INVESTIGATIVE RADIOLOGY
Volume 28, Supplement 2, S10–S12

Preparing a Grant Proposal

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WHEN ASKED to prepare a presentation on grant proposal writing, I was reluctant and perplexed; reluctant because there is something proprietary about the manner in which one prepares a grant proposal (if not proprietary, at least individual), and sharing it is a bit like sharing with the neighbors the location of the best clamming beds in the pond we live near each August; perplexed because I had never thought about proposal writing in a coherent, systematic way, although certainly everyone who has ever prepared a grant proposal has worked out some sort of strategy. Thus, although I have shed my reticence to discuss these matters openly, the reader should appreciate that this article is a personal musing and far from universal dogma.

Preparation for grant-proposal writing takes place months and years before setting pencil to paper. Just as it is necessary to receive training in clinical skills, it is necessary to undergo training in research if one aspires toward that. Medical and clinical scientists probably are made, not born. We would not skimp on clinical training in preparing a clinical radiologist; likewise, we cannot skimp on research training in preparing an investigative radiologist.

For those who would do research in this latter part of the 20th century, it is essential to be well grounded in modern biologic sciences, the physical sciences and engineering, or the quantitative statistical sciences; preferably, two or all of these. To be sure, the competition in other clinical disciplines is well prepared. Charlie Brown has advised that "a good education is the next best thing to a pushy mother." For grant-proposal writing, it is vice versa.

Most National Institutes of Health (NIH), Department of Energy (DOE), and National Science Foundation (NSF) proposals are written in English; thus, a command of the language is required, especially the ability to write simple, declarative sentences. If you do not have this ability, work on it. As in learning to read chest films, learning to write requires practice and skilled criticism.

Most good research ideas do not come out of the blue. They arise from an intimacy with and affection for a field of study. Good investigators are characterized by a deep desire to understand; they are willing to focus their attention on a fairly circumscribed field of knowledge. An investigator is expected to know where the frontier is, and knowing the folklore of the field is useful, as well. Contrary to popular legends, it is difficult for an ignorant and naive investigator to survive. When I become sufficiently interested in a field to consider working in it, I generally write a review of what I believe to be current understanding. Then I show it to an expert; only after several iterations do I get it right.

The most important part of a grant proposal is mental preparation; this aspect cannot be done against a deadline. Composting in the mind, like composting in the garden, takes many months. Of course, starting a new compost heap takes longer than adding to an existing one. When you are trying to think, isolate yourself from the distractions of everyday demands and allow yourself uninterrupted time. Some find a comfortable, relaxed atmosphere conducive to contemplation and creativity; others like to be poked from time to time.

What Reviewers Look For

One way to approach the matter of emphasis and priority is to examine what reviewers are asked to analyze and describe. They should be able to find these answers in your application.[1] They are asked to

1. Provide a description. "Clearly and concisely describe the objectives and procedures of the application. You may use the abstract provided." How pleas-

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Reprinted from *Investigative Radiology* 1987;22:250–252.

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ant for a reviewer to find that the abstract is a clear, concise description of objectives and procedures.

2. Provide a comprehensive evaluation of the application, or critique the significance and originality of proposed study in its scientific field (be specific and appropriately modest; no diagnostic radiology grants have actually cured cancer); validity of the hypothesis (avoid syllogisms: "Socrates is a man, a man is a featherless biped, a plucked goose is a featherless biped, ergo Socrates is a plucked goose"); logic of the aims (proposals aimed at counting the number of angels on the head of a pin generally receive low priority scores); feasibility and adequacy of the procedures (if you are going to count angels on the head of a pin, it is probably best to use a scanning electron microscope). Is the research likely to produce new data and concepts? (Anyone can produce new data; producing new concepts is really difficult.) Have alternate routes to the solution of the problem been provided? (Don't be too pejorative; if you criticize the Blankenstein method, Blankenstein is certain to be the reviewer.) For renewals and supplements, evaluate past progress (nothing succeeds like success, the rich get richer, etc. The best way to land grant number 2 is to have batted well in grant number 1).
3. Analyze the competence of the principal investigators and key staff to conduct proposed research in terms of academic qualifications, research experiences, productivity, and special attributes.
4. Describe the resources and environment, that is, special aspects of the facilities and equipment; extent of departmental cooperation; and availability of essential laboratory, clinical, animal, computer, and other resources.

When grades are to be given, it is always useful to know what instruments and system will be used as a basis for the grading!

Why Proposals Fail

Another way to approach the matter of emphasis and priority is to use back-projection by asking why proposals fail. An analysis of why NIH grant applications for clinical research fail was made by Janet Cuca of the Division of Research Grants at the NIH.[2] Her study indicates that grants are disapproved or receive low priority scores for the following reasons:

1. Experimental Design. Technical methodology that is questionable, unsuited, or defective.
2. Research Problem. A hypothesis that is ill defined, lacking, faulty, diffuse, or unwarranted.
3. Experimental Design. Data collection procedures that are confusing in design or that use inappropriate instrumentation, timing, or conditions.
4. Experimental Design. Study group or controls that are of inappropriate composition, number, or characteristics.
5. Experimental Design. Data management and analysis that are vague, unsophisticated, and not likely to provide accurate and clear-cut results.
6. Research Problem. Deficiency of significance—a proposal that is unimportant, unimaginative, or unlikely to provide new information.
7. Investigator. A principal investigator with inadequate expertise or familiarity with literature in the research area, poor past performance or productivity on an NIH grant, or insufficient time to be devoted to the project.
8. Resources. Inadequate institutional setting, support staff, laboratory facilities, equipment or personnel; restricted access to appropriate patient population; insufficient collaborative involvement of colleagues and co-investigators.

Cuca found that 65% of the low scoring is due to faulty experimental design (items 1, 3, 4, 5); 28% due to bad formulation of the research problem (items 2, 6); 6% due to investigator inexperience or poor performance (item 7); and only 1% due to inadequate institutional resources (item 8). Failing grant applications, whether clinical or nonclinical, each had, on average, 2.8 of these deficiencies.

Some Suggestions and Caveats about Proposal Writing

I will use the NIH format in this section, but do pay attention to the format of the agency to which you are applying in every instance. It is dismaying to a reviewer to think he is providing a second-string review in case another application fails.

Titles are important. First, at the NIH you could be in real trouble if your proposal ends up in the wrong study section, the members of which may not know your field and may even be hostile toward it (particularly when money is short). More important, they may not know you or your accomplishments. For clinical studies, most of your proposals should end in the radiology study section; don't be clever and have yours end up in molecular biology. Second, provide a revealing, descriptive title—the reviewer senses immediately the topic in which you are interested, the agency staff knows how to catalogue your proposal, and colleagues and competitors can figure out what you are doing if you are funded (they never find out what you are doing if unfunded).

Along with the title, an important means for routing your proposal to the proper study section is the abstract or summary. Generally, it is the first thing read by a primary reviewer; it may be the only thing read by other reviewers. A useful device is to write the summary several days after the application itself has been finished and, then, from memory. This helps convey the sense of what you want to do without listing in-depth details.

The budget should be reasonable, believable, well researched, and superbly justified. A word about percent effort; this can be a real problem for investigators who are practicing clinicians; it is one of the reasons that radiologic scientists rather than clinical radiologists end up with the lion's share of the research monies. Be realistic, but do obtain from your department head enough unencumbered research time if he or she wants you to be successful in research.

In preparing your biographic sketch, follow the instructions; sometimes the chronology reads up, sometimes it reads down. Use your head in making entries on the sketch and list relevant appointments, major experiences, and significant honors. Your appointment to the staff of an obscure nursing home will not impress a reviewer—he may even think you were moonlighting. A mathematics medal from high school is not a significant honor. In listing your other research support, be honest; in the computer age, there is no place to hide.

In the resources and environment section, list the important information. Most medical school grant offices furnish help with the bureaucratic material. Be sure that your intended collaborators provide written consent.

The research plan has several sections:

1. Specific aims. What do you actually hope to accomplish? An explicit hypothesis may be helpful here—there is often an implicit one. An example follows.

   Hypothesis: The laughing gland of the hyena is energized by the happysomes, and the tonality of the sound is determined by specialized structures on the cell surface called "ha-has."

   Specific aim I: To isolate happysomes by gradient centrifugation.

   Specific aim II: To study their oxidative phosphorylation in happy and sad culture medium.

   Specific aim III: To characterize the cell-surface structures by transmission and scanning electron micrography.

2. Significance. This section should have two parts: background and statement of importance. In background, demonstrate your understanding of the subject. Make clear what work was done by whom. Your choice of references often reveals your understanding of the subject; it can also point out that you are using a MEDLARS list for the first time. (Use titles as well as authors, be up-to-date and be selective.) Under importance, indicate where this work fits in with the development of a field: understanding X, settling a controversy about Y, diagnosing and treating Z.
3. Progress report. Be concise. Provide enough detail to support your argument, observations, and conclusions, but do not provide every experimental detail (especially if there are manuscripts in the appendix). The use of small tables and figures is encouraged; reduction photocopying can help.
4. Experimental design. This is so important that it pays to write this section right after specific aims, certainly before one collapses from grant-proposal-preparation fatigue. It is handy to begin with the research strategy so that a reviewer can see your plans. If there are materials and methods to be used repetitively, state and/or reference them. For particular approaches, provide significant detail. You must convince the reviewer that you have the expertise to do what you propose and the wherewithal to do it. What kinds of data will you get? How will you analyze the data? What might go wrong? How will you know what you will do under those circumstances?

When you have finished the proposal, have a critic read it; friendly, uncritical readers are useless. Certainly your colleagues and collaborators should be involved in the process. Everyone should be tough and skeptical.

Conclusion

A grant proposal, like an examination, is a document to be read by a specific individual or groups of individuals who are going to give it a grade. That individual is likely to be overworked and tired of reading research proposals. At least, keep yours neat, complete, and succinct. Better still, capture his/her attention with its originality, clarity, and brilliance.

References

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2. Cuca JM. NIH grant applications for clinical research: reasons for poor ratings or disapproval. Clin Res 1983; 31:453–461.