Ten Fatal Mistakes in Grant Writing

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Ten very basic and almost invariably fatal mistakes made by principal investigators when they are preparing grant proposals are listed and discussed.

I have just stepped down as the Chair of the National Institute on Drug Abuse (NIDA) Initial Review Group for Epidemiology, Prevention, and Services Research. The massive title means only that I chaired the meetings of the group of scientists who reviewed the grant proposals that many psychologists and other scientists submitted: grants to study epidemiological, social, psychological, or treatment aspects of drug abuse. Our primary task was to decide whether the research had scientific merit. If, in our judgment, the proposed study was good science, we then assigned a priority score to the grant: a score showing how much we thought the research would contribute to scientific knowledge about drug abuse.

Those ratings were extremely important; they almost always determined whether a proposal could eventually be funded. While I was on the committee, not one single grant that the committee rejected was funded. Furthermore, more than 90% of the time, the proposals that were funded were selected from those that had the best priority numbers.

I do not want to discuss how to prepare a good grant proposal or even list all of the little things that can go wrong. There are excellent materials available elsewhere on how to write a grant (Gordon, 1978; Holtz, 1979; Lindholm, Marin, & Lopez, 1982). After participating in this review process for several years, however, I decided that there were a few very basic mistakes that people made, mistakes that almost always led to rejection of their proposals. I saw these same fatal flaws time and again at NIDA, and also when serving as a special reviewer for other agencies. Avoiding these errors might keep one from wasting a lot of time and effort in preparing a proposal that would have almost no chance of being funded.

Mistake 1: ‘‘Let’s Get a Grant to Pay for Treatment’’

Careful reading of the goals of the grant and the budget reveals that the primary aim is to get more treatment staff or to fund treatment staff that have been cut by budget reductions. The research plan is weak, and the emphasis is on how much good will be done by providing service.

It is, of course, possible to provide service on a research grant, if it is an integral part of the research. A project, for example, could be conducted to test treatment effectiveness, if the research plan would actually allow determination of what kind of treatment worked, for whom it worked, and how well it worked. Many proposals, however, are dominated by service delivery and include only a sparse and patched-together research plan. They are often, but not always, submitted by a service agency, with a research plan added by an academic from a nearby institution. The goal is clearly to provide service, not to answer important questions about how and why treatment works. Even if the committee agrees that providing the service is laudable, they still must rate the project in terms of its contribution to science, and just providing service does not usually add much to knowledge.

Mistake 2: Signing Up for the Wrong Race

If you were a sprinter, fast but without much endurance, it would be a blatant error to sign up for a marathon. Despite this obvious principle, NIDA receives grant proposals from scientists that do not enable them to use their greatest strengths, and instead focus on areas in which they are relatively weak. I have deep sympathy for some of the personal and professional goals that lead to this error. One researcher, for example, applies for a grant to study an idea that is totally novel and does not fit into his or her past research. Another scientist is trying to use grant funds to retrain, to move into a different area of research. Still another scientist, highly skilled and experienced in psychological assessment, tries to be thorough and includes extensive physiological measures in the proposal despite minimal experience with physiological measurement.

In an ideal world, all of these behaviors should be encouraged. Unfortunately, given current conditions of funding, any of these proposals is likely to end up being rejected. A decade ago, when more grant funds were available, a proposal that was only reasonably good, or one that entailed considerable risk of failure, might have been funded. Today, a proposal has to be very strong to be funded, and that usually means that the risk of failure has to be low.

It is hard enough to design a tight and carefully controlled experiment when you already have considerable research experience in an area. When the idea is really new, creating innovative measurement techniques and experimental meth-
ods can be very difficult, and a proposal is likely to leave doubts in the minds of at least some members of the committee. Sadly, funding for research is tight enough so that even one or two committee members with doubts can be enough to put a priority score out of the funding range. The scientist who is trying to retrain or move into a new area, or the one who includes areas outside of his or her expertise, is at a real disadvantage: the proposal will be read by people who are already skilled and knowledgeable in that other area, and the proposal is likely to look unsophisticated and weak to those experts.

A strong consultant or a co-principal investigator (Co-PI) who is an expert in the new area could solve some of these problems. All too often, however, the consultant is an afterthought and was not deeply involved in writing the proposal or in designing the study, and the proposal remains weak. Investigators might be better off if they thought seriously about their strengths and considered how they might contribute to knowledge by using their existing talents, skills, and experience, while retraining or polishing new skills or innovative ideas until they can be winners.

Mistake 3: “Trust Me — I’m an Expert!”

The principal investigator does not provide details of how important tasks will be done, pointing only to a past record of solving similar problems. This may appear to be arrogance, but there is a good chance that it is not. Part of the problem that experienced researchers have is that they are excruciatingly familiar with a particular aspect of the problem or with a particular method. It is hard to remember that others may not have the same familiarity.

Being famous is definitely not enough to automatically get you a grant. No matter how “important” the scientist is, or how experienced, providing complete details in the grant proposal is essential. Proposals are not reviewed “blindly.” In fact, part of the committee’s task is to evaluate the credentials of the investigator and to judge whether the investigator is competent to complete the project and likely to succeed. Having a strong publication record can be an asset; the past record is important because it demonstrates that the principal investigator (PI) does have the capability to complete research and bring it to fruition. But in some ways, the committee expects more of an established scientist. An experienced researcher is expected to have a firm grasp of the literature and should have the ability to write a proposal in which he or she perfectly describes procedures, methods, and instrumentation. If the proposal is not excellent, the committee is likely to wonder why. Although committee members try to maintain complete objectivity, they are human and are likely to be more critical of the expert’s proposal than if it came from a relative novice.

Sometimes young investigators feel that they are at a disadvantage, but this may not be entirely true. The new investigator, proposing a reasonably well-designed study, is often viewed with considerable sympathy. The risk with a new investigator may be somewhat higher, but in assigning a priority score, the committee may weigh the value of recruiting and encouraging a new scientist fairly heavily, particularly if the cost of the study is reasonable. When this weight is added to the perceived value of producing the study, a proposal may receive a very favorable priority number.

It is not only senior scientists who fail to provide enough detail. The single most frequent reason for rejecting a reasonably good proposal or approving it with a low priority number may be that it did not include enough detailed information.

Mistake 4: Ignoring the “Pink Sheets”

When your grant proposal is evaluated by the committee, you get feedback. The results of the review are printed on baby pink paper. If you really want to have your research funded, these “pink sheets” are more precious than rubies. They tell you, in detail, just what that committee saw as the strengths and the weaknesses of the proposal.

When you resubmit the proposal, the information on the pink sheets can be used to emphasize the strengths and to deal specifically and clearly with every weakness in the proposal. This may seem obvious, but many investigators do not resubmit, and others send the proposal back with only cosmetic changes and do not deal with the weaknesses that were clearly specified on the pink sheets. Dealing directly and effectively with every weakness does not guarantee that you will be funded, but ignoring a weakness or glossing over it does ensure that you will be rejected again.

Incidentally, a criticism of your proposal does not necessarily mean that you are “wrong.” Your approach could be perfectly appropriate, but you need to communicate better, showing exactly why what you plan to do is the best way to do it. You do not have to refute the pink sheets; you can clarify, present the material from a different angle, or provide what is missing to make your point clear. Why are you doing it in this way? Why are you doing this project at all? A criticism on the pink sheets means that you have not convinced the committee.

The pink sheets may also show that you were approved, but with a poor priority. This means that the committee thought that your proposal had scientific merit and did not have any fatal flaws, but that you have not convinced the committee that the result, as you planned the study, would be highly valuable. The criticisms provide a good set of ideas about how you might modify your proposal, but you cannot just answer the questions. You need to rethink the proposal, clean it up, make it really relevant and important. In short, make it better science.

Mistake 5: “I’m Going to Develop a Scale”

When I see this statement on a proposal, I always hear Andy Hardy shouting, “Let’s do a show!” Andy thought that doing a show was going to solve everyone’s problems. In all too many grant proposals, “I will build a scale” is presented in the same way: as the solution to all of the project’s problems.

Building a scale is usually listed as a supposedly minor
part of the project. The author finds that there is no already tested method for assessing one or more of the variables that have to be measured. The answer is to build a scale. That would be fine, but the proposals that are likely to be rejected usually present "building a scale" as a simple solution to the problem. There may be a few short sentences mentioning that the scale will be tested for reliability, but the author provides no details to show that he or she is aware of the technical steps needed to construct a reliable and valid measure, or of how difficult the task really is.

Unless, for example, the author includes samples of items that may appear on the scale, the committee may find it difficult to even begin evaluating whether a scale will be able to take its place as an important part of the research. Experienced researchers and the members of the committee have a great deal of experience, know how hard it is to write good items, and are likely to want to see what kinds of items the author is capable of creating. Are they clear? Will the research subjects be able to read and understand them? Could they have more than one meaning? Do they relate to the construct being assessed?

The PI sometimes mentions testing the new scale for reliability but does not mention what will happen if the scale is not reliable. There is no pilot test, no opportunity for revision, and no alternative plans, simply an assumption that the scale will be reliable. The reviewer has no choice but to mark a proposal like this unfavorably because if the scale does not work, the research is usually of little or no value.

Constructing a reliable and valid scale is a very difficult, technically complex, and challenging task. If a new measure must be constructed for a study, the PI has to show in-depth awareness of the theoretical and practical problems involved, show knowledge about where and when things might go wrong, and show how adjustments and alterations can be made so that eventually an adequate measure will be produced. If the value of the research depends totally on the adequacy of a new scale that is yet to be constructed, it is essential to present at least pilot data showing that there is very good reason for believing that with a minimum of further work, a reliable and valid scale will be produced.

Mistake 6: "If I 'Tack On' Drug Abuse, NIDA Will Fund My Research"

This kind of proposal usually results when the principal investigator has a consistent and ongoing research program in an area other than drug abuse. Sometimes funding has been cut off by another agency; sometimes the scientist wants to expand research into new areas. The result is a proposal that can be very strong in those parts that deal with the investigator's true love and very superficial in the parts that link that research with drug abuse.

The committee has little choice but to reject the resulting proposal because it would usually contribute little to scientific knowledge about drug abuse. Rejection is often accompanied by real regret because drug use, like other human behaviors, is cross-linked to a very wide range of human problems and personal and social characteristics, because the investigator is often a very good researcher, and because the proposal sometimes presents interesting possibilities and ideas.

The solution is obvious: the scientist sending a proposal to NIDA has to recognize that drug use is as important to the proposed research as the rest of the study and warrants the same in-depth review of the literature and the same careful and sophisticated consideration as the other variables in the study. One approach might be to include a Co-PI or consultant who is experienced in research on drug abuse and who has a major involvement in preparing the proposal. Another is simply to do your homework: to truly study the literature in drug use as you would the literature related to your own initial field of interest.

Mistake 7: The "'No-Problem' Problem"

Suppose one proposed a study to examine drug use by high school superintendents, or another to provide expensive drug avoidance training for institutionalized handicapped youth. In neither of these situations is there any evidence that a real drug use problem exists, and in both of them there is fairly good reason to believe that there will be relatively minimal drug involvement. Neither study seems to attack a real problem.

Another kind of "no-problem" involves the hypothesis that is hardly worth testing: for example, "I am going to find out how the make of car that a person owns relates to drug use."

You could, of course, make a case that no one really knows whether high school superintendents have drug problems. They do have high-stress jobs and might be tempted to use drugs to relieve that stress. If no hard choices about research funding had to be made, an argument that high school superintendents are an important group and that people should know about their drug use might be cogent.

You might also draw up some inferences about what automobiles symbolize to people and how particular drugs match those symbols. You might also argue that people could learn something about Freudian theory from the relationship between cars and drugs. But research funds are severely limited. When there are obvious and serious drug problems, cogent and important theoretical hypotheses that need to be tested, and not enough funds to do the research that really needs to be done, a proposal to study a "non-problem" is likely to get short shrift.

The argument "little is known about . . . " is not a very good one. Little is known about making love in a canoe while standing up, but then again, why bother? If you really believe that what you want to study represents a serious problem, then you will probably have to do the pilot research to demonstrate that fact, and write a very persuasive section on significance of the research, before you will get funding.

Mistake 8: Excuses—Explaining Why the Study Cannot Be Done Right

"The administrators of the clinic would not agree to a control group."
"Subjects in School D cannot be given
good research, not to fund your research. An excuse counts
for nothing; it is only the quality of the research that will
actually be done that counts. If a control group or a saliva
test would be important to producing a good study, and if
either one is not possible, then the study is not worth fund-
ing as it stands.

This is not to say that control groups or saliva tests or
any other specific conditions are essential for good re-
search. If, however, particular experimental conditions or
methods would ordinarily be needed for a good research
plan and cannot, for some reason, be provided, you have
to present a strong case showing why they are not essential
parts of your proposed study.

Failure to provide control conditions in treatment or pre-
vention studies can be particularly serious. Although the
reviewers know that they are difficult to construct and that
it is hard to obtain approval for them, not having adequate
comparisons or controls may not only damage the ability of
the study to reach conclusions, but also suggests that the
PI may not have real control over other essential conditions
of the study. If administrators or staff feel the need to dic-
tate conditions and do not understand or are not in sympathy
with research goals and needs, it may be symptomatic of
other potential problems. What other changes might be dic-
tated by treatment staff or administration either before or
during the course of the study that may negate the value of
the research?

Mistake 9: Using Inappropriate Tests or Measures

I can list only a few of the many different ways that this
mistake has been made:
1. Giving a test or scale developed on college students
to 7th grade students, and not presenting any data showing
that it is reliable or valid for that group;
2. Giving a test validated on one cultural group to an-
other, with no recognition of the need to dictate
conditions and do not understand or are not in sympathy
with research goals and needs, it may be symptomatic of
other potential problems. What other changes might be dic-
tated by treatment staff or administration either before or
during the course of the study that may negate the value of
the research?

7. Presenting pilot data on a test, but data with internal
inconsistencies that show that the data are inaccurate;
8. Picking a measure of a particular characteristic on the
basis of the name of the test or trait, when examination of
the test items would show that it would not enable one to
assess what the investigator really wants to measure;
9. Giving so many tests or repeating tests so often that
they are almost bound to be reactive and lead to inaccurate
results;
10. Changing the method of test administration from
written to oral without considering possible effects.

Mistake 10: The Critical Mistake
That One Never Sees

A minister prayed again and again, asking to win the
lottery in order to use the money for good works. Time and
again the lottery went by and the minister did not win.
Finally one night after a hard session of prayer, the minister
asked, "Why don't you answer my prayers? I am a good
man and I have promised to use the money for good works.
Why won't you let me win the lottery?"

Suddenly a voice spoke from the heavens, "Buy a ticket!"
The biggest mistake of all is not to write a proposal. It
is absolutely fatal. You cannot get funded without trying.

Writing a proposal and having it rejected is painful. You
will invest a lot of work, and will almost undoubtedly invest
considerable ego. Being rejected is no fun. But it is not
necessarily a waste of time even then. There are secondary
benefits that can be important. Writing a proposal makes
you consider seriously the significance of what you are
doing, forces you to review the recent literature, and en-
courages detailed planning of your next research efforts.
The pink sheets provide feedback on your effort. The ex-
ercise sharpens your thinking and creativity, and will im-
prove your work regardless of whether you are funded.

And there is always the chance that you can convince
the committee that your ideas will really advance science.
Then you will have money and time to invest in your re-
search that you could not hope to get in any other way. Go
ahead and "Buy a ticket!"

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