Thoughts on Marsden Grants

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- The process is that there is a panel who individually give rankings to each proposal.
- These rankings are in two parts.
- The most overwhelmingly important one is the first: what is the scientific merit of this proposal.
- Track record is important, especially as it relates to the project, but the one above is the most important.
- “Business as usual” projects are frownes upon.
- Contribution to NZ skills is less so nowadays.

- Conclusion: You must write the proposal to maximize the score on the scientific merit part.
- Clearly track record and potential is implicitly taken into account in the score. (Years of experience is factored in also.)
- The panel will be a number of eminent scientists in your general but not your specific area.
- Questions I ask: What are the new ideas here? What is the novelty? How can I believe that this person can carry it thru? (ie not just a long list of questions with no clear plan of how to attack).

- Think about the people who are likely to be on the panel: what sorts of people are they?
- Bear in mind there will be probably at most one person on the proposal who will be active in your specific area.
- So DON'T launch into the proposal as if you were e-mailing your favorite co-author, with only the occasional "the" and "I'm good" to separate the jargon.
- But, not only can the panel read English , but they are experienced in reading proposals, and will have training in your general area.

At the panel meeting

- Scores are merged.
- Scores have been done done using percentage bands to spread the grades.
- Individual proposals are each considered and each is discussed to see if the panel wants to revise the scores.
- Panelists with a conflict of interest (same department, etc) have not scored those proposals and leave the room for the re-scoring. Panelists with a major conflict (spouse etc) of interest are not involved at all.

My Suggestions

- First paragraph: Say in general terms what you plan to to, and why it is interesting. This I think should be accessible to any person in your general area.
- Middle bit: say more specifically what your project will do in more detail aiming at more of an expert. Say what are the new ideas you will bring to bear.
- Now give more details that more of an expert in your area would be convinced by that you have a good plan and the ability to carry this out. How are you going to do this. Again stress ideas.
- Now finish: Reprise the first paragraph. what are you doing, why, why is it important, what are your new ideas.

Referees

- In the second round: We don't simply look at the referee's scores. The proposals are carefully read, and the referee's comments are seen as more important than the "scores".
- The should comment on the proposal not you.
- Choose thoughtful referees who will actually comment on your proposal in a meaningful way. Not just :
- "This is the best proposal I have read and the proposer is brilliant, maybe even a genius. He is also a good ballroom dancer" with nothing else would probably count against you (unless its true!).
- Don't suggest your or AI's co-authors as referees, nor people at your university, and likely in NZ.
- Choosing famous prople can have a big payoff, but that can be one of two ways!
- Personally I like a good geographical spread, but this is probably discipline specific.

Pet Hates

The following are things I personally find will downgrade a proposal to me.

- Overly grandiose claims. “This will revolutionize computational gerionics,”
- Sounding too much like a used car salesman.
- Especially concentrating on outcomes (like listening to a politician... “we’ll deliver optimism.”)
- Saying what will happen from research. If it is known then why are we supporting it? Say what you expect will happen.
- Telling me over and over again that it is good for New Zealand.
- Proposals with just one (or fewer) little idea.
- Proposals that are just a simple list of problems, with no indication as to why we care or how they might be attacked.

Pet Likes

- Proposals that have really new ideas, and are not just direct extensions of previous work.
- Proposals that could have a big pay-off.
- proposals where there seems a coherent programme rather than a list of problems.
- Proposals that link areas together, with impact beyond their own speciality.

Common False Beliefs

- If you fail to get a grant for 2-3 years you will never get one. Panels change each year. Your history changes.
- It is always the same people. There is no guarantee of continuity.
- Being young counts against. Rather the opposite in my experience.

Common False Beliefs, ctd

- Applying and not getting a grant serves no purpose.
- First there are many other granting agencies (e.g. local strategic) which you might well be applying to, and you will need to think about that anyway. It cannot hurt to think about what you are doing.
- Second in my experience, getting to, say, the second round allows for a lot of local leverage for local (e.g. science faculty) grants.

- Third as your ideas evolve you can upgrade the proposal for the next year. Many poeple are more succesful in the second, or third tries because their ideas have had more time to simmer.
- Fourth at the very worst, the numbers are determined by how many reasonable preliminary applications are in the area.

Final Thoughts

- Get as many people as possible to read the proposal.
- Make sure that someone who is not a mathematician reads it. Especially someone who actually knows how to write good English .... I ask my wife.
- Take care who you put on as co-PI's. This can count against you. Both (or more) of you will be rated.
- After you write it, put it aside for a week, then read it again.
- Start NOW!