

How to Write a *Losing* Proposal

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No one wants to write a *Losing Proposal*. But knowing the most common ways to generate a loser is one step to avoiding pitfalls and thus writing a winner.

DISCLAIMER: Five of the following items were recognized in 1990-91 when AS was a "rotator" (one-year Program Officer) in the Chemistry Division of the National Science Foundation (NSF). The sixth became evident in more recent years. These insights are NOT NSF or U.S. Government policy, no one at NSF has commented on the accuracy, relevancy, or timeliness of these comments, and the author assumes no liability for the use anyone might make of the insights. However, it has been the experience of a number of people in the last 15 years that these strategies are in concert with funding trends in science.

And now, the *losing* approaches!

1. Propose something that's *already been done*.

Why this loses: Science is supported to generate new capabilities and new opportunities. Redoing what's already been done consumes resources without generating economic benefit. "What if the old work might be wrong?" Cast the issue as a new problem. "But I thought the scientific method rewarded those who checked that earlier work was right?" Rechecking may be prudent or necessary while carrying out your new idea, but grants are funded to do such rechecking, not because of granting agency policies, but because of the reaction of reviewers to proposals to revisit what's already in the literature.

Example: You worked on a project as a graduate student and know some details that could have been done better. So you propose to redo the weaker parts of your graduate thesis – or of the thesis of the person across the corridor.

2. Write a *review article* instead of a proposal.

Why this loses: Reviewers want to know what you plan to do; history only sets a context.

Example: For a 15 page proposal, write either 13 pages review and 2 pages of proposed work, or 11 pages review, 2 pages preliminary data, 2 pages proposed work.

3. Be blinded by *subfield boundaries*.

Why this loses: the average citizen couldn't care less IF your sub-specialty exists, much less about its assets and limitations. Any real problem requires a multi-faceted approach to solution. It's the solution, not the tools, that matter to whoever supports your work. If you can't solve the whole problem yourself, get a collaborator who can compensate for what you can't do alone. Corollary: share your expertise with others.

Example: suppose you're an analytical chemist (like the author). Synthetic chemistry is NOT something we do very often or very creatively. If a problem requires novel molecules to matter, but the Sigma-Aldrich catalog has other molecules that can show only proof-of-principle, DON'T just use the catalog compounds. Get a collaborator to make the molecules that are most appropriate to the question at hand.

4. Have a *solution* looking for a *problem*.

Why this loses: it's been observed that if the only tool one has is a hammer, the whole world looks like a nail. A proposal that says, "I have a hammer. Let me apply it to your nail" will fail if it turns out that that nail is actually a screw, a light-bulb, or a delicate flower. Research needs to solve Real Problems (of which the world has many!). If you've identified a problem where your favorite tool can help, focus the reviewer's attention on how you will actually solve the problem, not on the fact that your tool is a good tool. Such focus invites the reviewer to think of 3 other tools that are better, and 5 reasons you've conveniently omitted about where your tool has limitations.

Example: melatonin is a "hot molecule" in sleep research. So any chromatographer saying, "I'll optimize the separation and quantification of melatonin" falls into trap 4 unless one can point to some biological problem that can not yet be solved because of inadequacies in the separation of melatonin from other biomolecules.

5. Find someone else's *bandwagon* and climb aboard.

Why this loses: whoever built the bandwagon is likely funded and will always run faster than you can in your attempt to catch up and then surpass the "someone else's" achievements. Make your own bandwagon; do something so clever that other people decide to chase you (and let 'em eat your dust!).

Example: anyone who started to do research in the fundamentals of electrospray mass spectrometry after it was announced that John Fenn got the 2003 Nobel Prize for inventing ESMS fell into this trap.

6. Study something *abstractly interesting* but of no practical value.

Why this loses: there are so many practical problems that all the money gets spent on them. Much as this author laments the passing of the "pure science" era (where one did science to learn and understand, not necessarily to profit economically), that era is over (some maintain it was a fiction, even in the '60's).

Example: If faced with funding a proposal that might cure cancer or one that might provide some new theory of why more chemists are left-handed than in the population as a whole, which gets the money? There are thousands of people who can live without knowing the answer to the latter, but not the former.

In addition to the six fatal errors, it is important to have sufficient preliminary data in most cases. "But how do I fund the collection of preliminary data?" Visit someone else's lab where the necessary equipment is in place. "Bootleg" funds from prior support or startup funds to get things going. Cite literature work showing what you want to do is practical and connect that to work you've published. Your goal is to make the reviewer think: "This is great stuff! It can't fail. The ONLY thing preventing this ground-breaking, critically important work is MONEY, and all I have to say is "Excellent!" for this obstacle to be removed."