

Writing a good grant proposal

Simon Peyton Jones and Alan Bundy

Writing a good research grant proposal is not easy. This document is an attempt to collect together a number of suggestions about what makes a good proposal. It is inevitably a personal view on the part of the authors; we would welcome feedback and suggestions from others.

APPROACHING A PROPOSAL

The first and most obvious thing to do is to read the advice offered by your funding agency. In the case of EPSRC, the primary funding body for computing science research, there is a "Guide to EPSRC Research Grants". We make no attempt to duplicate the material in the EPSRC guide or any other; you must get yourself a copy and follow the guidance closely.

The most substantial part of any grant application is some form of "Case for Support". It is this case which will persuade, or fail to persuade, your funding body of the value of your proposal. Proposals range very widely indeed in their quality. You can improve your chances enormously simply by ruthlessly writing and rewriting. This document is entirely about improving your case for support.

There are two vital facts to bear in mind:

- Your case for support will, with luck, be read by one or two experts in your field. But the programme manager, and most members of the panel that judges your proposal against others, won't be expert. You must, must, must write your proposal for their benefit too.
- Remember that programme managers and panel members see tens or hundreds of cases for support, so you have one minute or less to grab your reader's attention.

Based on these facts, here are two Golden Rules:

- *Ask lots of people to help you improve your proposal.* Give it to your colleagues, your friends, your spouse, your dog, and listen to what they say. If they misunderstand what you were trying to say, don't say "you misunderstood me"; instead rewrite it so it can't be misunderstood. If they don't immediately see the value of what you want to achieve, rewrite it until they do. And so on.

This isn't a big demand to make on someone. Ask them to read your proposal for 10 minutes, and say what they think. Remember, most committee members will give it less time than that.

- *Make sure that the first page acts as a stand-alone summary of the entire proposal.* Assume (it's a safe assumption) that many readers will get no further than the first page. So don't fill it up with boilerplate about the technical background. Instead, present your whole case: what you want to do, why it's important, why you will succeed, how much it will cost, and so on.

CRITERIA FOR A GOOD GRANT PROPOSAL

Most funding agencies apply similar criteria to the evaluation of proposals. We discuss these below. It is important to address these criteria directly in your case for support. A proposal which fails to meet them will be rejected regardless of the quality of its source. Otherwise, there is a danger of discriminating unfairly in favour of well-known applicants.

Major criteria

Here are the major criteria against which your proposal will be judged. Read through your case for support repeatedly, and ask whether the answers to the questions below are clear, even to a non-expert.

- *Does the proposal address a well-formulated problem?*
- *Is it a research problem, or is it just a routine application of known techniques?*
- *Is it an important problem, whose solution will have useful effects?*
- *Is special funding necessary to solve the problem, or to solve it quickly enough, or could it be solved using the normal resources of a well-found laboratory?*
- *Do the proposers have a good idea on which to base their work?* The proposal must explain the idea in sufficient detail to convince the reader that the idea has some substance, and should explain why there is reason to believe that it is indeed a good idea. It is absolutely not enough merely to identify a wish-list of desirable goals (a very common fault). There must be significant technical substance to the proposal.
- *Does the proposal explain clearly what work will be done? Does it explain what results are expected and how they will be evaluated? How would it be possible to judge whether the work was successful?*
- *Is there evidence that the proposers know about the work that others have done on the problem?* This evidence may take the form of a short review as well as representative references.
- *Do the proposers have a good track record, both of doing good research and of publishing it?* A representative selection of relevant publications by the proposers should be cited. Absence of a track record is clearly not a disqualifying characteristic, especially in the case of young researchers, but a consistent failure to publish raises question marks.

Secondary criteria

Some secondary criteria may be applied to separate closely-matched proposals. It is often essentially impossible to distinguish in a truly objective manner among such proposals and it is sad that it is necessary to do so. The criteria are ambiguous and conflict with each other, so the committee simply has to use its best judgement in making its recommendations.

- An applicant with little existing funding may deserve to be placed ahead of a well-funded one. On the other hand, existing funding provides evidence of a good track record.
- There is merit in funding a proposal to keep a strong research team together; but it is also important to give priority to new researchers in the field.
- An attempt is made to maintain a reasonable balance between different research areas, where this is possible.
- Evidence of industrial interest in a proposal, and of its potential for future exploitation will usually count in its favour. The closer the research is to producing a product the more industrial involvement is required and this should usually include some industrial contribution to the project. The case for support should include some 'route to market' plan, ie you should have thought about how the research will eventually become a product --- identifying an industrial partner is usually part of such a plan.
- A proposal will benefit if it is seen to address recommendations of Technology Foresight. It is worth looking at the relevant Foresight Panel reports and including quotes in your case for

support that relate to your proposal.

Cost-effectiveness

Finally, the programme manager tries to ensure that his or her budget is to be used in a cost-effective manner. Each proposal which has some chance of being funded is examined, and the programme manager may lop costs off an apparently over-expensive project. Such cost reduction is likely to happen if the major costs of staff and equipment are not given clear, individual justification.

COMMON SHORTCOMINGS

Here are some of the ways in which proposals often fail to meet these criteria.

- *It is not clear what question is being addressed by the proposal.* In particular, it is not clear what the outcome of the research might be, or what would constitute success or failure. It is vital to discuss what contribution to human knowledge would be made by the research.
- *The question being addressed is woolly or ill-formed.* The committee are looking for evidence of clear thinking both in the formulation of the problem and in the planned attack on it.
- *It is not clear why the question is worth addressing.* The proposal must be well motivated.
- *The proposal is just a routine application of known techniques.* Research funding agencies are interested in funding research rather than development. Industry are expected to fund development work. The LINK scheme is appropriate for proposals which combine both research and development. If the development would benefit another research field, rather than industry, then look to the funding agencies of that field.
- *Industry ought to be doing it instead.* If the work is 'near market' then it should be done by industry or industry or venture capital should be funding you to do it. If no industry is interested then the prima facie assumption is that the product has no commercial value.
- *There is no evidence that the proposers will succeed where others have failed.* It is easy enough to write a proposal with an exciting-sounding wish-list of hoped-for achievements, but you must substantiate your goals with solid evidence of why you have a good chance of achieving them.

This evidence generally takes two main forms:

- "We have an idea". In this case, you should sketch the idea, and describe preliminary work you have done which shows that it is indeed a good idea. You are unlikely to get funding without such evidence. It is not good saying "give us the money and we will start thinking about this problem".
- "We have a good track record". Include a selective list of publications, and perhaps include a short paper (preferably a published one) which gives more background, as an appendix. If you make it clear that it is an appendix, you won't usually fall foul of any length limits.
- *A new idea is claimed but insufficient technical details of the idea are given for the committee to be able to judge whether it looks promising.* Since the committee cannot be expert in all areas there is a danger of overwhelming them with technical details, but it is better to err by overwhelming them than by underwhelming them. They will usually get an expert referee to evaluate your idea.
- *The proposers seem unaware of related research.* Related work must be mentioned, if only to be

dismissed. Otherwise, the committee will think that the proposers are ignorant and, therefore, not the best group to fund. The case for support should have a list of references like any paper, and you should look at it to check it has a balanced feel - your referee will do so. Do not make the mistake of giving references only to your own work!

- *The proposed research has already been done - or appears to have been done.* Rival solutions must be discussed and their inadequacies revealed.
- *The proposal is badly presented, or incomprehensible to all but an expert in the field.* Remember that your proposal will be read by non-experts as well as (hopefully) experts. A good proposal is simultaneously comprehensible to non-experts, while also convincing experts that you know your subject. Keep highly-technical material in well-signposted section(s); avoid it in the introduction.
- *The proposers seem to be attempting too much for the funding requested and time-scale envisaged.* Such lack of realism may reflect a poor understanding of the problem or poor research methodology.
- *The proposal is too expensive for the probable gain.* If it is easy to see how to cut the request for people/equipment/travel, etc. to something more reasonable then it might be awarded in reduced form. More likely, it will be rejected.
- *The proposers institution should be funding it.* Research agencies will usually only fund research that requires resources beyond that which might be expected in a "well-found laboratory" --- indeed, this is part of the charter of the research councils. If it looks like your proposal might be done by a PhD student on the departmental computer then that is what should happen. If the proposer's laboratory is not "well-found" then this is taken to be a vote of no-confidence in the proposer by his/her institution.

Doubtless there are other common grounds for failure that have been omitted. If you know of any please let us know!.

Often, one can tell from independent knowledge of the proposers or by reading between the lines of the proposal, that the criteria could have been met if a little bit more thought had gone into the proposal. There is a clear question being addressed by the research, but the proposers failed to clarify what it was. The proposers are aware of related research, but they failed to discuss it in the proposal. The proposers do have some clear technical ideas, but they thought it inappropriate to go into such detail in the proposal. Unfortunately, there is a limit to which a funding agencies can give such cases the benefit of the doubt. It is not fair for referees to overlook shortcomings in proposals of which they have personal knowledge if similar shortcomings are not overlooked in proposals which they have not encountered before. In any case, proposals which *do* meet the criteria deserve precedence.

CONCLUSION

We hope that this document will help you to write better grant proposals, and hence to be more successful in obtaining funds for your research. This article is not just about writing better grant proposals to obtain more money. The basic set-up of peer-reviewed grants of limited duration is a sensible one. It compels researchers regularly to review and re-justify the direction of their work. Behind poorly presented grant proposals often lie poorly-reasoned research plans. Perhaps if we can improve the quality of Computer Science proposals we will also improve the quality of Computer Science research.