

Tips for NIH Postdoctoral proposals (F32)

Gary Roberts, Professor of Bacteriology, University of Wisconsin-Madison, April 2006
(groberts@bact.wisc.edu)

The following is based on discussions at an F32 panel in March 2006. There are four criteria in the evaluation: candidate, sponsor and environment, research proposal and training potential.

1. Candidate. The main issues here are productivity and evaluations. Productivity is counted as papers accepted or at least submitted. A "manuscript in preparation" is worth something, but these are always of unclear credibility. If you do have a manuscript in preparation, you might mention its status very briefly under thesis research (and remind your PI to also do that in their letter). Most everyone has good-to-glowing letters, but reviewers are less persuaded by letters from people who will themselves benefit from your receiving the award, so be sure to have a couple reviewers who will not so benefit (thesis committee members, collaborators of your thesis advisor). An output of 2-3 first-author publications in good journals is considered very satisfactory - more is better.

2. Sponsor. The sponsor has to be a very credible scientist with a decent plan for your training. For credibility, it obviously helps if they have a Nobel prize or the like, but even a junior faculty member can build a compelling case if they are being productive, have good levels of funding and can create a training environment. The last term refers to any effort to help you grow as a scientist. This might be journal clubs, seminars, scheduled meetings or the like. In a few cases, mentors have set up small faculty committees who agreed to monitor the post-doctoral training and this was generally viewed as very positive. It was a strong negative for a candidate to go to a lab that was not recently productive, had any sort of funding issues or was at an institution without a fair amount of scientific infrastructure.

3. Research proposal. The research proposals were fairly important, but not nearly as important as they are for a standard NIH/NSF grant proposal. I would almost argue that most people hurt themselves more with their proposals than they helped themselves. That is, no one expects a new Ph.D. to write a proposal that is spectacularly creative or whose performance will likely cure cancer, and so you need not try to do that. You do, however, need to do the following: (i) Make it clear that you know the literature and have picked a problem that is worth doing. (ii) Write a clear, organized proposal that is not too overly ambitious. Proposing much too much shows that you don't understand what you are doing. (iii) You must build a case that you have a fair understanding of the experimental approaches and likely results. In general, reviewers do NOT need to read about all your buffers (unless that happens to be a common error in the specific method), but you cannot simply cite a reference as an explanation about how to do something that you have never done before. It is fine to explain that the sponsor's lab is really good at this method, but still you should show some understanding of the possible problems. (iv) You need to talk explicitly about pitfalls and propose alternative methods where you can. (v) Do NOT build a plan where everything depends on acquiring a result that is currently uncertain (i.e. finding a specific mutant, or developing a specific assay).

4. Training potential. This is the 800-pound gorilla in the proposal and an issue that many applicants and their sponsors mess up! The point of getting this postdoctoral award is so that you grow intellectually, technically, and organizationally as a scientist. So working on a project that is very similar to your thesis research is the kiss of death. Indeed, using very similar tools to those you used for your thesis is a strong negative. No one is excited about someone who has done biochemistry on membrane proteins in *E. coli* to propose to do biochemistry on membrane proteins in *Bacillus*. Doing the same thing in yeast or animal cells would be a little better, but still not a good idea. In a perfect world, you propose to learn new methodology in a new organism and with a new intellectual perspective (genetics vs biochemistry vs bioinformatics etc).

So what does this mean for the proposal? First, it means that the choice of the sponsor's lab is very important. If you are a structural biochemist and that's the focus of the sponsor's lab, then you have already something of a problem. Now this is manageable if you can explain all the new aspects of the proposed project and how it is importantly different from your background, but you absolutely must build that case! It is also acceptable to work on the same organism as used in your thesis (though not a good idea), and maybe even the same problem in the same organism (though again, not a good idea), but then you need to have a completely different intellectual and technical focus on the problem. Still, new thinking, organisms and skills will always be seen as positive and old anything will be at least a bit negative. To the extent that there are similarities between what you already know and what you will do, then you need to downplay these and focus on the new stuff. So this means that when you write the proposal itself, it's OK to build on some of your current intellectual and technical strengths, but if these are the focus of the various aims, then you are in trouble.

Now the problem with this is that you must therefore write a research proposal on something that, by definition, you don't know a hell of a lot about. So this means that if you have chosen a sponsor and project that provide new opportunities, then you will have to do some reading and you will have to get some guidance from the sponsor's lab so you can put together a credible proposal.

A final issue: When do you write the proposal? Some sponsors want you to apply before you come to their lab, and others expect you to write after you have been in the new lab for a bit. I think that either works. The challenge in the first approach is that you have a harder time writing the proposal (and learning the field) since you are still working on your thesis work. You also will then have fewer submitted manuscripts from your thesis and more "in preparation." You won't have preliminary data from the new project, but of course no one will hold that against you. The second approach, which is to write the proposal after you have been in the lab for some months, solves the first two problems (assuming that your thesis work gets submitted), but then people start expecting some evidence that you have hit the ground running. If you have been in the new lab for much over a year, people start expecting to see a manuscript in preparation (at least) from that work, so the bar starts going up.