Writing a Better Science Proposal: the Summer 2009 workshop

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1 The premise(s) 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 write a research proposal — a formal, written document that describes your scientific goals and research plan — that solicits the necessary resources 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 and succeed 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/scientific idea(s) developed within a proposal are its most important aspect, and you will have a chance to discuss your ideas and insights at some length during the workshop meetings.

Granted the primacy of the scientific idea, it remains that a significant fraction of proposals that founder do so on issues that are not 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 second premise of this workshop is that many of these kinds of non-technical shortcomings can be mitigated, and the odds for our proposal thereby improved, if we write our proposal with a clear understanding of who and how a proposal is going to be evaluated. In this very important respect, science research proposals are rather different from the scientific papers and research seminars that you are used to writing and giving. This crucial aspect of proposal writing is emphasized here (and in 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 and the how of proposal evaluation is fairly straightforward. The 'who' starts with six to eight reviewers who will be selected by the agency to read your proposal and mail in their reviews (aka, 'mail reviews' and 'mail reviewers'). These mail reviewers will be experienced and knowledgeable scientists, though probably not 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 make specific recommendations to the senior NSF program manager regarding their relative merit and funding priorities. This *ad hoc* panel is made up of mid-career scientists who are all highly accomplished. The panel members will span a very wide range of the discipline being considered, and so they are even less likely than the mail reviewers to be (literal) experts on the topic of your proposal. But whether they are the experts 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 most do not?

There are likely to be two broad qualities of a successful proposal: 1) an attractive, workable scientific idea, i.e., 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. It is crucial to appreciate that mail reviewers and *ad hoc* panel members will be dealing with your proposal as a significant, additional burden on their already heavily-committed time and energy. If they have to struggle to understand what you want to do, then you are probably not going to get to do it, at least not with their money, not this time around. Thus, it could hardly be overemphasized that research proposals, even more than research papers, must be *clear and concise*. In common with research papers, a research proposal should ideally be *rewarding* to the reviewers and panel members who have to read it. We will discuss this last point during our first workshop meeting (Section 7, Week 1, below). The 'how' then, is also fairly straightforward, at least in this minimal outline.

3 The plan and the goal

The plan of the workshop is that each participant will write or revise a proposal as we go along. The modest goal is that the end result will be more likely to succeed than it would have been in the absence of the workshop's guidance. It is not necessary that you be the sole author or even the senior author of your proposal, but you do have to be willing to explain the full proposal to the workshop, and you have to be willing to accept what may be ill-founded though well-intentioned criticism.

There are as many different kinds of proposals as there are funding agencies, and they have diverse, arbitrary and sometimes awkward formats. You must know and adhere to the appropriate format, especially when you are responding to a specific call. In what follows we are going to assume that our proposal is intended for 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, about once per week, and for about an hour and a half meeting at each meeting. Each meeting will consider a specific section of an NSF proposal, e.g., in week one, the Scientific Background section. During that meeting, each participant will have a chance to deliver a brief, roughly five to ten minute oral presentation of some highlight from the scientific background of their proposal. Clearly you won't be able to tell us everything in such a short time, but you should strive to teach us something interesting, i.e., something that will be rewarding for your proposal audience. Another way to state the second premise of this workshop is that you should be able to communicate a clear sense of your research problem, your hypothesis and your research plan to classmates who are not within your sub discipline. This extra effort toward clarity and, in careful measure, simplicity, will not be lost on the written proposal because most of the *ad hoc* panel members and many of the mail reviewers will not be leading experts on your specific topic, though they as we have said before, they will be very knowledgeable and accomplished, just like your workshop audience!

Along with these brief oral presentations, we will also exchange some limited written material from our proposals, i.e., the Project Summary page at the last meeting. 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.

4 Scheduling for Summer 2009

Participants can be from any WHOI department; the best case would be one participant from each department. In any event, the class size should be more than about three and less than about eight.

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 critically important. Ideally the workshop should begin during or no more than a few weeks before the period when you will begin writing your proposal. To be on time for the mid-August NSF deadline, the workshop will need to begin in mid to late June (date and time to be decided) and will meet once each week until the end of July; this gives five meetings in total, not counting an organizational meeting that will be scheduled in early June. 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 later session; since 2003 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 in many disciplines. There is a correspondingly large and diffuse body of literature that might be of benefit to us. Most meetings we will briefly discuss reading assignments that are drawn very selectively from this literature. These reading assignments are important insofar as they provide us with new ideas and insights into the proposal writing and judging process; without some 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 no more than about two hours per week.

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, 2000: *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 FF2000.

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 FF2000, 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, e.g., several suggest that proposal writing should begin a year in advance of submission. A small part of this well-intentioned advice may even seem downright silly to us. Clearly then, you will have to screen this material to select the pieces that suit you while discarding the parts that do not.

3) Deep background — Most weeks, a brief article that deals with some aspect of science, generally. These provide deep, deep background for our purpose, e.g., Ziman's 'What is Science?', and can be considered optional - of course, the entire workshop is optional. But consider this - 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 process. This part of the literature will help fill out the vocabulary of words and ideas that will be helpful for describing this aspect of your research, and in that regard this third kind of reading is more important 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, 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 readers and making extra work for them. Either way can spell 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 (to be the topic of week five)

II) Project Description

1) Results from prior NSF support (we are not going to discuss this section)

2) Statement of the problem and its significance (the topic of week two)

3) Scientific background (week one)

4) Hypothesis (week three)

5) Research plan (week four)

6) Expected results (week five, along with I above)

7) Remarks on the Budget (not this one).

7 Schedule of topics

Week 1) Scientific background

Reading for this week: 1) FF2000, Ch. 1, 8. 2) 'The art of writing proposals...', A. Przeworski and F. Salomon, on reserve. 3) Hoffmann, R., 2001. 'Why buy that theory?' On reserve.

1) 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 hypothesis (the next section of a proposal; the third week of our workshop) that forms the central idea or motive of the research project. If there is an ongoing controversy, point it out and be fair to all sides. Figures that summarize an extensive body of data or that encapsulate an important idea can be very helpful.

2) Desirable 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 to three 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 area and so you are implicitly asking to do more of the same. You had better use this chance to teach the reviewer something interesting and relevant. 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. 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 this 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 unlikely to want to see any more of the same. This is a very 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) FF2000, Ch. 3, 4. 2) 'Writing a good grant proposal', S. P. Jones and A. Bundy, online at http://research.microsoft.com/Users/simonpj/papers/Proposal.html 3) Ziman, J., 2000. 'What is science?' On reserve. 4) NSF Generic Proposal Problems, on reserve,

1) Aim This section is much like an extended, roughly two page, abstract. It serves to state the problem, your hypothesis and the research steps needed for progress towards a solution. What is different from a straight abstract is that this section should be quite explicit about the potential value of the prosed research. You have to make an argument for the Intellectual Merit (Section 8) 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 intend the Broader Impacts of this work to be (more on this in Section 8). Do not assume that readers will automatically appreciate the value of your research outcome.

2) Qualities In this first page or two of the proposal you must try 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 very 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. It may be the section to write last, after all the other pieces are in place. This section will be read by all of the reviewers and many of the panel members, and hence it has to be written for a nonspecialist audience. This is a bit different from the Scientific Background, which will be read mainly by the most interested and knowledgeable readers.

Week 3) The hypothesis (or alternatives)

Reading: 1) FF2000 Ch. 7. 2) How not to kill .., Part 3: 'So what?', On reserve. 3) Popper, K. R., 'Science: Conjectures and Refutations'. On reserve. 4) Chamberlin, T. C., 'Multiple Hypotheses'. On reserve. This last article is a (very) old classic, and may seem a bit tedious - treat it as optional.

1) Aim 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 Section

II and reviewed in Section I. 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 then the hypothesis itself.

2) Qualities/Comments

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 amounts to a shift of 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 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 primary to the structure 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 solely to state a hypothesis, 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) FF2000 Ch. 9. 2) How not to kill ..., Part 6: 'Developing your research plan.' On Reserve. 3) Popper, K. R., 1957: 'The aim of science.' On reserve.

1) Aim 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 know what to do.

2) 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 about 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

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

b) 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.)

c) Expected results, i.e., the likely payoff for society and for the scientific community. (Five minutes by each participant).

Reading: 1) FF2000, Ch. 5, 6, 10. 2) How not to kill ..., Parts 1, 2, 4 and 5. On reserve. 3) none this week.

1) 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 statement be made within the summary noting the intellectual merit of the proposed work, and the broader impacts resulting from the proposed work; take a look at the next section for more detail.

2) 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 on how to actually write 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.

Thackery, D. 'Proposal Writer's Guide', http://www.research.umich.edu/proposals/pwg/pwgcomplete.html?print Same comment as above.

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, 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. An explicit requirement is that a proposal must state up front how it addresses and meets these two criteria. This statement should be highlighted in some way, and it has to appear both in the Summary and in the main Project Description; I'd suggest within the Statement of the Problem, if possible. NSF has threatened to reject outright proposals that do not contain these 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 would benefit almost any project 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 a proposal should 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 to give advice about how to write an effective proposal than it is to actually write that proposal. You may already have the feeling that writing a proposal is overwhelming — a near impossible mountain that we never set out to climb, and yet there it is, directly in the path of the research that we have chosen to pursue. It will be easier to start this journey if we have a realistic expectation for our proposal and for the workshop. In the first place, we 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 our proposal will be perfect in all respects. Indeed, if perfection was our goal, then our 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 sincere and worthy, are nonetheless turgid and tedious to read, and 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 what we want to learn and explain how we are going to go about doing 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 that you can reasonably expect to attain during this workshop.