5 Common Mistakes That Will Sink Your Grant

The challenge that all reviewers face as they try to separate the outstanding from the merely good is to convert their intuitive, emotional response to a grant into a series of bullet points that encapsulate the proposal's strengths and weaknesses. Avoiding these pitfalls, and appreciating the issues that frequently diminish reviewer enthusiasm, should help you to write a better grant. Here are 5 common mistakes that recur among the grants of both first time and experienced PI's.

1. The reviewers did not find your central scientific question interesting.
2. The preliminary data are weak, and call into question the feasibility of the proposal and the validity of your central hypothesis.
3. The proverbial house of cards: the overall success of the grant is dependent on the outcome of a key experiment, which has yet to be performed.
4. The scope of the project is too ambitious, with multiple hypotheses or rationales that pull the grant in disparate directions.
5. The PI and or research team lacks the experience to carry out the proposed work.

1. The reviewers did not find your central scientific question interesting.

Arguably the single most common reason for a grant receiving a low score is the perception by reviewers that your central scientific question lacks significance. Grants that address significant questions provide reviewers with confidence that the results will have commensurately high impact. Reviewer disinterest in your question could stem from a failure to communicate its significance clearly, an overly narrow focus, or a lack of novelty and originality that suggests you are addressing a problem already solved. A common pitfall is that the applicant is so enamored of a particular technology or set of new observations that he or she fails to explain how the work will transform a field, or fails to highlight important links between the work in question and other fields. In today's "Omics"-driven scientific world, one may no longer be chained to the single over-arching hypothesis, but it is still necessary to provide your readers with a clearly understandable strategy for organizing and interpreting that mass of high-throughput data. One way to test the significance of your proposal is to provide a non-expert colleague with a three-sentence description; if he or she can appreciate why you are doing the work, then you are on the right track.

2. The preliminary data are weak, and call into question the feasibility of the proposal and the validity of your central hypothesis.

A second flaw that can doom your proposal is an overly large gap between your hypothesis and the actual data available to be cited or displayed (as preliminary data). A highly provocative hypothesis might be just the thing your field needs but, like a good murder mystery, your jury won't be convinced without detailed evidence. For example, you may have an exciting hypothesis around dinosaur physiology, but if proving your hypothesis requires the results of experiments on fresh dinosaur tissue, you've got a problem. Thus, your reviewers must be convinced of the chain of logic that connects your elegant hypothesis to the actual data presented in the grant, whether published or in preliminary form. Along these lines, a second flaw that kills some applications is a gap between the hypotheses presented, and what the results are actually likely to show. If reviewers perceive that the results will actually be quite a bit more mundane than what the central hypothesis is proposing, their scores will reflect this accordingly.

3. The proverbial house of cards: the overall success of the grant is dependent on the outcome of a key experiment, which has yet to be performed.
When one designs a complex research project, there is a natural tendency is to organize the experiments in a linear and sequential fashion, such that the results of each forms the basis of the next in series. As a template for a research grant, however, this strategy can be risky. If the succeeding aims all depend on a positive outcome of Aim One (whose outcome is as yet unproven), then the fate of the whole grant depends on the success of that first experiment. Likewise, if you are applying for a three-year grant, resist the temptation to anchor the grant to a question that will take 20 years before meaningful tests of the hypothesis can be proposed. In general, reviewers have a much easier time advocating for a grant whose aims are independent, but mutually supporting, with experiments that will provide useful information whether or not your starting hypothesis is true.

4. The scope of the project is too ambitious, with multiple hypotheses or rationales that pull the grant in disparate directions.
Another common flaw of novice grant writers is the “spaghetti syndrome”, where every good hypothesis, experiment, or reagent in the PI’s pantry is thrown at the problem. This approach rests on the assumption that reviewers will find at least a few good ideas stuck on the proverbial wall, and this will raise their enthusiasm. In reality, these types of organizational flaws generally diminish enthusiasm, because they signal a PI unable to prioritize among various facets of the project, which down the road can lead to an inefficient deployment of people and resources. Your research plan should portray a realistic balance between what you hope to accomplish, and the number of junior researchers that you will have available. A tricky scenario that will typically generate a spirited discussion around the table is the grant that has three great aims, but also a fourth and final aim that is less interesting or feasible. A good grant will generally try to strike the correct balance between the conservative/feasible and the risky/adventurous: different reviewers may very well come down at different points along the spectrum.

5. The PI and or research team lacks the experience to carry out the proposed work.
Once reviewers have determined that the work is significant and the approach is valid, they have to answer the question, “Is this the appropriate PI to carry out the work?” For first time and early investigators, the training and accomplishments during the post-doctoral years will provide clues about the likelihood of success. For more senior investigators, past career experience and productivity will be scrutinized carefully. Reviewers will generally accept any approach that you have previously published on, but to move your field forward, you will typically have to display innovation and creativity in adapting and developing new approaches. If a particular approach is unproven with respect to your lab, the most reliable strategies are 1) identifying and soliciting an outside collaborator with a published track record in the method, or 2) devoting existing lab efforts to generate the preliminary data to remove doubts about your ability. In general, this is arguably the most important use of “updates”, short progress reports that can be sent to the SRA after the submission of your grant, but before the panel meets to discuss it.