

NASA COPAG High Risk / High Reward Proposal Survey

The NAC Science Committee has charged the NASA Astrophysics Advisory Committee (APAC) with addressing the question of whether the community feels that NASA has the correct mechanisms and processes in place to adequately and fairly deal with proposals that address high-risk / high-reward or interdisciplinary subject matter. This survey was crafted by the NASA Cosmic Origins Program Analysis Group (COPAG) Executive Committee to solicit input relevant to that charge.

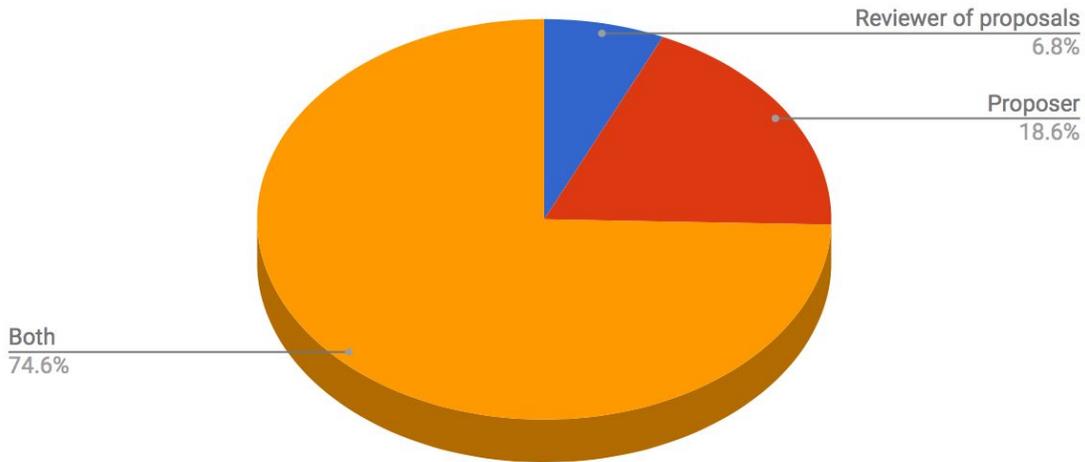
High Risk-High Reward: A project that has the potential for impactful scientific outcome, but also carries high risk, due to factors including, but not limited to, unique or difficult observation or analysis techniques, or new technology development or usage.

Interdisciplinary and InterDivisional: A topic that crosses one or more programmatic borders within NASA. Examples might include high-redshift GRBs (of interest to both Cosmic Origins and Physics of the Cosmos), star formation (of interesting to Cosmic Origins and Exoplanet Exploration), or comparative planetology (of interest to both Astrophysics and Planetary Science).

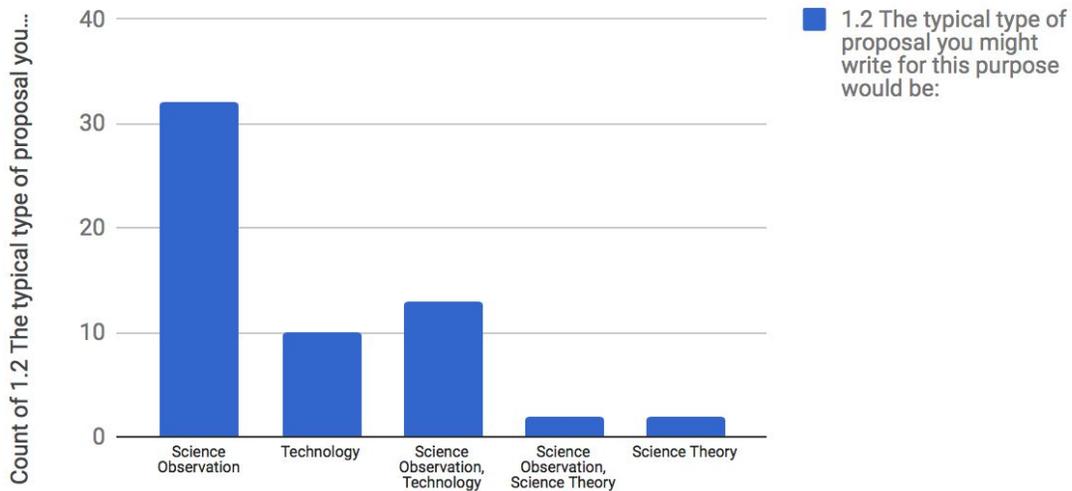
Demographics

Number of respondents: 59

Count of 1.1 Your role in the proposal process:



Count of 1.2 The typical type of proposal you might write for this purpose would be:



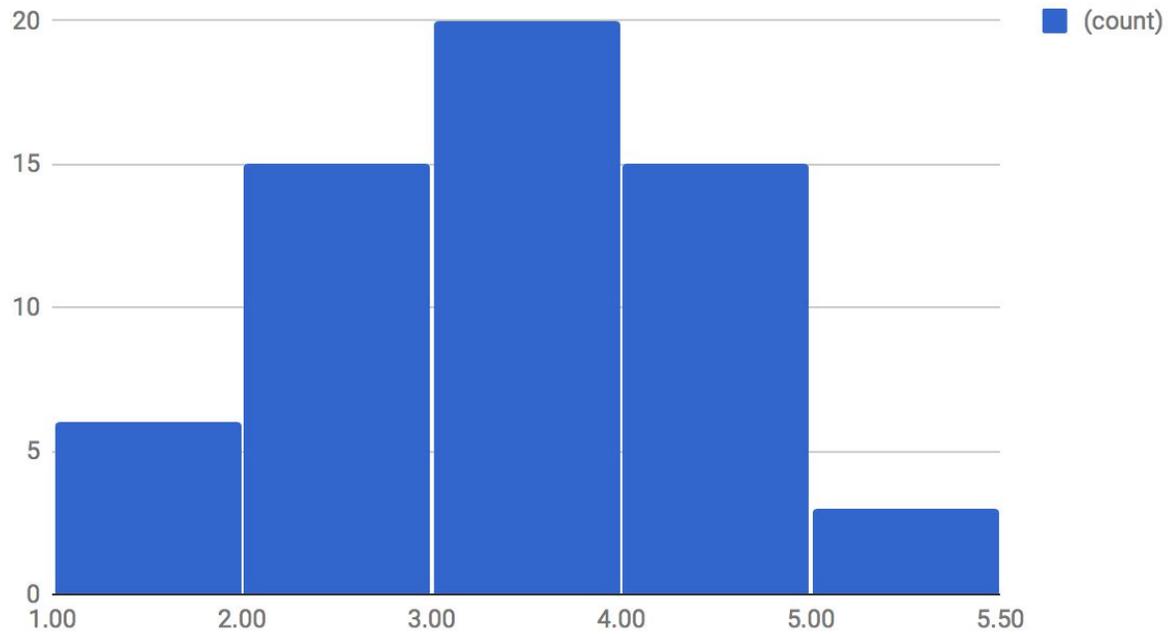
1.2 The typical type of proposal you might write for this purpo...

2.1 How strongly do you agree/disagree with the statement, on scale of 1-5:

The current practice of soliciting by topic and evaluation for merit, followed by flagging high-impact/high-risk projects for the selection official, is adequate.

1=Strongly Disagree; 5=Strongly Agree

Histogram



2.2 Discuss aspects of the solicitation, review and selection process that could be added, removed or modified that would allow NASA SMD to more effectively elicit and support high-risk/high-impact projects:

I believe that the detector technology area is still way underfunded which reflects an aversion to high risk/high potential proposals. APRA Technology proposals are by definition in the TRL 0-3 range, which makes them high risk by definition. This should be seen as a normal aspect of these proposals rather than a weak point in the review process.

A dedicated high-risk / reward proposal category is essential since the risk-averse panel mindset is deeply ingrained. For instance, the sub-orbital program could be augmented to include a high-risk category. Opening up a line to NASA astrophysics SmallSats (related to the recent RFI) would also be a promising avenue.

I think the current review system does a fairly good job at identifying risky proposals, but that many times the psychology of review panels is very conservative and these do not get selected. This typically happens because oversubscription are high, and there are always plenty of non-risky proposals that yield exciting science. A potential way to encourage/select more risky proposals would be to set aside small pools of time in each call for these types of observations. These pools would not have to be filled each cycle, but they could be filled with suitable projects that might not otherwise get selected. A review of the results of this process, suitably averaged over 5 or more cycles, could then be evaluated and the process, tuned if deemed successful.

In a resource-constrained environment, it is very difficult for a high-risk/high-impact project to compete against a portfolio full of lower-risk/high-impact proposals, or even lower-risk/moderate impact proposals. Peer reviewers tend to be biased against high-risk proposals. If the intent is to allow a higher selection rate in the high-risk category, then there should be an explicit call for such proposals, a separate budget to support them, a peer panel attuned to understand the risks and potential rewards, and perhaps a separate solicitation within ROSES. While the selecting official could choose to fund a high-risk proposal deemed to have a "selectable" grade, I believe that rarely happens in practice when a number of lower-risk proposals have better overall adjectival ratings, even when the "selectable" high-risk proposal has much greater potential value to NASA. (Recommendation: gather data to substantiate that assertion, and interview the Astrophysics Division Director to get his views on this subject. Dominic Benford could also provide helpful information, as he understands the issue as a technology developer/proposer and as HQ program scientist.)

Review process incentivizes well argued incremental proposals over higher risk proposals which are less fully formulated. A phased selection and maturation process for proposals where initial funding to fill out the idea is provided would help alleviate this. That is, great ideas where the proposal is only "fair" or "good" should be flaggable by reviewers for a separate incentivized grant of seed money to develop the idea.

The process tends to favor proposal which offer incremental advances -- review comments typically equate high innovation with high risk.

NASA SMD solicitations usually implicitly (occasionally explicitly) assumes it knows the answer to the question that is being solicited. As a result, proposals that follow the traditional path do well (low risk because we are comfortable with the concept) and high-impact/high risk projects are not selected (not clear it will work). In addition, for technology driven proposals, reviewers are not fully cognizant of technology advances or new approaches and claim something is not possible when it has been demonstrated. Since all proposals have the same page limit, spending pages to prove your new approach will work puts you at a major disadvantage with those doing minor improvements on traditional approaches. I think there are things that can be done: first, set aside some portion of a solicitation exclusively for high-impact/high risk projects. When I have had the opportunity (Chief Technologist for GSFC and later Chief Architect at Northrop Grumman) I set aside 10-15% of my funding to specifically advance high-impact/high-risk projects. I worked very well and did have a big impact. The project that astronomers are most familiar with that benefitted from that approach is the starshade concept. This was exclusively funded by Northrop high-impact/high-risk funds from 2005 to 2010 and laid the basis for what JPL and Northrop are doing today. Another option would be to allow a "technology appendix" for high-impact/high-risk concepts that does not count against the main proposal page count. My main proposal now has page parity with "do the status quo" proposals and I have an appendix to make my case for the new concept. As for reviewers, for high-impact/high-risk proposals I think SMD needs to send out the proposal to more external reviewers for mail-in reviews and not just do face to face reviews. Most (all?) of DoD and related use this approach and they have a much higher success rate at this type of project than does NASA.

I have no strong opinion on this question.

We should try to allocate 5-10% of the observing time/financial resources to high-risk/high-reward project because it is these that are most likely to bring about significant advances once in a while even if 10 of them fail. I think they should be reviewed separately (all fields) and the top one or two of them picked.

My experience is that there is a huge gap between NAIC and SAT/APRA. From sitting on panels, I have seen that when there is significant funding pressure (and there always is), there is a clear preference for the proposals that are have a high likelihood of return. Suggestions: in the review process, ask panels to identify proposals that are high-risk high-reward. Just getting the reviewers to recontextualize the proposal in this way will, I predict, cause some of the high-risk high-reward proposals to rise up in rankings.

Each SMD committee is requested to utilize its next two public meetings to receive and review information, deliberate and finalize written answers to the questions above, which could include recommendations or findings, plus any supporting data. Presentation format is the preferred medium for initial communication of the answer, followed up by a letter. Each SMD division committee is requested to provide a presentation to the Division Director, and the chair of each committee is requested to make a presentation at the Science Committee meeting. The Science Committee is requested to provide a summary and overview presentation to the SMD AA

Review panels need to be instructed to not be as risk-averse as they generally are. They need encouragement to take an occasional chance on a program where the outcome is not all but guaranteed.

Novel techniques often fall into the category "interesting, but may or may not yield results". Panelists could be instructed to put more weight into the "interesting" part, and less weight into the "may or may not yield results". Alternatively, panelists could be instructed to assign separate grades to these two aspects of proposals.

I propose and review in NASA Heliophysics, and I am not aware of the practice mentioned of flagging high-impact/high-risk projects for the selection official. (I am aware that risk is assessed, but not a high-impact/high-risk combo.)
$(\text{time to write HRHI proposal})/(\text{fraction of HRHI accepted}) = \text{not worth the effort}$
Limited funding availability tend to favor selection of proposal that have some guaranteed return in terms of publishing results. A separate avenue for high risk proposals would help reduce this issue (given limited funding availability; a preferred solution would of course to have larger pool of money for current proposals, which would result in review panels recommend more risky proposals).
Create a specific category where proposers can self-identify rather than relying on reviewers to make their own subjective judgements.
I have never seen this option on the APRA panels I've sat on, but maybe I just didn't notice. Would be a nice option.
Reviewers are far too conservative (nit-picky irrelevant details in the proposal) as a means of rejecting proposals that are creative and high-reward. In theory proposals, it's almost like reviewers demand Phase-A studies to prove that a theoretical has 100% chance of being right. The reviewers need instruction,
An external group of experts should be on-call to advise specifically on such cases. Although current use of one-off external reviewers is good, there is value to having the same group look over and compare the range of submissions coming in.
Add exclusion of reviewers known to have a conflict of interest and have ability to cross check.
There is little to no value placed on driving technology forward under the veil of being too risky. The process does not enable quick turn around opportunities to be leveraged to take advantage of changing environments. Process is good but should not be exclusive such that NASA misses opportunities that present themselves outside of the established process.
It is difficult to propose work to combine high energy electromagnetic observations for multimessenger work, because the other messengers are funding by NSF. In the proposals that allow this kind of work, it seems difficult to get funded because of the uncertain results. For example, I cannot propose specific science cases for gamma-ray and gravitational wave results because I do not know what the universe will provide. I was the EM lead on the joint gravitational wave-gamma ray burst paper and it has had a citation a day since release. The scientific payoff is immense and some of it cannot be uncovered any other way. But much work, effort, and money could go towards development that never pays off.
There should be explicit calls for these kinds of work, or some statement of preference for some fraction of awards to this kind of work within existing funding methods. This comes to much of the themes of how we communicate time-domain multimessenger science to maximum the return. The reason it is currently less than ideal is because gamma-ray funding is split between NASA and NSF, as is cosmic rays, and neutrinos and gravitational waves are NSF only. The funding is separate, so the pipelines and reporting are developed separately instead of in an integrated manner. This should be fixed.
Reviewers are not always knowledgeable, consistent or correct in new or more innovative technical areas, leading to misinterpretation. Biggest problem: NASA needs more \$\$\$!
High risk observations are looked at as 'fishing expeditions'

Not sure where to upgrade the process, but my feeling for the past 10+ years has been that the review panels are much too conservative when assessing science impact / TRL. The best way to win a proposal should NOT (as currently seems true) to do what everyone else is doing, but ten percent better!

Recalibrating the TMC for a new NASA risk posture -- they are the gatekeepers for risk, and then making that risk posture absolutely clear in an AO to proposers.

In my experience, successful proposers need to have already done a significant portion of the project. This gives a preference towards safe and unsurprising science ends results, and does not give room for exploratory, higher risk projects, where the return is not as predictable.

Provide more latitude in proposal topics. Not be too specific in solicitation.

It is nearly impossible for a small team to apply an existing, novel instrument to exoplanet science, as both NASA grant panels and ground-based telescope TACs require that the other grant funding or time to the PI first. That is, there is no current mechanism to obtain "good faith" funding/telescope time that can be leveraged to gain telescope time/funding. Director's Discretionary telescope time is an excellent mechanism to obtain telescope time in this arena, but no mechanism exists on the funding side. Panelists therefore highly score proposals that incrementally continue tried-and-true techniques, while high risk/high reward proposals are judged according to "we'll fund you to do the work once you show us the work was successfully accomplished." Such a proposal will not be scored highly, due to the gut feeling of the panel, and arbitrary items are inserted in the Major Weaknesses panel section to ensure the scoring will hold up to an appeal. That is, a certain number of Major Weaknesses must be present for a given number of Major Strengths in order to justify a relatively low scoring. There must be a new NASA ROSES solicitation for high risk/high reward science, or at least a sub-element that one can apply for within the main element. For example, a C.6 Solar System Observations proposal may also apply for the C.17 Planetary Major Equipment solicitation to obtain funding for instrument development for C.6 science. High risk/high reward proposals simply cannot compete with incremental continuation of well-established projects. This is especially true for new instrumentation, which is critical in the data-starved exoplanet field.

There needs to be a strong focus on the S/N prediction and prior success with the type of observation. Omitting these from the proposal should be a red flag that automatically disqualifies the proposal.

2.3 Please provide specific experiences where meritorious high-risk high-reward proposals have or have not been supported by the current system:

High risk proposals in the technology area usually include highly novel methods or devices that are not well known to the average reviewer, especially since most reviewers are focused on the science side of things. This was a problem in the past for me as the reviewer comments were clearly reflecting an inadequate level of knowledge in the area. For such proposals, we need to have highly qualified people whose primary focus is in new technology development and not someone with a focus on astrophysical science observations.

Numerous important SAT grants, and extragalactic-science focused cubesats have not been supported.

The community-derived Astrophysics Roadmap, "Enduring Quests/Daring Visions," recognizes interferometry as vital to NASA's future in astrophysics. For a number of years, the Cosmic Origins Program Office's PATR documents have consistently recognized the interferometry technique we're developing as a priority, but it has never appeared as a top priority. My spatio-spectral interferometry testbed and related research was initially supported through APRA. However, in several recent APRA cycles (and once in SAT, when the work was in scope for that year's solicitation), the proposal was rejected by peer reviewers who understood the importance and potential impact of the work, but who raised insignificant or impertinent technical objections. In each successive year, the prior year's weaknesses were addressed, and new trivial objections were raised, presumably by new reviewers. I attribute this to the peer reviewers' bias against interferometry, which may stem from the community's experiences with the technically challenging interferometry mission concepts SIM and TPF-I, and a general aversion to risk. The risk aversion seems not to be mitigated by the fact that our testbed yields high-quality data representative of the data a space-based far-IR interferometer will yield, our team's strong publication record, and our open data access policy. There is also a general reluctance in the community to support technology development through APRA for a mission that's more than a few years away, but that only leads to a perpetual cycle of delays. Finally, the mid-TRL desert has long been recognized, and it comes into play here. After proposing without success several years in a row, I decided that proposing was not worth the time, and I chose not to apply in recent APRA rounds. Working with a NASA-funded postdoc and a graduate student supported through the NASA Technology Research Fellows program, and with a modest amount of volunteer effort from a highly-skilled NASA optical engineer, we've been able to continue the research. The graduate student will soon defend his dissertation on the work we did together. This would not have been possible if I were not based at a NASA Center. I'm sure there are many other worthy high-risk/high impact studies that crumble for lack of a suitable proposal solicitation or a more risk-tolerant set of peer reviewers.

None, other than the usual 5-10x oversubscription

As a reviewer I have poorly rated proposals by innovative university groups passed over because the proposals are relatively poorly formulated compared to more incremental proposals from NASA centers

A detector proposal comes to mind which was submitted and declined three times with minor revision -- upon comments received and the advise of a colleague -- the offending innovative portions of the text were removed and the proposal was awarded. The stated innovations were subsequently explored, realized, and are currently found in widespread use by the community.

My days as a proposer of this type of proposal are over, but I have this bias toward high-impact/high-risk proposals in many reviews. Unfortunately, I am not allowed to be specific here because of the confidentiality of the review process. A proposal that I participated in that was submitted to the recent segmented telescope solicitation was not-selected, but we have not yet been given the debrief. It was a very novel high-impact/high-risk concept far from the mainstream. Depending on what is said in the review comments I may send a supplement to this survey with very specific comments.

There have been several "discovery" class projects both small and large which did not even rise to the criteria for selection. I was personally involved with proposals to do KBO occultations (incorrect sensitivity review at SOFIA), molecular H₂ studies at z~20 (too risky for ALMA), spectroscopic observations of non-LBGs at z~8-12 (Keck). There is too much group-think in these committees and there is a strong tendency to do "more of the same".

My experience is that high risk proposals usually get penalized because of the risk factor itself. Often you hear the comment: 'this proposal uses a well tested technique while this other is too risky'

I have several examples, but given that they involve my proposals, I feel that I can't give a clear argument without significant bias.

EMR and HRHR projects are expected to address important and challenging issues resulting in the advancement of scientific knowledge. However, in HRHR projects, the risks associated to address the proposed scientific issues are high, and if it is successfully solved, should result in a high reward. This could be in the form of new hypothesis, methodology, process or products, and may have the potential to open up new frontiers in S&T. In other words, proposals resulting in 'incremental knowledge' will not be supported under the HRHR scheme

I was PI of a large high-risk program with Hubble Space Telescope. It took 7 submissions to get it approved. In at least half of the cases where it was not accepted, it was the highest ranked large program below the cutoff line.

I not aware of any.

The problem is that reviewers as well as NASA have become risk averse. With oversubscription approaching 10:1 in some programs, reviewers are forced to choose projects for funding that are guaranteed success. This generally means low risk. So, risk aversion equates to a low ratio of proposals funded to proposals submitted.

When the total request is small, these types of proposals are usually seen favorably

Technology development projects (sounding rockets, balloons, CubeSats) have become so competitive that it is difficult for any but the lowest-risk proposals to get funded. This naturally leads to fewer high-risk, high-reward proposals funded. I expect that this will change with the increased technology funding in the DRIVE initiative.

they do not get written in the first place

NIAC is all about this concept. The problem is NIAC is "one and done." No renewal is possible and the problems are so hard that the \$500k can't be expected to make much of a dent.

Unfortunately, proposals have been victim of reviewers who have dishonestly sign off as having no conflict of interest. In more than one case, I have seen reviews that were done by people with conflicts of similar science, concept and/or same concept/science proposal and/or family member/student/member of faculty with conflict.

Lobster-I as a quick turn around collaborative opportunity with DoE as an Air Force payload was foregone because it was out of cycle. Collaboration, money, and precious time was traded for process.

These high-risk proposals that I have been involved in that have succeeded include the last round of senior review for Fermi and my NPP application. However, I believe they succeeded in large part because these proposals were specific to Fermi, which has heavily invested in multimessenger science.

The high-risk proposals that I have been involved in that have failed is every proposal that extends beyond Fermi itself, which have largely been for other fellowships. This is unfortunate as these are the more ambitious projects (combining information from multiple instruments/messengers). It is hard to ask NASA for money to combine information from NASA missions and NSF missions. This is compounded when the proposals is 'this type of detection might happen in the future and I would like to develop the tools to maximize the scientific gain'. When the scientific payoff is either immense or nothing it seems the current process tends to the conservative side.

There have been 4 SiC mirror proposals submitted, leveraging demonstrated successes resulting from \$100M+ investments by others, opening new mission possibilities (active cryo mirrors to avoid expensive cryo null figuring, e.g.). Each review wildly different from the other, some drawing conclusions that ignore explicit discussions in the proposals, some seemingly reflecting a strong conservative bias. Proposals were generally selectable, but not selected.

Observationally, these are too numerous to recount

Many years ago, there was a proposal to develop a free-flying occulting disk to detect exoplanets. It was not funded.

Results of panel discussions are confidential.

No personal experience I can think of at the moment.

I received 2 full nights of Director's Discretionary Gemini North time to commission my POLISH2 polarimeter, which directly detected scattered light from the WASP-12b exoplanet after 10 h of observations and requires 0.035 to 0.11 micron corundum cloud particles. This is a coup, and I am nearly ready to submit a paper to Nature, yet I receive the following panel reviews from the E.3 Exoplanet Research Program panel:

Major Strengths: "The proposal argues convincingly that polarimetry would contribute to the measurements of the geometric albedos and to the characterization of clouds and hazes of hot exoplanets."

Minor Weaknesses: "The proposal did not adequately demonstrate that the additional observations proposed here would improve our understanding of the properties of exoplanet clouds or hazes."

The following year, I received the following Major Weakness in my 5 sigma detection of scattered light from the exoplanet: "While the preliminary data of WASP-12b and tentative detection appeared to be a positive step..." That is, the 5 sigma detection was called "tentative," even though a Nature paper describing the 5 sigma detection of atmospheric features in a different exoplanet had just been announced. There is a stigma that the community applies to new techniques, regardless of statistical validity. There is also a vicious cycle for instrumentalists where the lack of discovery papers (due to the time required to design, develop, test, commission, and calibrate instruments) gives the community "cold feet" when funding and telescope time proposals are submitted. In order to get the telescope time or funding to perform the research, that research must be perceived to be commonplace. There is an intense risk-aversion to the field, even though it is precisely the high risk/high reward instrumentation that drives it. Where would we be without Kepler? Yet Bill Borucki has told me how difficult it was to convince the community to develop and launch Kepler. Where would we be without the GPI, SPHERE, MagAO, etc. high contrast imaging systems? While these instruments were developed by mid-career, highly respected PIs, it is quite difficult for an early-career instrumentalist PI to obtain funding and telescope time.

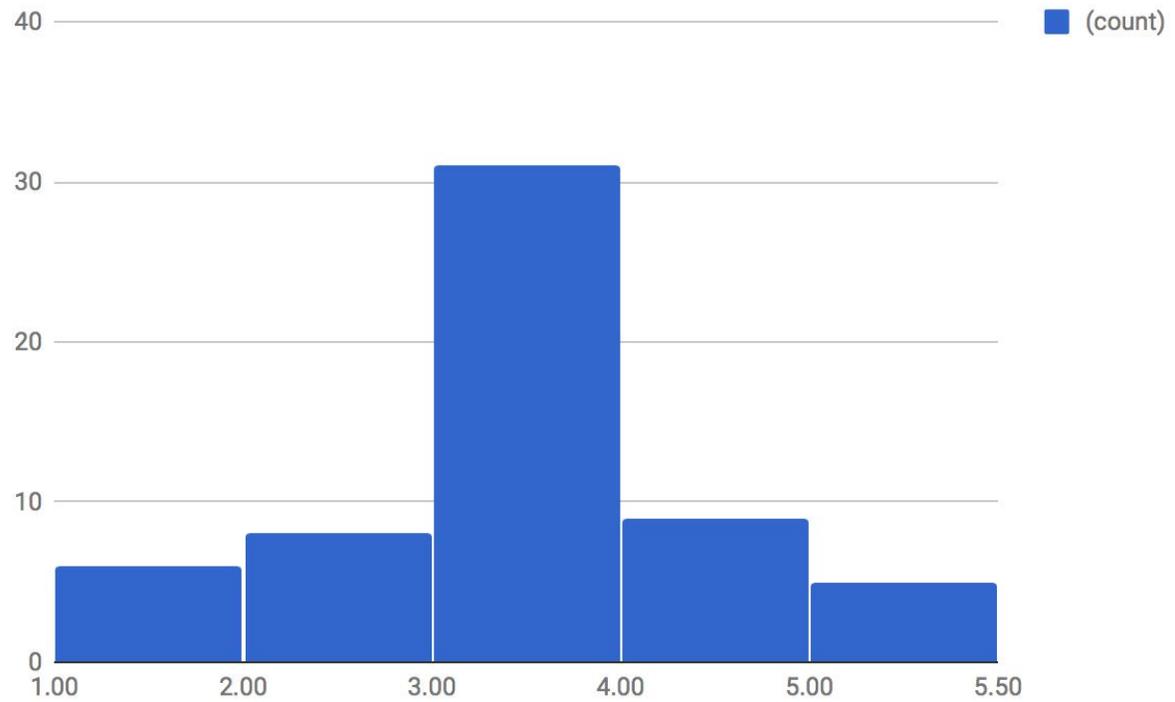
All our eggs are put in the space-based basket, yet JWST will be delayed a year and WFIRST may be cancelled. Please consider funding relatively cheap high risk/high reward, ground-based research to advance science. It is a travesty that mature, novel instrumentation should lay fallow due to gut feelings by panelists. It is difficult to allay those fears in a 15-page proposal that must 1) introduce the new technique, 2) describe the new instrument, 3) provide evidence of instrument sensitivity, accuracy, and stability, and 4) discuss why every potential atmospheric or stellar systematic effect is alleviated. As an example, I have 50 nights of Lick 3-m data with POLISH2 observing the HD 189733b hot Jupiter. This spotted star shows no variability tied to the rotation period of the star in B band, yet I always receive panelist comments (both for NASA funding and telescope TAC proposals) asking how starspots affect observations. When I show signal to noise calculations and plots, there is always a question about how Earth's atmosphere affects data quality. If I had another page or two, I could go into detail to prove that each of these effects is negligible. But the 15-page limit presents this. Please consider enabling a few extra pages for enhanced explanation for high risk/high reward proposals.

In many cases of long Spitzer stares, it could easily have been demonstrated that the observations were hopeless. Often, my group made the calculation and decided not to waste Spitzer time, but others unscrupulously (or ignorantly) applied. In some successful proposals, the predicted S/N was 0.5 or less per eclipse/transit. With Spitzer's systematics, there are very few, if any, examples of successfully stacking the data to get a clear detection of a single object that was not detected on its own. Yet, Spitzer awarded many hundreds of hours that could have been used to support dozens of investigations but ended with predictably null results. There is a big difference between "high-risk" and "hopeless". Simply omitting the S/N calculation should be a strong red flag. Depending on stacking when it's been shown not to work should be, as well. Review managers need to recognize that proposers are supposed to act selfishly and reviewers are busy and often not expert in every aspect of a proposal. They should develop some basic criteria/best practices/lists of questions that they give to reviewers, and critically evaluate the quality of the review.

3.1 Similarly, how strongly do you agree/disagree with the statement, on scale of 1-5:

The current practice of soliciting by topic and evaluation for merit, followed by flagging important interdisciplinary and/or interdivisional projects for the selection official, is adequate.

1=Strongly Disagree; 5=Strongly Agree



3.2 Discuss aspects of the solicitation, review and selection process that could be added, removed or modified that would allow NASA SMD to more effectively elicit and support interdisciplinary and/or interdivisional projects

<p>The NAI CANs are a good example of interdisciplinary programs.</p>
<p>Not sure here what the right strategy might be, but clearly the interdivisional walls make large projects (e.g. great observatories) extremely difficult. Studying successful examples from the past where cooperation resulted in breaking down these divisional barriers would be instructive, and regular discussions between divisional representatives about strategic projects that reach across divisions (because of financial or other requirements) would be valuable.</p>
<p>The problem is systemic. A handful of solicitations emphasize the importance of interdisciplinary work, but most do not. Given that, the assembled peer reviewers tend to defend a narrowly-defined field, assuming that the R&A resources are zero-sum and that their discipline area would lose if some of the resources were to "escape" and fund something outside of or peripheral to traditional discipline boundaries.</p>
<p>I have no direct experience here.</p>
<p>I have often seen inadequate reviews by both panels for an interdisciplinary proposal. When a call for proposals is oversubscribed by factors of 5-10, then the easiest thing for each panel is to say the proposal belongs in the other panel thereby killing the proposal.</p>
<p>As far as I know, the NASA astrobiology institute is the only successful example of cross-disciplinary studies which attempts to bring together biologists, chemists and astronomers. Unfortunately, science has become so specialized that review committees (which are categorized in these specialized topics) find it challenging to review things which are beyond their sub-niche. Looking to the future, AI and machine learning will be a larger part of science than in the present and will probably need input from computer scientists, hardware engineers and physicists. I think the most effective way to have more interdisciplinary programs is to combine academic and industry participants. To that end, I recommend the following:</p> <ol style="list-style-type: none">1. NASA/NSF/DOE send a message to technologists in industry to identify which ones are available and would like to contribute their time to proposals. This provides a starting point for academic researchers to work with specific industry participants. For example, if Space-X had indicated they were looking for a payload to Mars orbit on their Falcon-9H, I would gladly have provided a basic instrument to study the zodiacal dust if NASA would provide downlink resources. Similarly, with the data volume we are facing, I would like to have inputs on how industry is planning to scale-up distributed databases.2. International participation should be encouraged and restrictions against paying local expenses for such investigators should be removed. Right now, there are restrictions against Chinese nationals for example, which is absurd for an astrophysics enterprise (especially when ITAR detectors are not involved). This also allows international participants to pursue funds in their countries for the projects.3. Remote review panels should be allowed such that people can provide inputs on proposals without necessarily traveling to a meeting (which is onerous especially if only a part of the review meeting overlaps with one's expertise).4. Mandating that leads of topics in a proposal come from different divisions/departments at either one institution or different institutions.

XRP is one area that appears to actually be working, but my view of that process is limited.

The key challenge, for many of the kinds of areas described, is in the assembly of panels with diverse backgrounds.

SMD R&A has the right balance of interdisciplinary versus discipline - focused work. The SC conveyed a sense that the scientific community may perceive the R&A process to be inherently conservative and incremental. Risk - adverse choices are made by proposers (choosing safer lines of inquiry or trusted methods), reviewers (who may not be familiar with new ideas and thus not support those) and selections made by SMD officers (to safeguard success rates). The community may in consequence fail to grow new ideas, thus furthering this cycle. A variety of tools and mechanisms already exist in the R&A program that could support high - impact endeavors. The SC underscored that multi - year awards allow long - term vision, and SMD and the SC noted generally that awards of at least 3 years work well. Also, though small - scale awards support many researchers, there may be a need for a quota for important, large - scale projects that require more investment

increase the % of the total grade that goes for interdisciplinary.

NASA Astrophysics has consistently underfunded planetary astronomy work that is actually less high risk than moderate risk with the exoplanet missions, while Planetary Science Division encourages us to apply to use astrophysics assets to do planetary science. For ALL astrophysics missions there needs to be a planetary science directorate person doing duty on identifying cross cutting planetary astronomy work being proposed and work on a cost sharing between the two divisions to ensure the science can be done.

Again, I'm not aware that this practice is used in Heliophysics, at least. I have always been under the impression that the proposals we submit must be relevant only to Heliophysics.

very narrow solicitations would allow informed panels and higher acceptance fractions. Interdisciplinary proposals are especially hard to review, as often nobody on the panel understands all of any of the proposals. Since it takes about 0.25 FTE to write a 3 FTE for 3 year proposal raising the acceptance fraction would be beneficial; also, it has been know at least since Cole, Cole & Simon (1981, Science) that except for the very best and very worst proposals peer selection is essentially random. This means that you do not actually fund better proposals by increasing the size of the pool. You do waste the proposers time.

Who flags the proposals as interdisciplinary? The proposer or the reviewer? Place more emphasis on proposers self-selecting.

Never seen this an an option

Review teams need a variety of reviewers for each interdisciplinary topic. I have sometimes been the sole person on a team with specific technical expertise, and it is dangerous for one person to be the decider.

same comment

I have not ever seen an RFP that includes review criteria that provides for interdisciplinary benefit or technology demonstration, therefore, it's assumed that this is not of value.

I believe I answered most of this with my previous answer. Specific to this, there are very few interdisciplinary/divisional options for multimessenger work, as the agencies that fund the different messengers are different, and they do not work together for a common solution.

System level consequences of subsystem innovations should be explicitly requested and evaluated.

Clear weighting from divisional benefits (ie. planetary +astrophysics) in the AO scoring.

For a useful review of interdisciplinary/divisional projects, It is necessary to have a complementary set of reviewers; not just reviewers with the same background and/or same viewpoint.

Facilitate more cross-disciplinary/cross-divisional projects.

Allow more latitude in proposals subject matter./

I am not familiar with scoring of interdisciplinary proposals.

The difficulty is that there is no catch-all program for which anything goes, so long as it is not covered by another program.

3.3 Please provide specific experiences where meritorious interdisciplinary and/or interdivisional proposals have or have not been supported by the current system

I am aware of an unsuccessful astrobiology proposal to combine insights, models, and analysis from all four of NASA SMD's discipline areas to study exoplanet habitability. However, I can't blame a bias against interdisciplinary research for this failure, as NASA's astrobiology program is inherently interdisciplinary, one of the few programs that fits that description. NASA would probably fund more interdisciplinary research if more research programs were explicitly dedicated to it.

N/A

I have proposed to HST several times to study the emission of exoplanet host stars because their emission influences the photochemistry of their exoplanets. The exoplanet panels has thrown this out on the grounds that it is a stellar proposal, and the next year the stellar panel throws the proposal out because it is an exoplanet topic. It was the same proposal both times.

I haven't been a part of these and only know about them through word of mouth.

detailed budget and budget justification of no more than two pages for funds requested from the Big Ideas Seed Grants program. Applicants should provide a strong rationale for their budget request and describe how funds will be used to support preliminary study and/or data collection (e.g., GRA or postdoctoral research associate support, travel, etc.). Funds may not be used for faculty summer salary or teaching release time, which may instead be provided by the PI's academic unit or college to strengthen institutional commitment to the proposed effort. Sub-awards are not allowed; however, external collaborators may invoice project-specific expenses

My K2 programs characterizing solar system bodies have been submitted as large proposals, however I have repeatedly only gotten the small proposal fundings worth of money, a difference of a factor of three in value, even though I met the requirements for a large proposal and received zero feedback as to why I was not receiving the large proposal funding . As a result the scientific community is not getting a solar system pipeline that works for K2 and would easily be adapted for TESS. Also as the only solar system guest investigator for the TESS mission, my selection letter informed me I would only be allowed a budget one half the value quoted in the AO (\$25k vs \$50k) with again zero justification, but my budget could be cut at in the future.

Joint proposals (i.e., HST/CHANDRA) remove the double jeopardy problem.

I have a great example - I work as part of a team that coordinates and analyzes NuSTAR observations of the Sun. There are very few NASA opportunities in which this work could be funded. The reason for this is that we can't propose to the Astrophysics GI program (because our science goals are heliophysics, not astrophysics) and we can't propose to the Heliophysics GI program (because NuSTAR isn't a Heliophysics mission). This does not leave us totally without opportunity, as the Heliophysics SR program and NuSTAR GO programs are appropriate, but it leaves us significantly less chance of getting funded because we are an interdisciplinary team.

the best proposals are simply not written

Not applicable

I was on a review team which considered coronagraphy at the same time as detector development for Origins. There were several proposals which did not fare well because some members on the review team favored only one discipline.

demonstration of interdisciplinary interest/benefit leads to pointing to the other disciplines that they should be responsible. Hence an important concept gets downgraded because it would benefit more than one discipline.

We have attempted to show in SiC mirror proposals that mirror stability needs to be evaluated in a closed-loop form, combining the effects of material stability, plus active thermal control and wavefront sensing and control. This is certainly acknowledged by the Decadal study groups. Yet the reviewers seemed fixated on material properties only -- ignoring the interdisciplinary nature of the ultimate application. So SiC, which is highly thermally controllable, was dismissed because it has a higher thermal expansion than ULE glass, which is 400x harder to control.

several concepts proposed to measure zodiacal dust/beyond zodi by piggy-backing on an outer planet mission.

At various times, the Kepler (and "FRESIP") proposals were categorized as part of the "small bodies" program with asteroids, and as fitting neither the NASA Astrophysics Program nor the Solar-System Exploration programs.

I am a stellar astronomer. I talk to solar physicists, and I would like to work on projects which can use the detailed data on the Sun and apply it to the diverse stellar environments. There is a real barrier to using solar data that could help interpret stellar data. When I have inquired about writing an ADAP proposal for such a project, I have been told that it can't fund exploration of Sun as a star projects. From my reading of opportunities in the heliophysics division, there is a similar restriction.

Starshade technology proposals submitted at early stage of concept development

After the Chelyabinsk impact, my team, which was actively modeling such impacts on Jupiter, sought funding to model Chelyabinsk in the context of observed Jovian impacts. We were the only team in the world with the models and experience to do this well. Published Chelyabinsk models had 1-2 orders of magnitude lower resolution than ours. This is the only modern impact that has economically affected humanity, and was a perfect laboratory for such small-scale threats. Congress has directed NASA to study and mitigate this threat. We aggressively and thoroughly marketed our proposal at both NASA (planetary and Earth science) and NSF. Absolutely everyone in both agencies a) said this was very important work, reasonably budgeted, not too expensive, and proposed by the right team with the right tools, and b) said it wasn't their problem or didn't fit any of their programs. We didn't even get a review. The work was never performed. With our tools, it would have been easy and highly impactful.