A Brief Manifesto on Writing Grant Proposals

(note: this is based on very limited success and a lot of failures)

**GRANT PROPOSALS**
Your proposal should be a reflection of your 'guiding questions' in research. These guiding questions should be of interest to your discipline and sub-discipline, and hopefully, society (i.e., your uncle in Boise). Guiding questions are broad, but reflect something unique that you are bringing to the table. You must be able to articulate your guiding question in 1-2 sentences that are mostly free of jargon. E.g., My guiding question is "How does the physical structure of rivers influences biological processes that occur within and around rivers."

**3 Fundamental Rules of Proposals:**
1. Proposals are skimmed, not read. It is only when you are really close to being funded will the proposal be read carefully and scrutinized. Different reviewers will focus on different sections, so each section has to be very strong. In general though, because they are skimmed initially, as Stuart Fisher says, "Get all your sex and violence in the beginning." Get your guiding question and cool idea up front, so the reader gets hooked. Try to convert them from a skimmer to a reader.

2. Reviewers are, at times, looking for reasons to reject proposals. Acceptance rates are getting absurdly low (5-10% for most national funding agencies). 50% are thrown away; 25% are resubmit; 15% are "love you as a friend", and 10% are funded. No mistakes or obvious overlooks. Note, however, that other funding approaches have higher acceptance rates but lower dollars. These are valuable to go after, particularly if NSF/EPA routes are getting too frustrating.

3. Someone has to champion your proposal. You have to convince a group of people that your idea is not only important, but is worth money. Funding is not like publishing; any paper can eventually get published, but not every idea is fundable. You have to convince a series of readers to be excited about your idea, and you have to convince someone to argue that your proposal should be funded.

**Getting Started towards a first draft:**
1. Your guiding question.
2. Your ideal project (if you could do anything, what would it be?). Articulate this in 2 paragraphs.
3. 2-5 sub-questions and associated hypotheses
4. A few graphs of what you think will happen (i.e., your hypotheses), and also guide what data you will collect.
5. Scale back the ideal project into a realistic one, but keep the ideal one in your head
6. Blend these into a 2 page proposal that is your boiler-plate for all subsequent proposals. You should always have a 2-3 page proposal handy for RFPs that come out of the blue, and for talking with professors.
   a. Have the 2 page ideal one for some funding groups and some communication
   b. Have 2 page realistic one for other funding groups and other communication
7. KEY: Identify what is the single key aspect of this proposal that merits it being funded; this is what you concentrate your proposal on (in terms of length). Sacrifice the rest of the proposal to make sure that the novel portion is fully developed, and that attention is brought to that portion.
   a. If novel concept, make sure that this concept is laid out clearly, and with multiple 'teaching styles' (text and figures)
   b. If a method, focus on this and make sure you show it will work (i.e., prelim data), or that you have the skills to develop it
   c. If unique data, then show how these data are fundamentally missing in existing research (although beware that funding agencies are wary of funding 'more data collection')

**Getting specific:**
1. Be hyper-organized
   a. The worst proposals are those that you can't follow the thought process; the last thing you want is a pissed off reviewer

      b.    I've found most papers are better organized than most proposals, which I find ironic and frustrating.

      c.    Make the organization of the proposal very clear.

            i.    I use a numbering system as this allows reviewers to focus on sections that they are interested in.

           ii.    I prefer bite-sized chunks of ideas and concepts rather than long un-broken text.

2. Literature review

      a.    My advisor always said that a reviewer should be nodding his/her head the whole time that they are reading the proposal, i.e., that you are logically leading them through the literature and showing them the way to your research questions. Every sentence/paragraph is there to lead them along the way without losing them. <u>The point of the literature review is to lead the reader logically to the research questions.</u>

      b.    Start big picture and work your way down to your specific project area; do this quickly because you won't get funded because of how great your lit review was.

      c.    The point of your lit review is to show that your key core idea (see above) is truly important in the field, is a natural extension of current paradigms in the field, and that you understand the disciplinary context for your research.

            i.    Note that if you are proposing something and saying that it will be a paradigm-changing study in your field, you better be right

      d.    Gauge the length of lit review needed based on what is novel or important about your proposed work;

      e.    Don't be too critical of previous studies in the literature – you never know who will review your proposal

      f.    Don't just review, SYNTHESIZE

            i.    Synthesize previous studies into a table, figure, something

           ii.    Running rule in my PhD lab at Purdue was all proposals alone should lead to a publishable manuscript (e.g., Progress in Physical Geography)

          iii.    Since most reviewers skim, a synthesis figure or table draws their attention to the main points as they skim

3. Research Questions

      a.    You have to use the literature review to lead to obvious research questions. Your research questions shouldn't come out of the blue to the reviewer. They should agree, based on your lit review, that these questions are critical in the field.

      b.    Tell the reviewer why the questions are important. Don't assume that they will have picked up on the subtlety of the literature review.

      c.    Tell the reviewer what is missing in previous studies. Be blatant about this and draw attention to this.

      d.    Draw attention to this section by using bold/underline, etc.

      e.    Lots of complex proposals with many hypotheses and sub-questions use flow charts.

4. Methods

      a.    Just like with lit review, scale the length and detail of the methods based on whether this is technicality or if developing new methods are the thrust of the proposed work. Are you proposing a fundamental new method, or are you going to use established methods?

      b.    Be only as specific as you need to be: reviewers are looking for reasons to reject, don't give them one in the methods.  Be generic and cite classics or 'methods papers or books'

      c.    However, you can only be generic if you have previous experience to show you can actually do what you say you are doing, or can illustrate via prelim data that you can do this stuff already and easily

      d.    Use either a novel study site or an already funded NSF/USDA study site

            i.    E.g., "The proposed work will occur at the NSF-funded Coweeta study site, thus leveraging off of the large datasets already available…"

           ii.    Tie in to other ongoing projects if possible, but be sure to point out how this will add a new dimension rather than just be more of the same

5. Hypothetical outcomes

      a.    The hypothetical outcomes section shows that you have thought about several things in detail

    i. How the system is likely to behave: this shows you will be able to interpret data

    ii. Where systems are likely to go non-linear or have thresholds: this would then be the focal point of the data collection

    iii. To get the reviewer interested in the study: if you can show what the eventual outcomes could be, then the reviewer can see the importance more easily.

  b. For physical/environmental proposals, I cannot emphasize strongly enough how important this section is.  It really helps the reader gauge your ability to do the project.

    i. Use hypothetical graphs

    ii. Use fast-and-frugal modeling

6. Preliminary and pilot data

  a. Running joke is that NSF proposals are showing the study that is already done and that the proposal is for work that you haven't even thought of yet

  b. Prelim data lets the reviewer know that you know how to collect the data; takes some of the pressure off of the methods section

  c. Shows the reviewer what kinds of things you can do with your data

  d. This is key if you are proposing a new method

  e. They show that the project can be implemented quickly

7. 'Broader impacts'

  a. If your work is only intellectually relevant, NSF or other group is your only route, and NSF is becoming less of an option (regardless of what Don Mitchell says in his Annals article on funding)

  b. Get students involved as assistants

    i. K-12 education gets over-used

    ii. Think about using undergrads as much as possible, particularly as independent study options so that you don't have to pay them as much and they get credit

  c. Use under-utilized ways of getting results into the hands of those who would be interested: workshops with real stakeholders at the end of your funded grant to tell them what you learned; people want to hear from researchers, they just don't know the avenues to engage them.

**After the First Draft is Written: adaptive management for getting it funded**

1. Know your audience

  a. NSF: fundamental science

    i. Investing in people and ideas, not projects

    ii. Investing in young scientists

  b. USDA: agriculture, sustainability, etc. Read RFPs

    i. Hyper-sensitive to details

  c. EPA: RFP driven

  d. NIH: ???

  e. US Forest Service: good example of a group that can have specific requests for proposals on specific work.  Funding rates are high; dollars are good but not great. These can be great connections to build through proposing on small, initial projects.

  f. Others: know what the mission of the agency/group is, and write around that mission. Don't propose fundamental new science to USDA; they don't care.

2. Know the review process

  a. Find out who the reviewers are (are they professors, extension officers, endowment people, students, city managers…)

  b. Find out what the review criteria are (intellectual merit, broader impacts)

  c. Try to find someone who has been on the panel; get the inside scoop

  d. What is funding rate

3. Talk to the program officer

  a. They will tell you what they want to see in the proposal; make sure you put it in there.

  b. They will recognize your name.

  c. Find out what has gotten funded, what hasn't gotten funded, and other details if possible.

  d. Send them a 1-2 pager (see above) and get their feedback.

  e. Do this early in the process.

        f.     <u>MEET THEM</u> (AAG Conference NSF is always there)

        g.    <u>New caveat:</u> a friend of mine in Marine Sciences said that the program officer there hated when people came by to talk about proposals; so I guess it is worth time to ask around and see what the situation is for your specific area.

4. Get it reviewed
   a. Ask others to review proposal. Note: grad students will tend to be good at reviewing details but not whether this proposal is fundable.
   b. Find someone in your department or that you know in the field who has been on a panel for the agency and ask for a quick review.
   c. For a specific program, find someone who has been funded by that program and call on them. It is actually a slight form of flattery.
   d. Getting a prelim review also helps find the stuff that you missed (e.g., typos, missing figures, etc.). These small errors won't sink the ship under the full review process, but as they accumulate, they start indicating sloppy preparation.
   e. Make sure when you submit a proposal on line (e.g., NSF Fastlane), that you check the converted document thoroughly. Be sure that the pages didn't get screwed up and that all the figures came out OK.
   f. Don't call on people too often; be selective for when you need it.
   g. Only send a very finished product; don't waste their time with a very rough draft.
   h. Generally have the draft you send be 75% of the length of the final proposal; this allows you some room to adapt based on their comments.
   i. If you don't get it reviewed until right before it is due, then you can't make meaningful changes; have a draft ready 2-3 months before due date so that you can rewrite with a month to go.

5. Review others
   a. As soon as possible, get on the review cycle for NSF or other agencies. You learn a lot more by being on the other side.
   b. Review your friends proposals freely.
      i. You learn how to write proposals
      ii. You learn the literature of other fields
      iii. You become known as a good reviewer

**When it Gets Rejected**
1. The rejection aftermath
   a. Don't react to rejection immediately; wait a few days for emotions to settle down. (I once called NSF program officer the minute I got rejected; that didn't go over very well as I hadn't processed the comments and was too ticked off to have a good conversation).
   b. Sit down with comments with a senior person who has been funded and walk through them.
   c. There will always be the rogue reviewer (my funded projects have all had a 'poor' in the mix). You usually can't do much about this one guy who missed the entire point of your proposal, but see what you can do.
      i. If there is similar criticism between reviews, these are key points to address.
      ii. Have to decide if you need to overhaul proposal, or just revise it
   d. <u>The worst review is 'goods' and 'very goods' across the board. This means that you haven't excited any of the reviewers.</u> Most nice reviewers will give goods for mediocre proposals, so many 'goods' are really 'nice idea, but don't fund it.' This is what I call "I like you as a friend but I won't go to prom with you" reviews. They are the death nail.
   e. If you got excellent review or reviews, what was it that excited the reviewers? Make sure that these aspects are coming out for the other reviewers next time.

2. Resubmitting
   a. Find out from program if they want you to explicitly address previous review comments. I highly recommend explicitly addressing the criticisms in the revision. Because panels change between cycles, the new panel members need to be aware of the previous panel criticisms. They may still have new ones, but at least they will know that you are taking their comments seriously.

b. Find out from program officer if you should resubmit or if it won't be a fundable idea no matter how many times you revise it

    c. Rule of thumb: you get two shots with an idea at an agency then it is time to move on; possibly a third shot?

3. Look for other programs to go after that are more appropriate

4. Revise and resubmit

5. Publish the lit review: you cannot drop the ball if you have written a good grant proposal. Particularly for young professors, you have to get some mileage out of the time you put into a grant proposal. However, if you got fairs and poors as reviews, the idea might not be that good. Take an honest look.


The CAREER proposal

1. The career proposal is a unique program at NSF that is a significant investment in junior professors within each discipline. It funds 5 years of research at a decent funding rate, but the odds of getting them are substantially low. Here is what I have learned about them.

2. CAREER awards are not normal proposals:

    a. You need stellar research statement

        i. You will not have room to put in a lot of details, so you need to be very thoughtful about what information needs to be in there and what information is 'standard';

        ii. Make sure that you are tying in to the cutting edge science of the day: identify what the big questions are in the field, what remains unknown, what direction is the field headed…

        iii. State your research goals very clearly, and show how your goals tie in to broad paradigms of your field: remember that they are to invest in you as the rising star of the discipline; you can't be a rising star within just your sub-discipline, your stuff has to be important across a wide range of your field

    b. You need an outreach/education section that is more than the token paragraph or page saying that you will help middle school kids realize how great science is

    c. You need to show that you are using this program as a career development program, and thus how your career will evolve over the course of the grant, and how the grant will serve as a springboard for your career.

3. Read a lot of them

    a. I read as many as I could get my hands on. There is no set formula for successful awards, but the more you read the more you get a sense of what communicates well and what doesn't

    b. Get a sense of what types of education and outreach people are doing; this really helped me a lot

4. It may not be worth going after one: your chances of success are higher with normal proposals, so particularly early in the pre-tenure process you should pursue smaller grants rather than the mother lode.

    a. This shows you can manage projects and grants

    b. It gets you some data and publications to show you are rising scholar

5. Education/Outreach

    a. This is a real tough one because this can be a significant portion of your budget/time if it gets funded

    b. Don't propose to develop a new program: instead find an existing program that you can tie in to and leverage off of. This provides you with people who really know what they are doing so that you don't have to put a lot of time and energy; I have found that what many education people want is help in developing more 'cutting edge' science into their existing programs. They are more than willing to do the work if you can help them get the early stuff thought through.

    c. Talk to people in the education school; find a professor who does science education.

d. Don't just say that you are going to develop a topic specific undergrad and/or grad course: I had a friend get really pissed when he saw this in a career proposal and said "You should be doing this anyway, without funding from NSF!"
e. My main advice about this education part is not to propose the normal fare: everyone talks about middle school education these days. Instead, the proposals I have been most excited about have been
   i. Working with local national park for revamping their natural history program for tourists (think about how many people can be taught geology this way…)
   ii. Developing a documentary for local PBS TV station
   iii. Developing a planetarium show (my personal favorite; it better get funded)
f. regardless, the goal is to get increased exposure to a lot of people, however you can do it. Get creative, because chances are that in-classroom activities aren't what got you interested in your field

Some words of caution:
1. Be sure that you actually want the grant; some grants can be so small that they are more work than they are worth; for my kind of research that line is somewhere around 20-40K.
2. Many very successful researchers build a string of grants from various funding agencies. I look at this as the mutual fund approach; diversity begets security. If you only go after pure funding from NSF, then you are likely to hit points when you are between grants and scrapping to get funding. The diversity of funding can be a headache though: reports, meeting, responding quickly to RFPs rather than being deliberate about proposal writing. The main caution is to maintain balance in it; use variety of funding, but don't give up on the "gold standard" of NSF, as they allow you to do, potentially, the best research.

FINALLY: there are points when we need time more than we need money. Writing proposals and managing grants is a time-consumptive lifestyle; be sure that you have enough money to get you the time you need to do GOOD work, not just do work.