overlaps (e.g., Accelerator Stewardship and General Accelerator R&D).

In view of the overlaps with several DOE/SC offices (HEP for plasma-based acceleration, including laser and dielectric acceleration, and BES for new electron sources for applications such as light sources), the subpanel recommends that a new partnership between NSF/PHY and DOE/SC/HEP be explored for the Accelerator Science program. This would allow coverage of all areas of accelerator science (conventional sources and advanced concepts), and a strong connection between national lab programs and universities.

Integrity and efficiency of the program review process and management

The Plasma Physics and Accelerator Science 2019 COV subpanel has reviewed the quality and effectiveness of the merit review process for each program. The subpanel was very pleased with the quality and excellence of the reviewing process. This is primarily due to the fact that the Plasma Physics Program Director maintains an extensive list of potential reviewers with a record of their general expertise and a history of their service to the program. The selection of reviewers for write-in *ad-hoc* reviews and in-person panel reviews is made on the basis of expertise for *ad-hoc* reviews and breadth of knowledge for panel reviews.

The subpanel was pleased with the detailed records kept by the PD in the area of Education, Outreach, and Diversity. In particular, while Plasma Physics continues to be deficient in the area of diversity (in terms of women and underrepresented minorities), it was reassuring to see how the selection of reviewers showed a remarkable participation rate from women and underrepresented minorities, from 17.5% (with 5 review panels in FY15), to 35% (FY16), 32% (FY17), and 45% (FY18) with 3 panels in each year.

COV review of jackets

As part of the 2019 COV subpanel review, the jackets of 24 proposals in the Plasma Physics program and 12 proposals in the Accelerator Science program were reviewed. The 2019 COV subpanel was tremendously impressed with the wide variety of research topics covered by even such a small sample of projects and proposals in Plasma Physics. Given the broad range of research topics in Plasma Physics, there was a strong support among the 2019 COV subpanel for the separation of the Plasma Physics program from the AMO program, which were previously combined until after the 2015 COV. In addition, the 2019 COV subpanel was supportive of the division of the review panels in Plasma Physics into three sub-panels dealing separately with (1) magnetized plasmas, (2) low-temperature plasmas & plasma material interactions, and (3) high-energy-density plasmas & laser-plasma interactions.

The total number of projects and proposals considered by the NSF/DOE Partnership during the FY15-FY18 showed a definite reduction compared to the numbers reported in the 2015 COV (from an average of 144 proposals per year during 3-year FY12-FY14 period down to an average of 107 proposals per year for the 4-year FY15-FY18 period). It is noted that some of the reduction is attributable to the stricter than previous enforcement of NSF proposal compliance rules, resulting in several proposals being returned without review each year. The immediate consequence of this reduction, combined with increased support for the field within NSF, has had the positive effect of yielding a rise in the funding rate from its lowest level at 10% in FY14 (well below the PHY Division average) to a healthy level averaging 30% during the FY15-FY18 period. Lastly, the subpanel found ample evidence for the impact of a permanent NSF/PHY Program Director for Plasma Physics on the stability and the constant high standards for the review process.

Research highlights

A small sample of proposals funded either by the NSF Accelerator Science program (Arefiev and Cousineau) or the NSF Plasma Physics program (Baalrud and Loureiro) gives a measure of the wide range of topics supported by the Accelerator Science and Plasma Physics programs.

New Frontiers of Direct Laser Acceleration in Megatesla Magnetic Fields
(Alexey Arefiev, University of California at San Diego, NSF PHY-1632777)

A high-intensity laser beam propagating through a dense plasma drives a strong current that robustly sustains a strong quasi-static Mega Tesla-level azimuthal magnetic field. The transverse laser field efficiently accelerates electrons in the presence of such a field that confines the transverse motion and deflects the electrons in the forward direction, establishing the novel forward-sliding swing acceleration mechanism. Its advantage is a threshold (rather than resonant) behavior, accelerating electrons to high energies for sufficiently strong laser-driven currents. Analytical predictions are confirmed by numerical simulations, indicating Mega ampere-level threshold currents and energy gains two orders of magnitude higher than achievable without the magnetic field.

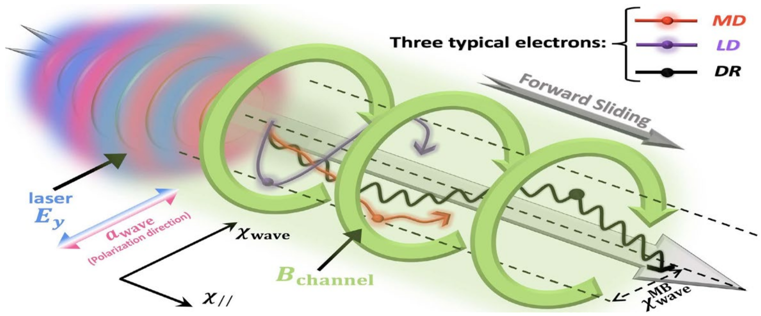
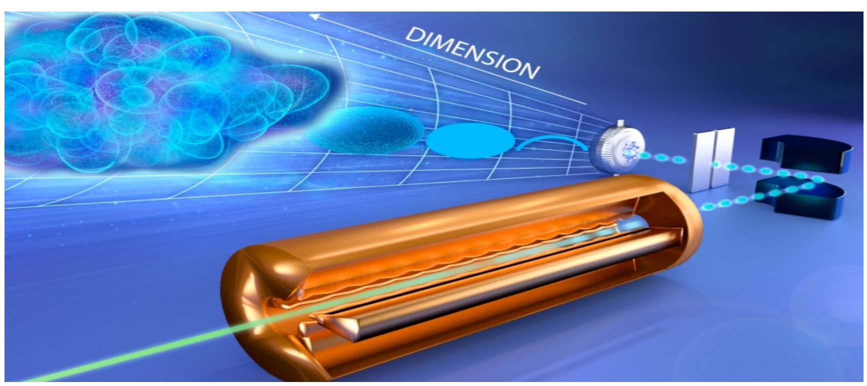


Figure 1 from *Forward sliding-swing acceleration: electron acceleration by high-intensity lasers in strong magnetic fields* (https://arxiv.org/abs/1811.00425).

First Six Dimensional Phase Space Measurement of an Accelerator Beam
(Sarah Cousineau, University of Tennessee, NSF PHY-1535312)

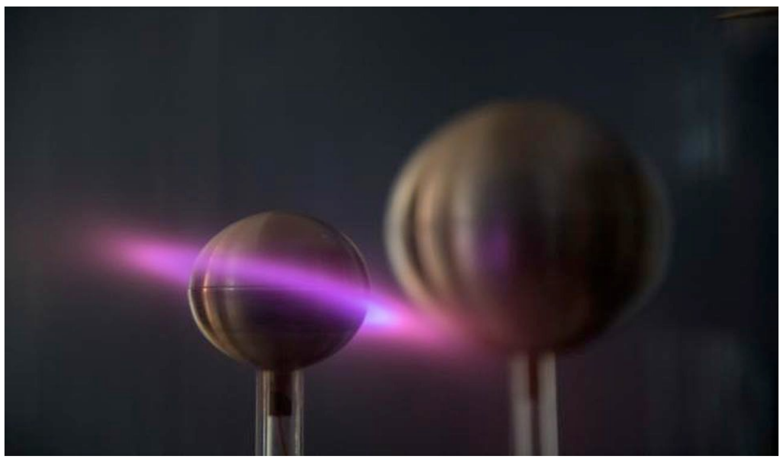
A particle beam in an accelerator is completely described by six independent degrees of freedom (three physical coordinates x,y,z, and their conjugate momenta), e.g, by a 6D phase space.This project produced the first ever complete and direct measurement of the 6D phase space of a beam in an accelerator. Prior to this work, the best measurements were only up to 4D. The 6D measurement revealed hidden structure in the phase space distribu\on of the beam that is only visible in measurements 5D or higher; any lower dimensionality measurement integrates out the structure. These results provide accurate initial beam conditions for particle beam simulations, which have historically been limited in accuracy due to not having this information. A large increase in simulation accuracy is required to advance the field of particle accelerators to the next decade of beam power capability.



The artistic representation illustrates a measurement of a beam in a particle accelerator, demonstrating the beam's structural complexity increases when measured in progressively higher dimensions. Each increase in dimension reveals information that was previously hidden. *(https://www.ornl.gov/news/ut-ornl-team-makes-first-particle-accelerator-beam-measurement-six-dimensions)*

The Barkas Effect in Plasma Transport
(Scott Baalrud, University of Iowa, NSF PHY-1453736)

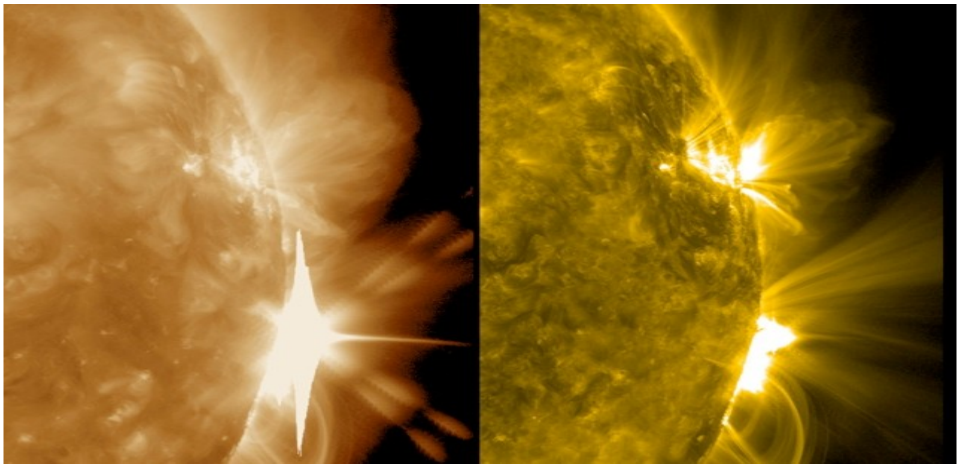
Symmetries are fundamental principles in physics. A symmetry underlying the theory of hot dilute plasmas is that the rates of transport (such as diffusion and electrical conductivity) are independent of the signs of the interacting charges. This means that identical rates are predicted if the plasma is considered to consist of positrons and ions rather than electrons and ions. This research showed that this charge-sign symmetry is broken as the plasma reaches a cold or dense state called a strongly coupled plasma. Theory predicted that the opposite sign of electron and ion charges significantly increases the rate of collisions. This prediction was verified using molecular dynamics simulations. The results influence strongly coupled plasmas in a variety of circumstances, including ultracold plasmas and dense plasmas found in dense astrophysical objects such as white dwarf stars and the interior of giant gas planets.



University of Iowa students have recreated in miniature the Earth's auroras by building a planeterrella, a rare model that teaches students and visitors about the science behind these phenomena. Photo by Tim Schoon. (https://www.forbes.com/sites/drsarahbond/2017/02/27/3d-modeling-the-universe-and-the-polar-lights-from-aristotle-to-iowa/#256f32ecb637)

This award supported development and construction of the Iowa Planeterella, which is a hands-on demonstration of plasma phenomena associated with the Sun-Earth interaction. Several demonstration modes are possible, including aurora, Van Allen radiation belts, and ring currents. The device was constructed by graduate students, and is used primarily by undergraduate and graduate students in outreach activities. It has been featured in a museum exhibit at the Flagship University of Iowa building, the USA Science and Engineering Festival, and two plasma physics expos at the annual APS Division of Plasma Physics meetings.

Magnetogenesis Revisited: The First Self-consistent Plasma Dynamo
(Nuno Loureiro, MIT, NSF CAREER 1654168)



Magnetic reconnection is a complicated phenomenon that Nuno Loureiro, an associate professor of nuclear science and engineering and of physics at MIT, has been studying in detail for more than a decade. To explain the process, he gives a well-studied example: "If you watch a video of a solar flare" as it arches outward and then collapses back onto the sun's surface, "that's magnetic reconnection in action. It's something that happens on the surface of the sun that leads to explosive releases of energy." Loureiro's understanding of this process of magnetic reconnection has provided the basis for the new analysis that can now explain some aspects of turbulence in plasmas. (http://news.mit.edu/2017/study-uncovers-new-mechanisms-astrophysical-plasma-turbulence-1201)

Summary of COV Subpanel Review and Recommendations

As the Frontiers of Physics keep expanding, the range of research topics in which Plasma Physics can play a pivotal role also keeps expanding. As a result of the enormous range in energy spanned by plasmas (from meV energies associated with ultra-cold plasmas to highly-relativistic plasma physics events studied in laboratories and extreme astrophysical events at the GeV energy scale and beyond), the 2010 Decadal Survey of Plasma Science [Plasma Science: Advancing Knowledge in the National Interest (2007)], carried out by the National Academies of Sciences, Engineering, and Medicine (NASEM), revealed a vigorous field that has an ever-increasing wide-ranging impact of society.

As a result of the 2019 COV review of the programs in Plasma Physics and Accelerator Science, the subpanel wishes to make the following recommendations, listed here in no particular order:

1. The presence of a permanent Program Director for the Plasma Physics program provides great stability and a long-term vision to the program. The subpanel strongly recommends that the presence of a permanent Program Director for Plasma Physics be maintained.
2. The averaged funding of 30% in Plasma Physics provides ample evidence of the rigorous standards applied by the Program Director. The subpanel recommends that the high reviewing standards applied by the Program Director be maintained.
3. The subpanel was encouraged to see that the NSF funding contributions to the NSF/DOE Partnership in Basic Plasma Science and Engineering appear to be increasing relative to the DOE/SC/FES contribution, which speaks to the continued interest from the plasma physics community in carrying research activities that focus on high-quality research in fundamental theoretical, computational, and experimental plasma physics. The subpanel recommends that the NSF PHY division continue its vigorous support of Plasma Physics.

4. The Plasma Physics program appears in the list of NSF programs connected with the Windows on the Universe (WoU) Big Idea program, which involves the Astronomical Sciences and Physics divisions (within the MPS Directorate) and the Office of Polar Programs (within the GEO Directorate). The subpanel recommends that the participation of Plasma Physics in the WoU program be supported aggressively by promoting it within the plasma physics community.
5. Among the strategic opportunities associated with the Harnessing the Data Revolution (HDR) Big Idea program, two topics intimately connected to the Plasma Physics program (matter at the high energy and intensity frontier, and space weather prediction) are cited as topics that will advance as a result of harnessing the data revolution. Plasma Physics continues to play a pioneering role in contributing to the development of high-performance computational methods and advanced diagnostics in physical space and higher-dimensional phase space. The subpanel recommends that the NSF Plasma Physics program seek ways to become an integral part of the HDR Big Idea program.
6. Since the technological limits of traditional particle accelerators are in plain sight, the development of advanced plasma-based accelerators is a crucial component of the NSF research portfolio in Plasma Physics. Although it is a cutting-edge physics field on its own, plasma-based accelerators have applications (novel light sources, colliders) of tremendous benefit for many other disciplines (material science, high energy density science, high energy physics, medical imaging and diagnostics, non-destructive imaging, homeland security). In view of the overlaps with several DOE/SC offices (HEP for plasma-based acceleration, including laser and dielectric acceleration, and BES for new electron sources for applications such as light sources), the subpanel recommends that a new partnership between NSF/PHY and DOE/SC/HEP be explored for the Accelerator Science program. This would allow coverage of all areas of accelerator science (conventional sources and advanced concepts), and a strong connection between national lab programs and universities.
7. The subpanel recommends that the NSF Plasma Physics program continue its support and promotion of the NSF Mid-Scale Research Infrastructure program within the plasma physics community.

VI. Appendices

Appendix A: Template Response

INTEGRITY AND EFFICIENCY OF THE PROGRAM'S PROCESSES AND MANAGEMENT

Briefly discuss and provide comments for *each* relevant aspect of the program's review process and management. Comments should be based on a review of proposal actions (awards, declinations, and withdrawals) that were *completed within the past three fiscal years*. Provide comments for *each* program being reviewed and for those questions that are relevant to the program(s) under review. Quantitative information may be required for some questions. Constructive comments noting areas in need of improvement are encouraged.

I. Questions about the quality and effectiveness of the program's use of merit review process. Please answer the following questions about the effectiveness of the merit review process and provide comments or concerns in the space below the question**.**

QUALITY AND EFFECTIVENESS OF MERIT REVIEW PROCESS	YES, NO, DATA NOT AVAILABLE, or NOT APPLICABLE
1. Are the review methods (for example, panel, ad hoc, site visits) appropriate? Comments: The COV was very pleased with the methods the Physics Division uses for merit review of research proposals. In most cases it is a combination of ad hoc reviews by several experts, panel reviews by other experts, and finally an informed judgment by the relevant Program Director(s). In special cases, for example Physics Frontier Centers, a somewhat different combination may be used, including site visits. In all cases it appears that proposals receive very thorough, fair, and consistent reviews. We hope that the Physics Division will continue to have the resources to provide this commendable merit review process.	YES

2. Are both merit review criteria addressed a) In individual reviews? b) In panel summaries? c) In Program Director review analyses? Comments: The COV found that in general both merit review criteria (Intellectual Merit and Broader Impact) were considered in individual reviews, panel summaries, and Program Director reviews analyses. There were some cases in which an individual reviewer probably should have paid more attention to the Broader Impact criterion, but they were relatively rare and it did not seem to have impacted the final decision.	 YES YES YES
3. Do the individual reviewers giving written reviews provide substantive comments to explain their assessment of the proposals? Comments: While there was some variation in the degree to which individual reviewers provided explanations of their assessment of the proposals, most reviewers in fact provided extensive and substantive explanations of their assessments. We are sure that this was very useful to the proposers and to the Program Directors.	YES

4. Do the panel summaries provide the rationale for the panel consensus (or reasons consensus was not reached)? Comments: The panel summaries were often concise, but did certainly provide appropriate rationale for the panel consensus. Panels rarely if every failed to reach consensus.	YES
5. Does the documentation in the jacket provide the rationale for the award/decline decision? [Note: Documentation in the jacket usually includes a context statement, individual reviews, panel summary (if applicable), site visit reports (if applicable), program Director review analysis, and staff diary notes.] Comments: The documentation in the jackets was very complete, including all of the appropriate elements. Most impressive typically was the analysis by the Program Director, which clearly took into account all relevant information. In the cases where the Program Director disagreed with the recommendation of a reviewer or panel, the documentation was important and compelling.	YES

6. Does the documentation to the PI provide the rationale for the award/decline decision? [Note: Documentation to PI usually includes context statement, individual reviews, panel summary (if applicable), site visit reports (if applicable), and, if not otherwise provided in the panel summary, an explanation from the program Director (written in the PD Comments field or emailed with a copy in the jacket, or telephoned with a diary note in the jacket) of the basis for a declination.] Comments: The documentation provided to the PI is in general very informative and complete. In the case of a declination the Program Director often provides perspective on what might be done differently next time. The Program Directors are also available to have additional discussions with the PI if it is requested.	YES

7. Additional comments on the quality and effectiveness of the program's use of merit review process: Comments: The merit review process in the Physics Division is very well respected in the community. A critical component of this is the expertise and judgment of the Program Directors, and all indications are that this is working very well in the Division. It also seems that the criterion of Broader Impact is playing an important role, which is commendable.	

II. Questions concerning the selection of reviewers. Please answer the following questions about the selection of reviewers and provide comments or concerns in the space below the question**.**

SELECTION OF REVIEWERS	**YES , NO, DATA NOT AVAILABLE, or NOT APPLICABLE**
1. Did the program make use of reviewers having appropriate expertise and/or qualifications? Comments: The COV was most impressed with the expertise and qualifications of the chosen reviewers. It is clear that diversity of reviewers is an important consideration, as it should be. We are also impressed that, for large collaborations where an outsider might not be knowledgeable enough about details, ways are being found to use some insiders as reviewers while still obeying COI guidelines.	YES
2. Did the program recognize and resolve conflicts of interest when appropriate? Comments: The Physics Division clearly takes the issue of conflict of interest very seriously. The COV saw many examples during our reviews of conflict of interest being recognized and dealt with appropriately.	YES

3. Additional comments on reviewer selection: N/A	

III. Questions concerning the management of the program under review.
Please comment on the following:

MANAGEMENT OF THE PROGRAM UNDER REVIEW
1. Management of the program Comments: Overall the COV finds that management of the programs within the Physics Division is excellent. We are most impressed with the expertise, judgment, and dedication of the Program Directors. We hope that they can continue to have the opportunity to provide the management that is so critical to these programs. We have some concerns about increasing demands on their time from thrusts like the Big Ideas. We very much hope that NSF leadership will achieve an appropriate balance for these demands.
2. Responsiveness of the program to emerging research and education opportunities. Comments: The COV finds that the programs we have reviewed have done very well in responding to emerging research and education opportunities. The Program Directors are alert to such opportunities and the interdisciplinary nature of many of the programs means that this issue is always at the forefront.
3. Program planning and prioritization process (internal and external) that guided the development of the portfolio. Comments: The specific process varies considerably across different programs in the Physics Division. For example, for Nuclear Physics and Elementary Particle Physics a strong influence comes from external planning via NSAC, P5, etc. But in general the COV finds that the Program Directors are well plugged into the community and understand the intellectual frontiers that are of highest priority.

4. Responsiveness of program to previous COV comments and recommendations. Comments: In general the COV finds that the Physics Division was appropriately responsive to the 2015 COV comments and recommendations. These recommendations included suggested structural changes among programs and proposed increases in co-funding between programs and divisions. An issue was also raised related to CAREER awards, which we will address again in this year's report recommendations.

IV. Questions about Portfolio. Please answer the following about the portfolio of awards made by the program under review.

RESULTING PORTFOLIO OF AWARDS	APPROPRIATE, NOT APPROPRIATE, OR DATA NOT AVAILABLE
1. Does the program portfolio have an appropriate balance of awards across disciplines and sub-disciplines of the activity? Comments: The portfolio of programs supported by the Physics Division is remarkably diverse. It also aligns very well with the most important scientific questions in the field. The COV is impressed with the flexibility that the Program Directors maintain to respond to emerging opportunities.	APPROPRIATE
2. Are awards appropriate in size and duration for the scope of the projects? Comments: In general the COV believes the size and duration of awards are appropriate, but it certainly is a complicated dynamics. Given overall funding constraints, the size of awards of course directly influences the success rate of proposals. We heard from several programs that they are worried that many awards are not large enough to support	APPROPRIATE

postdocs at this critical time of their careers. The typical duration of an award is 3 years, but this can vary depending on the details of the project. We certainly encourage the Physics Division to continue to give Program Directors the flexibility to apply different sizes and durations of awards depending on the circumstances. They seem to be doing very well at optimizing the scientific and educational outcomes in a complicated, constrained environment. The 2019 COV addresses some of these issues in its main report.	
3. Does the program portfolio include awards for projects that are innovative or potentially transformative? Comments: The Physics Division portfolio includes many examples of awards that are innovative and potentially transformative. This is one of the driving factors that determine success in funding.	APPROPRIATE
4. Does the program portfolio include inter- and multi-disciplinary projects? Comments: The portfolio includes many awards that involve interdisciplinary or multidisciplinary research. This can be seen from the number of awards that have co-funding and from the alignment with some of the Big Ideas thrusts.	APPROPRIATE
5. Does the program portfolio have an appropriate geographical distribution of Principal Investigators? Comments: The COV did not investigate this in a comprehensive way, but it is clear that the portfolio includes PIs from many parts of the country. It is also the case that centers and laboratories around the country are supported by the portfolio of awards. While we did not have the opportunity to look at comprehensive data on this issue, we see no reason to think there is a problem.	APPROPRIATE

6. Does the program portfolio have an appropriate balance of awards to different types of institutions? Comments : The COV found that the Physics Division portfolio includes awards to major research universities but also many awards to smaller univerisities and RUI institutions. Typically, large experiments include a variety of collaborators from different types of institutions and the Physics Division funding includes the full range.	APPROPRIATE
7. Does the program portfolio have an appropriate balance of awards to new and early-career investigators? NOTE: A new investigator is an individual who has not served as the PI or Co-PI on any award from NSF (with the exception of doctoral dissertation awards, graduate or post-doctoral fellowships, research planning grants, or conferences, symposia and workshop grants.) An early-career investigator is defined as someone within seven years of receiving his or her last degree at the time of the award. Comments: The Program Directors in the Physics Division appear to place important emphasis on supporting promising researchers early in their careers. The CAREER award program is an example of such support, but it is by no means the only example. In general the programs have a good balance of awards to researchers at different stages of their careers.	APPROPRIATE
8. Does the program portfolio include projects that integrate research and education? Comments: The great majority of awards integrate research and education, either through training of young scientists or through outreach or both.	APPROPRIATE

9. Does the program portfolio have appropriate participation of underrepresented groups[8]? Comments: The COV believes it does not have enough data to provide a clear answer to this question. It does seem that proposal success rates for women and/or URM PIs are consistent with their representation in the field, but that representation is of course still low. We encourage NSF to do what they can to collect relevant data, as this issue is definitely an important one.	DATA NOT AVAILABLE
10. Is the program relevant to national priorities, agency mission, relevant fields and other constituent needs? Include citations of relevant external reports. Comments: The Physics Division programs are closely aligned with national priorities and the Agency mission. This can be seen from the strong amount of co-funding in the programs and the connections to NSF-wide thrusts such as the Big Ideas. External reports that show the close alignment include the Quantum Initiative, Decadal Surveys sponsored by the National Academies, the Nuclear Science Avisory Committee Long Range Plans, and the Particle Physics Project Prioritization Plan.	APPROPRIATE
11. Additional comments on the quality of the projects or the balance of the portfolio: We want to emphasize that we found the quality of the projects funded to be outstanding and the balance of the portfolio to be remarkable. The NSF Physics Division personnel and the community as a whole have every reason to be proud.	

[8] NSF does not have the legal authority to require principal investigators or reviewers to provide demographic data. Since provision of such data is voluntary, the demographic data available are incomplete. This may make it difficult to answer this question for small programs. However, experience suggests that even with the limited data available, COVs are able to provide a meaningful response to this question for most programs.

OTHER TOPICS

1. Please comment on any program areas in need of improvement or gaps (if any) within program areas.

Comments:

More than one of the subgroups noted that it appears that the Theory part of a subfield is often funded relatively more sparsely than the Experiment part. In particular this seems to have the result of limiting the number of postdocs in theory. We comment on the postdoc funding issue elsewhere as well.

2. Please provide comments as appropriate on the program's performance in meeting program-specific goals and objectives that are not covered by the above questions.

Comments:

N/A

3. Please identify agency-wide issues that should be addressed by NSF to help improve the program's performance.

Comments:

It appears that the meta-program funding mechanism, compared to a separate solicitation, makes it much easier to leverage cross-cutting projects with already existing funding programs. For example, the Windows on the Universe thrust is already helping to support important projects in the Physics Division, while the Quantum Leap thrust is not.

The expertise and dedication of the Program Directors in the Physics Division is really commendable. The Division and the Agency as a whole should do everything possible to continue to attract and retain such wonderful scientists. This includes continuing to give them the flexibility to really manage their programs as well as avoiding overloading them with more and more tasks.

4. Please provide comments on any other issues the COV feels are relevant.

Comments:

The numbers of grants, success rates, and typical grant sizes have not changed greatly in the last decade. At the same time, the quality of funded research and of investigators has been consistently high. However, the cost of doing research has significantly risen. This raises the obvious point that the impact of the typical reward has diminished. The COV understands that solving this issue is not possible for NSF or the Physics Division alone, but it is important to raise the issue.

We encourage the Physics Division to pursue co-funding projects with private foundations whenever possible.

5. NSF would appreciate your comments on how to improve the COV review process, format and report template.

Comments:

Overall the COV found the current review process to be a significant improvement over previous processes. The availability of e-jackets before the meeting meant that the committee could find time to review the jackets and iterate with their subgroup colleagues prior to the in-person meeting.

Some committee members did find it difficult to access the information needed to carry out the COV evaluation. Documents were spread out over several sites and with different file formats. We suggest creating a consolidated source of information that includes the relevant reports, proposals, and other documents.

The Committee of Visitors is part of a Federal advisory committee. The function of Federal advisory committees is advisory only. Any opinions, findings, conclusions, or recommendations expressed in this material are those of the Advisory Committee, and do not necessarily reflect the views of the National Science Foundation.

SIGNATURE BLOCK:

For the 2019 Physics Division Committee of Visitors
Robert P. Redwine
Chair

Appendix B: Meeting Agenda

National Science Foundation
Division of Physics
DoubleTree Hilton—Crystal City
300 Army Navy Drive, Arlington, VA 22202
June 20-21, 2019

Thursday, June 20 – Room TBD

8:00 Refreshments (Room TBD)

8:30 Introductions, welcome, and Charge to Committee of Visitors (COV)
A. Kinney, Assistant Director, Directorate for Mathematical and Physical Sciences (MPS)

8:50 Introductory Remarks
Robert Redwine, Chair, COV

9:15 Introduction to Division-Level Review
- Division's Balance, Priorities, and Future Directions
- *Additional Topics:* Division's leveraging of the Big Ideas: Balance between core research and engagement in NSF initiatives

Denise Caldwell, Division of Physics

10:15 Full Panel Discussion of Division-Level Questions
Robert Redwine, Chair, COV

11:00 BREAK

11:15 Instructions for Breakout Sessions
Robert Redwine, Chair, COV

11:20 Individual Program Groups Discuss Division-Level Questions *(Breakout)*

12:30 WORKING LUNCH
Follow-up on individual programs *(Breakout)*
If applicable, receive answers from PDs on additional questions

13:30 Executive Session to consolidate input to Items I, II, and III *(Breakout)*

14:30	Executive Session Robert Redwine, Chair, COV
15:30	BREAK
15:45	Preparation of Program Reports
17:45	Executive Session (If necessary, formulate additional questions to Division Leadership) Robert Redwine, Chair, COV
18:30	Adjourn for Dinner

Friday, June 21-- Room TBD

7:30	COFFEE (Room TBD)
8:00	(If necessary) PHY answers to previous evening questions Denise Caldwell, Director, Division of Physics
8:30	Presentation of Preliminary Program Reports by Program Chairs

Appendix C: 2019 Physics Division COV Participants

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Appendix D: 2019 Physics Division COV Subpanels

COV Chair	**Institution**
Robert Redwine	Massachusetts Institute of Technology

Atomic, Molecular, Optical Physics/Quantum Information Science

Julio Barreiro	University of California, San Diego
Philip Bucksbaum	Stanford University
Timothy Gay*	University of Nebraska
Randy Hulet	Rice University

Elementary Particle Physics and Cosmology Theory

Lisa Everett	University of Wisconsin, Madison
Xerxes Tata	University of Hawaii
James Wells *	University of Michigan

Elementary Particle Physics Experiment/LHC

Emanuela Barberis	Northeastern University
Robin Erbacher*	University of California, Davis
Michael Shaevitz	Columbia University

Gravitational Physics/LIGO

Andrew Geraci	Northwestern University
Peter Saulson*	Syracuse University
Joshua Smith	California State University, Fullerton

Integrative Activities in Physics

Jonathan Baggar	TRIUMF/Johns Hopkins University
Andrew Baker*	Rutgers University
Jennifer Pearl	American Association for the Advancement of Science

Nuclear Physics, Theory and Experiment

John Beacom*	Ohio State University
Elizabeth Beise	University of Maryland
Arthur Champagne	University of North Carolina
Michelle Dolinski	Drexel University

Particle Astrophysics

Maury Goodman	Argonne National Laboratory
Julie McEnery*	NASA/Goddard Space Flight Center
William Wester	Fermi National Accelerator Laboratory

Physics Frontier Centers and Physics at the Information Frontier

Catherine Royer	Rensselaer Polytechnic Institute
Marius Stan	Argonne National Laboratory
Daniel Steck*	University of Oregon

Physics of Living Systems

Dagmar Ringe	Brandeis University
Aravinthan Samuel*	Harvard University

Plasma Physics and Accelerator Science

Felicie Albert	Lawrence Livermore National Laboratory
Alain Brizard*	St. Michael's College
Michael Murillo	Michigan State University

*Subpanel Chairs

Appendix E: Charge to the 2019 Division of Physics Committee of Visitors

By NSF policy, each program that awards grants and cooperative agreements must be reviewed at four-year intervals by a COV comprised of qualified external experts. NSF relies on their judgment to maintain high standards of program management, to provide advice for continuous improvement of NSF performance, and to ensure openness to the research and education community served by the Foundation. Reports generated by COVs are used in assessing agency progress in order to meet government-wide performance reporting requirements and are made available to the public.

The COV is charged to address and prepare a report on:

- the integrity and efficacy of processes used to solicit, review, recommend, and document proposal actions;
- the quality and significance of the results of the Division's programmatic investments;
- the relationship between award decisions, program goals, and Foundation-wide programs and strategic goals;
- the Division's balance, priorities, and future directions;
- the Division's response to the prior COV report of 2015; and
- any other issues that the COV feels are relevant to the review.

The COV report is made available to the public to ensure openness to the research and education community served by the Foundation.

Decisions to award or decline proposals are ultimately based on the informed judgment of NSF staff, based on evaluations by qualified reviewers who reflect the breadth and diversity of the proposed activities and the community. Systematic examination by the COV of a wide range of funding decisions provides an independent mechanism for monitoring and evaluating the overall quality of the Division's decisions on proposals, program management and processes, and results.

The review will assess operations of individual programs in PHY as well as the Division as a whole for four fiscal years: FY 2015, FY 2016, FY 2017, and FY 2018. The PHY programs under review include:

- Accelerator Science
- Atomic, Molecular, and Optical Physics
- Computational Physics
- Elementary Particle Physics
- Gravitational Physics
- Integrative Activities Physics
- Midscale Infrastructure (Division-wide)
- Nuclear Physics

- Particle Astrophysics
- Plasma Physics
- Quantum Information Science
- Physics Frontiers Centers
- Physics of Living Systems

Where appropriate these include both experimental and theoretical research programs.