Writing a Better Research Proposal: Winter 2018

Jim Price
Physical Oceanography Department
Woods Hole Oceanographic Institution
508-289-2526
jprice@whoi.edu
https://www2.whoi.edu/staff/jprice/

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1 The premise of the workshop

We all have worthwhile ideas for our next research project. The question is, how are we going to pay for it?

If you are on the staff of a not-for-profit research organization like WHOI, then the short answer is that you will have to solicit the necessary resources by way of a research proposal — a formal, written document that describes your scientific goals and research plan — and that requests support from a funding agency. Your proposal will be just one among many worthy proposals, and these days the sum of resources requested collectively often exceeds the resources actually available by a factor of five to ten. More to the point then, you will have to write a research proposal that will in some way stand out in a crowded and competitive funding environment.

Writing such a proposal requires a great deal of thought and hard work, most of which is directed at issues that are highly technical and discipline-specific. As you would probably anticipate, the technical and scientific ideas that are advanced within a proposal are of the greatest importance, and you will have the opportunity to discuss your ideas and insights at some length during the workshop.

Granted the primacy of the scientific idea, it remains that a significant fraction of proposals that founder do so on issues that are not strictly technical or discipline-specific but that are nevertheless ’scientific’ in so far as science seeks to develop knowledge that can be communicated, evaluated and consolidated in a semi-public domain. The premise of this workshop is that many of these kinds of non-technical shortcomings can be mitigated if we write our proposal with a better understanding of who will read our proposal, and how it will be evaluated. In this respect, research proposals are rather different from the scientific papers and research seminars that you are used to writing and giving. This aspect of proposal writing is emphasized in this syllabus, and see also http://www.whoi.edu/science/PO/people/jprice/whoisgoingtoread.ppt

2 The who and the how of proposal evaluation

If the funding agency is the National Science Foundation (NSF), then the ’who’ starts with six to eight reviewers who will be selected by the agency to read your proposal and mail in their reviews. These mail reviewers will be experienced and knowledgeable scientists, and some, but not all, may be among the leading experts on the topic of your proposal. Their mail reviews will come to an ad hoc panel of about ten scientists and program managers who will then read the entire set of proposals and reviews, and then make specific recommendations to the senior NSF program manager regarding their relative merit and priority for funding. This ad hoc panel will be made up of mid-career scientists who are all highly accomplished. The expertise of these panel members will likely span the full range of the discipline being considered, and so these panel members are even less likely than the mail reviewers to be (the literal) experts on the topic of your proposal. But whether they are experts on your topic or not, it is this panel’s judgment that counts the most.

It may be only a small comfort to be told that the evaluation procedure followed by NSF is thorough and scrupulously fair (from my limited experience as a panel member and program manager). But it is also a product of real people who can be expected to react to a proposal in an altogether human way, with a mixture of hard, cold rational evaluation, along with an intuitive feeling that a proposal and the proposer are worthy of funding, or not. As we have already noted, most proposals are worthy from a strictly technical perspective, so why do some proposals succeed while others do not?

Successful proposals are likely to share two qualities: 1) an attractive, workable scientific idea framed as a testable hypothesis, and 2) a logical research plan that has been clearly and concisely communicated to a non-expert reader. We have already hinted at this last quality and it bears emphasis: mail reviewers and ad hoc panel members will be evaluating your proposal as a significant, additional burden on their already heavily-committed time and energy. If they have to struggle to understand what you aim to learn and how you are going to do it, then you are probably not going to get to do it, at least not with their money, not this time around. Given this circumstance of review and evaluation, research proposals, even more than research papers, must be clear and concise. In common with research papers, a research proposal should ideally also be rewarding to the reviewers and panel members who have been tasked to read it. We will discuss this last point during our first workshop meeting (Section 7, Week 1, below).

3 The plan and the goal

The plan is that each participant will write and revise a proposal as we go through the workshop, and our workshop meetings will consist mainly of oral presentations of the pieces that make up a proposal. It is not necessary that you be the sole author or even the senior author of your proposal, but you do have to be prepared to explain your proposal to the workshop participants, and you have to be willing to accept more or less graciously what may be ill-founded though well-intentioned criticism.

There are as many different kinds of proposals as there are funding agencies, and they generally have specific, arbitrary and possibly awkward formats. You must know and adhere to the format appropriate to your proposal, especially if you are responding to a specific call. Your department's Administrative Assistant will be a huge help with this. In what follows we are going to assume that our proposal is intended for the National Science Foundation (NSF) and that it is unsolicited. The distinguishing feature of this kind of proposal is that it must stand on its own. We will consider what can help to make such a proposal successful, and the unpleasant flip side of this, what may cause such a proposal to fail, even though it may carry the germ of good research.

The workshop will meet five times, more or less weekly, for about an hour and a half. Each meeting will consider a specific section of an NSF proposal, e.g., in week one, the Scientific Background, in week two, Statement of the Problem, etc. During our meetings, each participant will have a chance to deliver a brief, roughly five to ten minute-long oral presentation of a highlight or two from the appropriate section of their proposal. Clearly you won't be able to tell the workshop everything about your Statement of the Problem, say, in such a short time, but you should be able to tell us what you aim to learn by your research, and why the achievement of your goals will be important to your discipline and field. Another way to state the premise of this workshop is that you should be able to communicate a clear sense of your research proposal to workshop participants who are not within your discipline. This extra effort toward clarity — and, in careful measure, simplicity — will not be lost on the written proposal because, as we have pointed out, most of the ad hoc panel members and some of the mail reviewers will not be the leading experts on your specific topic. They will, however, be very knowledgeable generally and highly accomplished scientists, just like your workshop audience.

The assumption is that you will probably write or revise a given section of your proposal in the week or two after we have gone over that section in our workshop meeting, and after you have given the short, oral presentation to the workshop participants and received some feedback. You may find it helpful to find a proposal buddy with whom you can share and critique your written material. The workshop as a whole will exchange and review a written (hard copy) of the one page, Project Summary at our last meeting.

4 Scheduling for Winter 2018

This workshop is intended to be a positive help to you as plan and write a proposal. The timing of the workshop with respect to your personal schedule is therefore all-important. Ideally the workshop should begin a few weeks before the period when you will begin your final revision. To be on time for the mid-February 2019 NSF deadline, the workshop will thus need to begin in late November or early December (date and time to be decided) and meet about once per week until we complete six meetings in total, counting an organizational meeting. If it happens that you will not be writing a proposal on the schedule of this workshop, then you might be better off to wait for a future session; during the last ten years, this workshop has been offered twice yearly, in mid-winter and in mid-summer.

5 Resources

Our problem, having to write a successful research proposal, is one that we have in common with a very large and thoughtful population of scientists and academics. There is a correspondingly large and diffuse body of literature that can be a benefit to us, and weekly reading assignments have been drawn very selectively from this literature. These readings are important insofar as they will provide new ideas and insights into the proposal writing and judging process; without this new grist for our mill we would likely end up grinding away ever harder (but probably no better) along our present plan. This reading will require about an hour each week. At the start of most meetings I will try to elicit your reaction and questions to the week's reading assignment.

Readings are of three kinds:

1) How-to-do-it — Each week, one or a few chapters from Friedland, A. J., and C. L. Folt, 2009: Writing Successful Science Proposals. Yale Univ. Press. This is a useful, if somewhat elementary and dry reference. I suggest that you buy this online, costs about $15, or borrow a copy from a previous workshop participant. Referred to below as FF2009.

2) Other views on proposals — Most weeks, an essay that addresses some aspect of how to write a proposal. These essays are brief, mostly less than 10 pages, and available in electronic copy. One of these is a loosely-coordinated, multi-part essay published by the AAAS entitled 'How not to kill a grant application, Part xx', that will be referred to below as 'How not to kill'. These essays are somewhat redundant with one another and with FF2009, and yet each takes a point of view that has some merit. It goes almost without saying that some of the advice in these articles will not be directly relevant to our situation and a small part of this well-intentioned advice may even seem downright silly to us., e.g., several authors advise that proposal writing should begin a year in advance of submission (we've missed that deadline by about ten months). Clearly then, you will

have to screen this material to select the pieces that resonate favorably with you, while not becoming too alarmed by the parts that seem to come from another academic universe.

3) Deep background — Most weeks, we will have a brief article that deals with some aspect of science generally, e.g., Roald Hoffman's 'Why buy that theory?' assigned in week one is a perfect fit to this workshop. These articles are intended to support an important goal of the workshop — compared with a scientific paper, a research proposal must demonstrate an especially keen sense of scientific thinking and the intention to follow some kind of scientific research process. This part of the reading will help you to fill out the vocabulary of words and ideas that will be useful for describing this aspect of your research proposal. In that regard, this deep background reading may be much more valuable than you might first suppose.

6 The structure of an NSF proposal

The NSF Guide to Writing a Proposal (linked below under Additional Resources) lists dozens of required sections that pertain to financial and administrative regulation of grants. Some of these are essential, but most will come from your department's Administrative Assistant. We are going to devote our effort to just two sections: the 'Project Summary', a one page abstract of the proposal that we will consider last, and the 'Project Description', which is the main narrative that reviewers and the panel will read (you hope). The only clear-cut, NSF-imposed requirement for these sections is that they have to state explicitly the intellectual merit and the broader impacts of the proposed work; more on these in Section 9 below. Aside from that, the main narrative of the proposal can be structured any way that you choose. However, if you attempt a novel or unconventional format, then you run some risk of confusing your reader or in some way making extra work for them. Either can mean death for a proposal. Consequently, we will advocate a conventional format; a Project Summary and a seven part Project Description. The order in which we will discuss these sections has to do with acquainting the participants with our research problem.

I) Project Summary (taken up in week five)

II) Project Description

1) Results from prior NSF support (may contribute to 3) below)
2) Statement of the problem and its significance (the topic of week two)
3) Scientific background (a part of this in week one)
4) Hypothesis (week three)
5) Research plan (week four)
6) Expected results (week five, along with the Project Summary noted above)
7) Remarks on the Budget (we are not going to do this one, but be sure that you do!).

7 Schedule of topics

Week 1) Scientific background

Reading: 1) FF2009, Ch. 1. 2) Hoffmann, R., 2001. ’Why buy that theory?’, on reserve.

Aim of This Section: To bring the reader/reviewer up to the state of the art on the topic and problem area that you intend to propose on. This review should lead directly to the problem you aim to address (week two) and the hypothesis (week three). If there is an ongoing controversy, point it out and be fair to all sides. New figures that show how you can summarize an extensive body of data or that encapsulate an important idea can be very helpful.

Qualities: It is important that your review be thoughtful and critical as opposed to merely thorough and extensive. Excessive length is to be avoided; if the proposal is ten pages of text in total, then this section should be no more than about two pages.

One way to think of a proposal is that you are asking the reviewer for some pot of money so that you can go off and do more of — what? The proposal that the reviewer has in front of them at that very moment is a concrete sample of what you have to say about your topic and so you are implicitly asking to do more of the same. You had better use this chance to teach the reviewer something relevant and most of all, interesting. You should strive to include a thorough didactic discussion of some specific aspect of the problem area that is relevant and accessible to the reader. This little ’nugget’ could take up as much as a third or even half of the total review. Ideally it would be something that you have developed in your previous research and so may partially duplicate the Results From Prior Support section (that we are not going to discuss separately). But this material need not be the fruits of your previous research, it just needs to be interesting and rewarding to the reader. This is what we hope to hear from you in our first meeting.

If the reviewer comes away from this section feeling overwhelmed by a mass of seemingly disconnected facts, then he or she will be very unlikely to want to see more of the same. Indeed, this is a common problem among proposals that fail. On the other hand, if the reviewer comes away from this section feeling rewarded — that they have learned something of value — then the prospects for your proposal will soar.

Week 2) Statement of the problem

Reading: 1) FF2009, Ch. 8. 2) Ziman, J., 2000. ’What is science?’, on reserve. 3) Przeworski and Salomon, ‘On the Art of writing proposals’, on reserve.

Aim of This Section: This section is something like an extended, roughly two page, abstract. It serves to state the problem, and outlines the research steps needed for progress towards a solution. You have to make an argument for the Intellectual Merit of the proposed research, i.e., why your method and approach has a good chance to teach us something new and significant. And you have to say why or how what you will learn can be used by others, i.e., what you envision the Broader Impacts of this work will be (more on this in Section 8). Do not assume that readers will automatically appreciate the value of your research outcome.

Qualities: In this first few sentences of this section you should strive to gain the interest and the sympathy of the reviewers and the panel. It is appropriate to be slightly optimistic and confident regarding your proposed research, but of course not to the point of seeming to be a simple-minded salesperson or claiming impact that is clearly beyond the scope of the proposed program. This is a delicate balance, but you can introduce caveats in later sections. What you must do here is keep the reader moving along, primed and even a bit eager to hear more. This is perhaps the hardest section to write, but also obviously one of the most important. In fact, it may be the section to write last, after all the other pieces are in place. This section will likely be read by all of the reviewers and many of the panel members, and hence it has to be written in a way that is intelligible to a non-specialist audience.

Week 3) The hypothesis (or alternatives)

Reading: 1) FF2009 Ch. 7. 2) Popper, K. R., 'Science: Conjectures and Refutations', on reserve. 3) Generic Proposal Problems, on reserve. Extra: 4) Chamberlin, T. C., 'Multiple Hypotheses', on reserve. This is a very old, genuine classic, that will come across as extremely tedious. Take a look, but take it as optional.

Aim of This Section: Your hypothesis is, or could be, the most important thing in your proposal - it gives a concise description of what you regard as a possible solution to the problem that you posed in week two. Everything up to this point in the proposal should build towards the hypothesis, and everything that follows should explain how you plan to test or develop the hypothesis. The ideal hypothesis is one that is plausible, based upon present knowledge, but that could be shown to be wrong, i.e., is potentially refutable.

This hypothesis need not be entirely your own new idea. It would be great if it was, but many a Nobel prize has been awarded to an investigator who made a significant test of someone else's hypothesis. The creative aspect of your proposed research will then be mainly in the way that you plan to test the hypothesis, rather than the hypothesis itself.

Qualities: It is simplest here and in a proposal to discuss *the* hypothesis, as if there was only one. However, this may not accord well with what we (Earth scientists, biologists) actually do most of the time. Alternatively, one might choose to emphasize an interesting and important question, and then list several possible answers that your research could discriminate among. This amounts to having multiple hypothesis, which has considerable merit; it may be easier and more interesting to choose among several competing hypotheses than to decide pass or fail on one only. This shifts the emphasis away from the hypothesis (the possible answer) to the question. However, a complete shift toward the question could be problematic; reviewers will need to understand what you regard as the outline or form of a possible answer in order to tell whether the proposed research activity will be relevant.

It may be the case that your research plans do not grow naturally from hypotheses per se, but rather from the development of new methods that you and others (the reviewers?) can deploy toward the solution of longstanding problems. That being so, this emphasis on hypotheses as the central element of a proposal may seem unnatural and even mildly disingenuous. So, does every proposal have to be built around a hypothesis? No, there is no rule that you must have a hypothesis. However, the most common theme cited in unfavorable reviews of NSF proposals to Ocean Sciences was 'No testable hypothesis'. The second most common theme was that the 'proposal emphasized data collection rather than problem solving'. If your research is development-driven rather hypothesis-driven, then it is vitally important to set your development goals into the context of a clearly-defined problem so that you can explain explicitly why more or better measurements will advance science goals.

The intent of this section of the workshop is not just to frame and state a hypothesis, as desirable as that is, but to insure that science — we should state that more emphatically — **Science!** — rather than research activity alone, is the emphasis of your proposal.

Week 4) Research plan

Reading: 1) FF2009 Ch. 9. 2) How not to kill ..., Part 6: 'Developing your research plan.', on reserve.

Aim of This Section: If you have crafted a proposal that the reader has followed up to this point, then this section should be fairly straightforward – all that you have to do is lay out the sequence of steps that will lead directly to a test of your hypothesis or development goal. Any item that shows up in the budget, whether people or equipment, needs to be a part of this plan.

It is essential that you say explicitly how the measurements or calculations are going to be used to test your hypothesis. For example, say that your objective was to test an ocean circulation model; what quantities will you have to compute or observe to evaluate the model? How well and how much will you have to observe in order to make a decisive test? What would constitute a good or a poor comparison? Error estimates, even if fairly crude, can be very helpful, and null hypotheses or models, assuming that they are viable, can also be very useful as a standard of comparison. In effect, a null hypothesis (or model) will make a clear trial run of your experiment. The extent to which you can be quantitative will be a strong indicator of your preparation for and understanding of your proposed research program.

It is understood that the detailed plans laid out in a proposal will probably not survive contact with nature several years hence, and that something better or more efficient than your present plan will be implemented instead. However, the fact that you can discuss the adequacy of your proposed work in an explicit and quantitative manner will be taken as strong evidence that when the time does come to make changes, you will be prepared to make wise choices.

Qualities: Here's another place in the proposal where you can demonstrate that you have a sense of how to do science — that you have a plan of research that will very likely lead to useful, new knowledge that you can share with all of us. Evidence of a scientific mind set is just what is missing from many proposals that are otherwise stuffed full of important problems, facts, and plans for industrious computation and measurement.

An important question to ask at this stage is — Should I be concerned with the (high) cost of my proposal? The answer is yes, of course, but perhaps less than you might think. Assuming that your estimated costs are within the agency guidelines, then the scope and extent of your research plan should be consistent with a decision that you have already had to make, namely, is this research intended to be a pilot (or feasibility) study, or, is it intended to give a more or less definitive answer? If the former, then you should say so from the beginning, and emphasize that the scope and costs of the research have been carefully limited and are appropriate to the risks and benefits. In other words, if this is a pilot study, then scope and costs should be an explicit consideration all the way along, and including in the research plan. However, if this research is intended to be more less definitive, then your emphasis should be to lay out a logical and complete research plan that is fully adequate to the task. You still have to justify everything that you ask for, and you should be aware that a bloated or greedy request will draw the wrath of reviewers and program managers alike. But if your plan for a definitive experiment is logical and lean and within guidelines, then you have done your job, and it will be up to the program manager to figure out how to pay for as much as they can reasonably afford.

Week 5) Other pieces of the proposal

Title. The one thing that everyone will see, so make it interesting!

Abstract/Summary. Also exceedingly important, as it will be read by all or most of the panel. (Prepare a written abstract of the appropriate length to be distributed to the class.)

Expected results. Describe as explicitly as you can the likely payoff for society and for the scientific community. (Five minutes by each participant).

Reading: 1) FF2009, Ch. 5, 6. 2) Brennan, J., NSF proposal preparation, on reserve.

Aim and Qualities of the Summary: The abstract (or project summary) should be a complete, stand-alone, tour of the proposal - the problem and its scope/significance, your hypothesis, a description of the research needed to test the hypothesis, and a summary of the expected results. All of this, in one page! Emphasis should be on your hypothesis and the proposed work.

The present format for NSF proposals requires that a very explicit (titled) statement be made within the summary noting the **intellectual merit** of the proposed work, and the **broader impacts** resulting from the proposed work (more in the next section).

Aim and Qualities of the Expected Results: This short section, typically no more than a page or two, is a chance to list and briefly discuss the products that one can expect to see from this research. These can be of several different kinds: 1) Specific measurements or model code that you can almost certainly expect to make and can plan to contribute to some kind of national archive. 2) Outreach activities and whom they will benefit. 3) Scientific results, i.e., what you expect to learn and then teach your colleagues. This may be more speculative, of course, but properly, modestly stated it can serve as a useful guide to the intellectual territory that you intend to explore.

8 Additional resources

NSF, 'Guide to Writing an NSF Proposal'. This is the definitive source for the format of an NSF proposal. There is, however, no guidance whatever on how you might go about actually writing a successful proposal. For that you have to look elsewhere. Online from NSF http://nsf.gov/pubsys/ods/getpub.cfm?gpg

NSF, DUE, 'A Guide for Proposal Writing'. This is a very nice, concise guide for proposals to the directorate for undergraduate education. Unlike the guide just above, this does give advice on how to write a successful proposal. It has an explicit and lengthy statement of what is meant by 'intellectual merit' and 'broader impacts', albeit tuned for the field of education. Online at http://www.nsf.gov/pubs/1998/nsf9891/nsf9891.pdf

Kracier, J., 'The art of grantsmanship'. Online at http://www.hfsp.org/how/ArtOfGrants.htm This is a good, complete outline of a proposal, I recommend that you skim through this once.

Muller-Parker, G., 'How to get NSF funding: the inside view', on reserve.

Amy Bower's WHOI website includes a collection of recent, successful NSF proposals that you may want to take a look at. These are from Physical Oceanography, but should be a useful guide for any ocean science. To access these you will need a username and password that will be made available to participants.

9 NSF merit criteria (may surprise you)

NSF has decreed two criteria against which reviewers and the panel members are to evaluate proposals: 1) Intellectual Merit, which has to do with the quality of the proposed research and the PIs, and is what you would expect so we won't say more about it. And 2) Broader Impacts (BI), which is not obvious. I will briefly summarize what I think BI means and suggest how you might try to address it in your proposal, but you should also go to the definitive source, the NSF 'Guide to Writing an NSF Proposal' pages 21, 39 and 40, linked here in the section on Additional Resources, and the NSF, DUE 'A Guide to Proposal Writing', pages 5-7, also linked. Notice: It is now required that a proposal must explicitly address how it meets these two criteria. This statement should be in bold-titled sections that appear in the Summary and in the main Project Description. NSF has threatened to reject outright proposals that do not contain these explicit statements.

NSF's Criterion 2, BI, reflects a fairly recent (ca. 1998) mandate from Congress. NSF is still adjusting to this, and the importance of BI has varied from panel to panel. Several WHOI staff members have participated on panels where BI were not discussed at all. But then a few others had been on NSF panels where it had been a significant factor in deciding a proposal's rating. In one instance involving an interdisciplinary topic and panel, the BI were considered to be of major importance. Bottom line is that we have to be prepared for the possibility that BI just might be a significant factor in the way our proposals may be judged.

Within BI there are two major categories, Societal Relevance, and Educational Outreach and Development. Almost all ocean or Earth science research has potential societal relevance, but the issue here is how you plan to make your research available to those who could find it useful. Somewhat surprisingly, the discussion of BI at the NSF panels noted above was mainly on the issue of Educational Outreach. Proposals that had a viable, sensible educational component benefitted, while those that had nothing were penalized. Educational outreach can take many forms. The most obvious is to propose that a graduate student will be supported on the grant (though they often get knocked out during the budget negotiation). Developing educational content for web pages, hosting visiting students, or having colleagues who are from disadvantaged countries or disadvantaged social groups will also count. As with anything that is claimed to be a benefit of a given proposal, it is necessary to be quite specific about what the activity will be, how it will be implemented, and what costs will be incurred.

My personal advice is to plan something that seems natural to you and that is likely to be beneficial to your project overall, i.e., don't 'outreach' too far, just in case you have to do it! As one easy example, a web page is something that almost all of us use to facilitate communication with the world at large. Could your proposed data set or modelling activity be used to enhance your web page? Could you make your web page more useful and accessible to undergraduate students or college professors? This kind of activity will benefit every investigator in the long run, and should also qualify as a valid educational outreach activity if planned and described in an appropriate, public-friendly, way.

10 On advice and expectations for the workshop

Every workshop should have a mantra, and ours is that our proposals will be written to be

clear, concise and rewarding

to the small, over-worked audience of reviewers and panel members who have the very difficult task of ranking them and deciding the winners and losers. For most proposal writers this will be valuable advice. But we have to allow that there could be someone out there who already writes in a very direct and simple way, and for them to act on such advice could be counterproductive. No matter how appropriate and useful this line of advice might be on average, only you can decide whether it will benefit you.

Before ending, I want to acknowledge the obvious, that it is much easier for me to give you advice about how to write an effective proposal than it is for you to actually write that proposal. You may already have the feeling that writing a proposal is overwhelming — a near impossible mountain that you never set out to climb, and yet there it is, directly in the path of the research that you have chosen to pursue. It will be easier to start this journey if you have a realistic expectation for the finished proposal and for the workshop. In the first place, you can devote only a strictly limited time and energy to writing a proposal, which is, after all, an ad hoc document. For this reason alone, it is not realistic to expect that your proposal will be a perfect, literary masterpiece. Indeed, if perfection was our goal, then the result would be despair, mainly.

Thankfully, an all-around perfect proposal is not required. It was noted at the beginning of this syllabus that the competition for NSF support is fierce, if only because of the sheer number of competing proposals. However, the prevailing standard of these proposals is not extremely high (please don't quote me on this). Most proposals, while technically worthy and heart-breakingly sincere, are nonetheless turgid and tedious to read. This is especially telling when the circumstances of the review process are taken into account. To stand out from this crowd we need only explain in a clear and concise way 1) what we aim to learn and 2) how we are going to do it. If at the same time we can reward the reviewers and panel members with some small nugget of insight or knowledge, then we will have significantly improved our chances of success. These are modest but significant goals toward which we can make real progress during the course of this workshop.