Successful grant writing takes careful thought as well as considerable skill. Experienced investigators appreciate just how much work and background development are required. However, those new to the ‘game’ are sometimes under the misconception that if they have a good research idea or it is clinically important, they are bound to succeed. ‘Do good science and the rest will follow!’ Unfortunately, this just is not true.

Successful grant writers appreciate three important points:

1. Don’t take anything for granted!
   - Even if you have a superb track record and great ideas that could fundamentally change a field, this is not an excuse for cutting corners and dashing off a poorly thought-out submission. Grants require care. Sloppiness is obvious.

2. Put your energy into a few, well-crafted submissions
   - It does nothing for your morale, or reputation, to keep resubmitting poor applications that fail. Reviewers and panel members have long memories.

3. The competition is tough
   - Success rates are low for most schemes, often less than 20%, sometimes even down to 10%. And your competition is stiff. It includes some of the finest people of your generation, your most able contemporaries. So, to stand a chance of success you have to give grant writing—and your competition—the respect it deserves.

When you submit an application, it is likely first to be screened by administrative staff to ensure that it fulfils the basic requirements. Some grants fall at this very initial hurdle. In some schemes, there might then be a triage or selection procedure, to filter out the applications that fail. Reviewers and panel members have long memories.

KEY REASONS FOR SUCCESS AND FAILURE

In my experience—as an applicant, reviewer and grant panel member—the key reasons for success include the points in box 1.

I will discuss some of these in more detail later but first it is also worth considering some major reasons for failure. For me, these include the following:
Box 1 Ten top reasons for success

The application
1. Fits the Call or Fellowship scheme well
2. Is carefully crafted and polished over time, improved
   by colleague feedback
3. Is timely, pertinent and asks good—even crucial—
   questions for the field
4. Is hypothesis driven and intellectually stimulating
5. Is clear, readable and intelligible
6. Shows that you are passionate about this topic
7. Provides pilot data and follows credibly from estab-
   lished findings
8. Justifies sample sizes with power calculations
9. Is to performed in a centre of excellence
10. Builds on your track record

1. The submission is unclear, written in haste or just poorly
    put together
   Remember that it has to be easy to read and compre-
   hend by reviewers who are extremely busy. You might
   think that the text you have laboured over for hours
   will be evaluated with great care. On many occasions,
   however, your applications will be speed read, perhaps
   in less than half an hour, in a setting where the
   reviewer is vulnerable to being distracted
   in less than half an hour, in a setting where the
   reviewer is vulnerable to being distracted
   —on a train or a flight, at home with children running around or in
   a busy office where there is always someone knocking on
   the door. If the text is not crystal clear, what you think
   —or assume—is obvious will be missed. This is one
   reason why it is essential to get your submission read
   by colleagues long before you submit. Unfortunately,
   most people leave this to the last minute when it is
   usually too late.

2. No hypotheses or poorly articulated ones
   This is a common reason for being shot down at a grant
   panel when your application is discussed. It is surprising
   how many submissions just do not have explicitly stated
   hypotheses. Goals and aims are not the same as saying
   what you are testing. When you do state your hypoth-
   eses, they have to be signposted well.

3. Inadequate track record or expertise in this area
   Collaborations with experts can help to get around this
   criticism, so think about approaching collaborators
   within or outside your institution early and get then to
   write a letter of support. In addition, try to get as much
   leverage as you can from what you have already
   published.

4. Far too ambitious
   A common criticism is that the applicant could not
   achieve all that is proposed in the time available for the
   grant. There is a difficult balance to achieve between
   promising enough and far too much. The key point is
   that all that is proposed must be feasible within the time
   scale. Your reviewers have lots of experience and they
   know what is practical.

5. Great ideas but no preliminary data
   You have to convince reviewers that your proposal will
   work. If this is a completely new approach, no matter
   how exciting it might be, the grant panel will require
   some evidence for its credibility. When in doubt get as
   much pilot data as possible before submitting.

6. Incremental research, not a step change
   This is a difficult issue. If the proposal seems too conser-
   vative and incremental, it might be rejected because it is
   not exciting enough and will not deliver sufficiently
   novel findings. On the other hand, if it is too risky and
   ‘blue-skies’ it might be considered a gamble. The best
   proposals incorporate a combination of both elements:
   build incrementally on previous work—either by you or
   by the group you hope to join—but also think about
   proposing a riskier, step change element. Remember
   that blue-skies ideas are good for only some
   parts of a submission but not for all of it.

BEFORE YOU START
It is important that even before you start putting pen
to paper you are clear why you are writing this appli-
cation. Motivation really does matter. Ideally, you will
be applying because you have some great ideas that
you have thought about carefully and really want to
test. Furthermore, you have the energy and enthusi-
asm to pursue this programme of work. In short, you
should be motivated to make this work. You really
should not be writing an application just because your
boss thinks it might be a good idea or because there is
a new Call which is vaguely in your area or simply
because you feel that it is worth a ‘punt’. Grant
writing takes a lot of time and energy and by the end
of an application most people are drained. So think
carefully about your motivations before you even
start. If you are not convinced or driven by this, it is
unlikely to succeed.

DOES IT FIT THE SCHEME?
Be careful about reading the specific requirements of
the Call or Fellowship scheme. Contact the funding
agency, if necessary. Don’t be frightened to discuss the
proposal you have in mind with one of the adminis-
trators there. They often have been in science them-
selves and are very helpful if approached
appropriately. Find out from them and your collea-
gues about what sort of proposals have been suc-
cessful before. Then check whether you would have
institutional support for applying to this scheme.
Don’t surprise your boss by asking him or her to
endorse your application at the last minute. Heads of
departments do not like surprises! It is well worth
finding out what the institution would be willing to
commit to in terms of supporting you for this applica-
tion and the implications that this might have for the
future. Finally, re-read the instructions and be clear in
your own mind that your proposal fits exactly what
the scheme is designed for.
WHAT MAKES A GOOD RESEARCH QUESTION?
If you put this to leading researchers you are likely to get many different answers. So ask them! You will learn a lot from the process and from interacting with them. I think a good research question has to be obviously good to people from outside your research area. It has to be big enough for others to appreciate immediately why anyone should spend years of their life using large amounts of money from the public purse or charities to answer such a question. For many researchers, the question also has to be intellectually stimulating—if possible, thrilling! But at the same time a good research question has to be one which is likely to be answered within the time frame of the grant. It has to be a practical proposal, not something so grand that it would take decades to unlock.

DON’T RUSH IT
Putting together a coherent and cohesive set of studies to answer your question is not easy. Ideally, before you start to write you should have assembled a plan of possible studies from which you need to cherry pick your best ideas for this particular application. You need time to get comments and feedback from colleagues to see whether they are convinced. This really does matter and the more experienced members of your department or your collaborators will be able to give you helpful advice (figure 1), provided you don’t leave it until the last minute. It is vitally important to polish up an application properly so that everything is crystal clear and cohesive. Remember also that costing take time and may alter your proposal if it turns out that the sums you require would be well above what the funding body is prepared to offer. Full Economic Costing in the UK also adds to the final total and it is important to be aware for which grant giving bodies this might be a factor.

MAKE IT HYPOTHESIS DRIVEN
It is well worth jotting down what your hypotheses are, for your own sake. Most studies are vulnerable to the criticism that they are ‘exploratory’ and do not have specific hypotheses that are being tested. Even if your study is largely exploratory, make some explicit hypotheses about what you might be directly testing, based on proposal. It is absolutely essential that you write down clear hypotheses, unless the Call is specifically for an exploratory study, which is very rare. ‘Fishing expeditions’, no matter how good they might be, do not fare well with reviewers or grant panels. I often use direct questions incorporating the hypotheses I want to test as subheadings in an application. That way no one is in doubt about what the hypotheses are.

WRITING THE DOCUMENT: PUT YOURSELF IN THE REVIEWER’S SHOES
While you are writing imagine how you would feel reading this material. The narrative has to be absolutely clear and coherent, with a linear trajectory. No matter how scientific or clinically applied your submission, remember that you are telling a story that the reviewer has to get immediately. Moreover, as explained earlier, you have to appreciate that many reviewers and even panel members might not actually be in your field of expertise. For many schemes, particularly Fellowships, panel members will not be even in your general area, so they need to understand why your case is so compelling and important. This is why the overview and strategic vision must be both clear, as well as exciting. Unfortunately most are relatively dull! One way to improve this is to write the summary for lay people at an early stage. Unfortunately these usually do not receive enough attention and are scrambled together at the last minute. However, you would be surprised how often reviewers and panel members read these summaries first to get an idea of what you are trying to do. Polish these well! It is well worth the trouble.

The proposal also has to be intellectually exciting, even for the non-specialist.

Make the document easy to read in terms of its formatting. Avoid clutter, use figures and boxes wherever you can. Try not to cram the entire document with dense text. Signpost a new section clearly and consider stating the hypotheses for that section up front with bullet points so they stand out and will not be missed. Use lots of paragraphs to break up the text and to make it readable. Do not go over the word limit. Panel members become particularly irritated if this is evident. Above all, you don’t want to hand them excuses to reject your application: because they do not understand it well enough, it just does not seem very exciting or you did not stick to the rules of the application.

SAMPLE SIZES
All studies need to consider a justification for the sample size. This is an issue that can be relatively easy to address explicitly but you would be surprised how
many applications do not include a power calculation. For some, applications you need to provide evidence you did this with a statistician. For others, you can do
it yourself using free software (eg, G*Power at http://
www.gpower.hhu.de). Sometimes it is not easy to
provide a power calculation for certain types of
research. If this is the case, you need to explain why
and ideally refer to a previous study which success-
fully used a sample size like the one you propose in
order to answer a related question.

**POTENTIAL WEAKNESSES OF THE PROPOSAL**
All proposals are vulnerable to criticism. Some
reviewer might consider the study design to be inade-
quate to answer the question. Others might not be
convinced that alternative explanations for predicted
results have been considered. You can try to strengthen
your proposal by considering these criticisms and
building measures to counter them, for example,
adding further controls or by considering more care-
fully the design and analyses. Think also about contin-
gency plans and whether you should explicitly discuss
these. If your entire programme of research depends
upon an initial study being successful, you are vulner-
able to the criticism that there is no plan should it fail.
Again, this is where early reading by experienced col-
leagues might help you to head off the killer criticisms
that a reviewer might raise. Don’t be shy to ask for
their help. They might, for example, advise you how to
avoid a linear, sequential strategy by creating a more
parallel

**USE FIGURES AND ILLUSTRATIONS**
Many submissions consist of endless pages of text.
Figures help to break up the appearance and make the
application more appealing to read. They also can be
used to improve the clarity of your proposal, to illus-
trate concepts or add flesh to the text with a concrete
equivalent example. Take care to write the figure legends well.
These allow you to reiterate points made in the text;
saying it differently increases the likelihood of a diffi-
cult point being understood. Figures are also essential
if you want to show off pilot data. A plot of the find-
ings has far greater impact than a line in the text,
which is easy to miss.

**IMPACT**
This is an important trend for many grant funders.
Ask yourself what your big idea is. Are you going to
fill a knowledge gap? Would anyone care? Is the pro-
posal timely? And how would it make a difference if
you were successful? The societal, economic, health,
intellectual property and other impacts might be
important in different ways for different grant
funding bodies. Understandably, many researchers feel
the pressure to make a case for their work being clin-
cially important. However, it is important also to
appreciate that there is no point in simply gesturing
towards ‘translational’ research. If your application is
not applied directly to a clinical problem, don’t
pretend. It annoys reviewers. Vision can nevertheless
be displayed, in part, by spelling out the next steps,
for example, ‘If we find this biomarker/gene/imaging
signature, then we will proceed to developing clinical
care, given time and resources

**Box 3 Reasons for rejection by grant panel**

1. No hypothesis
2. Far too incremental; not a ‘step change’
3. Dull, not sufficiently exciting
4. Great ideas but no pilot data
5. Great ideas but far too ambitious—impractical,
given time and resources
6. Sample sizes not justified with power calculations
7. Proposal has too many potential weaknesses; insuffi-
cient controls
8. Does not have strong enough track record
9. Plans to work in a centre which has insufficient
experience in this field
10. Unclear why this is interesting or what the impact of
work would be

**Box 2 Characteristics of poor impact proposals**

- **Lack of specificity on deliverables**
  What exactly will you have achieved at the end of the
  grant? How might it affect people outside your field?
  What would be the wider impact?

- **Lack of consideration of broader beneficiaries and
  stakeholders**
  If your work has potential to have wider impact, don’t be
  shy to mention these. It can sway the decision making of
  the grant panel.

- **Proposal is too narrowly focussed**
  Is the potential for impact too narrow? If so, how might
  you widen it?

- **Too much focus on track record rather than what will
  be done**
  Make sure you devote sufficient text to the details of the
  project and articulate the wider significance of this work.
early on. It is also important to think about how you might facilitate ‘knowledge exchange’ which ideally would be a two-way process, for example, between patient groups and your research group.

**DON’T GIVE THE PANEL EXCUSES TO REJECT**

Finally, try not to hand the panel or reviewers any reasons for rejecting you. I have covered some reasons for panel irritation above. Some recurring themes in committee discussions include the points in box 3, which might serve as a useful list to consider—and reconsider—as you polish your application.

It should be obvious by now that writing a grant is a formidable endeavour. The low success rates mean that it is simply not worth putting together an application that has not been well thought-out or is assembled at the last minute. If you do have the motivation, I hope the advice distilled into this short article will offer a little assistance in getting you across the finishing line, with success.

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**Key points**

- Successful grant writing takes time and care, so plan well ahead of the deadline.
- Proposals need to fit the call.
- They should be clear and exciting, but also feasible to complete within the time frame.
- Applications can be improved with advice from more experienced colleagues, so do ask for help.
- Ideally they should be hypothesis-driven, have supporting pilot data and justification of sample sizes.
- Their potential for impact on a scientific question, patient group or for Society needs to be clearly stated.

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