Chapter Two

The Research Idea

The true sign of intelligence is not knowledge but imagination

— Albert Einstein

Εὕρηκα! [*I have found it!*]

— Archimedes

So here you are with fingers poised over the keyboard of your computer. It's time to write that proposal to fund your new project. But where to start?

Any good proposal starts with an idea. If the author of a proposal does not have a good idea, no matter how polished the finished product, the proposal is not likely to have great success either with reviewers or with the agency providing the funding. This is the area where the author's experience, knowledge of the subject matter, creativity and imagination most come into play.

2.1 What Makes a Good Idea for a Research Proposal?

Words that are often used in a call for proposals (CFP) from a funding agency, to describe the concepts they are looking to fund, are *original*, *significant* or even *beyond state-of-the-art*. Funders are looking for fresh ideas and not merely the 'same old' approaches that have been tried before.

What I look for in a good proposal is excitement. A proposal that is simply incremental turns me off.

— Quote from an experienced reviewer

Funding agencies want the projects they fund to make a difference to the development of the field, possibly leading to new directions or to solving a problem. Such problems exist in all fields, and finding ways to solve them is at the core of a good research proposal.

Reviewers are often uneasy about an applicant who simply wishes to continue his or her PhD project in a new environment. Distancing oneself from a thesis advisor and being seen as an independent researcher with new ideas can be very important.

An example of moving in a new direction and not settling for incremental advances.

One of the scientists we interviewed described a situation where his group had shown how an estrogen receptor switched a particular target gene to 'on'. This was a major breakthrough and led to a number of papers in high impact journals. However, once the first two or three papers were published, the group decided that they would move on to another area and not to continue simply finding more examples of the same phenomenon. They believed that this would just be incremental work. Instead they moved into a new area of DNA methylation, and continue to make contributions in this area.

Firstly, you will need to read the CFP carefully to make sure you are addressing the requirements of the funders. There is no point submitting a proposal with an exciting idea if the funding agency is looking for the solution to a particular practical problem. For instance, funding agencies (particularly those in countries with emerging economies), often require that the proposals they fund make a positive difference to the economy of their country. Therefore, it is critical in your proposal to address the concerns of the agency, and to look for ways in which your project is likely to have an impact beyond gains in knowledge. We will discuss further the importance of the impact of research in Chapter Seven.

I have a number of problems with this proposal. First, where is the novelty? IR spectroscopy is a well-known analysis tool with many practical applications. Perhaps the best known is the measurement of blood alcohol content. Commercial instruments are readily available. What will be new in the current project?

— Comment from a reviewer

2.2 Where Do Ideas for a Research Project Come From?

Students finishing a thesis and moving to a research post are often tempted to repeat their thesis project under a slightly different guise. This may be a tempting and even reasonable strategy, where there remain interesting aspects of the subject to be explored. However, in most cases it is wise to strike out in a new direction when looking for a project. The best projects are those that capture your interest. Chances are that other people will also find this to be true, and your enthusiasm can be an added benefit.

However, no matter how captivated you are with a topic, it is always a good idea to ask yourself some important questions, such as:

- What difference will the results of the project make to the overall understanding of the field or of a neighboring field?
- How significant will the result be, even if the project is successful?
- Who will be interested in the results? People in my field? People in neighboring fields?
- Can the project be carried out with the equipment and techniques available now?
- What positive impact will the project have on society?

People will be interested in your results if they increase understanding in the field or if the project has the potential to help solve a problem locally or globally.

Sometimes an idea may be extremely important, but the technology is not yet available to be able to carry it out. **Timing can be critical**. Choosing a project that can be carried out successfully now, but which would have been impossible previously, is usually a good strategy. In some cases, the development of the technology itself may be a good research topic. Often such technical developments can spin off into numerous projects. Keeping abreast of current technology is also important in allowing one to capitalize on new developments.

Another reason for being familiar with the technology in your field is that it may turn out to apply to a problem in another field. There are many examples where a technique developed in one science has made an enormous impact in another. One example is the use of mass spectrometry to measure $^{13}C/^{12}C$ ratios, which has allowed geologists to study ancient climate patterns.

Archimedes gets an idea for a grant proposal.

So, as your career advances, it is important to continue to keep abreast of the latest advances in the field. The most common mechanism is to **read the relevant journals regularly**. With most journals available online, it is easier than ever to keep in touch with current work. In most fields of research, there are half a dozen or more journals that contain reports of the most important research. It is important to make time to read these critical journals to make sure that you know what is currently happening in your field of interest. There are also more general 'popular' magazines containing summaries of interesting areas of current research. In physics, for example, the popular journal *Physics Today,* produced monthly by the American Physical Society (APS), summarizes interesting new results in different areas of physics. Professional groups in other countries produce similar overviews for a more general audience.

As you read, collect and store ideas for possible proposals.

One researcher we interviewed, a chemist, told us that as he reads the literature, he collects ideas on index cards. Even when he took his first academic position, he had already collected a small box of cards with ideas. He still does this today, even though he is now an experienced researcher with a strong record of funding. Of course, there are modern electronic methods of keeping track of good ideas, but the concept is the same.

Another advantage of reading the literature regularly and carefully is that it can prevent your spending needless time on projects already published. Most scientists have experienced being 'scooped' on a project where a result is published just as they were preparing their own for publication. This is unfortunate but in most cases unavoidable. What is avoidable is spending time on a project already reported in the open literature. With modern electronic search tools, this should now be very rare indeed.

An example of the importance of reading the literature from one of our interviews — much time would have been saved by checking the literature.

When Dr. B was a postdoc at a large National Laboratory, the group with which he was working was developing an array of Barium Fluoride detectors. They discovered that if they cooled the detectors to zero degrees C, the response of the detectors was much better. They wrote up this observation for submitting to a journal. However, as they looked at the literature, they discovered that this observation had already been published. If they had been more conscientious about reading the literature, they might have saved themselves a good deal of time.

Ideas for research projects often come at unexpected times even when you are relaxing, maybe in an *onsen* (hot spring) or on a long airplane flight. One senior Japanese scientist has said that many of his best ideas come when he is daydreaming during a boring committee meeting. On the other hand, it is useful to deliberately create situations to encourage stimulating ideas.

Often a group of researchers will form a **journal club** where each member brings a different journal article to the meetings, and explains it to the rest of the members. The subsequent discussion may not only lead to a better understanding of the current topic, but also generate interesting new research possibilities to explore. A journal club with student members has the added advantage of providing the students with the opportunity to present ideas to an informed audience. This can help improve their communication skills.

The notion of a journal club also emphasizes the importance of **communicating with colleagues**. With very few exceptions, most people find that their thinking is stimulated by discussions with other people. This can happen informally, for instance, by dropping into a neighboring office. In some departments or institutes, there are also formal arrangements to encourage interaction. For example, we have experienced some academic departments host afternoon coffees for the whole department with all of the faculty and students strongly encouraged to attend. The blackboards around the room can be filled with symbols as small groups discuss various topics. This is a tremendously stimulating environment, and we believe it serves as an excellent model for a vibrant research group in any field.

Many departments and research institutes also hold regular **seminars and colloquia**, where visitors from outside the organization are invited to present their latest research. Both the presentation and the subsequent question and answer session are often stimulating sources of new ideas. Unfortunately, budget problems or simply geography can make it difficult to invite outsiders. If institutions are close enough, in a large city for example, it is sometimes possible to share the cost of a speaker and the staff of the different institutions can all attend the presentation.

In addition, it is important to **attend conferences** related to your research field. National and international conferences are held regularly in many countries and you should take every opportunity to attend at least one such conference a year if possible. Not only will you have the opportunity to hear experts address current topics, but you will have the opportunity to meet and mingle with other participants. This can be both stimulating and informative. In the United States, for instance, there are numerous Gordon Research Conferences held every year in the various fields of chemistry. These conferences are designed to be very informal.

The number of participants at any given conference is limited, no minutes are kept nor are the proceedings published. The afternoons are kept free to permit informal discussion among participants. Similar informal conferences, are held in many countries of the European Union, and could serve as a model for countries that are trying to improve their research performance. In some cases, funding agencies have special travel awards to permit attendance at such conferences (see Chapter Eleven).

Interacting with colleagues in neighboring fields can also be very stimulating. In some cases, there may be new techniques that may be applicable to your field. Or concepts which are well developed in another field may be transferable to yours (and vice versa). Group theory was well studied by mathematicians before being applied to particle physics, while the use of statistics and large databases is in the process of revolutionizing biology.

Enlightened chairs and department heads will often make funds available to allow their staff to attend conferences to keep them up to date with current research. This can be especially valuable for young researchers who do not yet have their own research funding, and for staff who may be suffering a temporary hiatus in funding. The sensible leader must simply be cautious that this opportunity is not abused and some outcome should always be expected.

Finally, in exploring opportunities for new interesting ideas, the **stimulation provided by students** can be very important. Even students in elementary classes sometimes raise questions that can force one to examine a topic in a different way, and this can lead to an interesting research project. In graduate classes or seminars this possibility becomes even greater.

DO read the current literature in your field and keep track of research ideas that arise.

DO consider forming a journal club with colleagues and students.

DO discuss your ideas with colleagues.

DO attend seminars and colloquia in your own and neighboring institutions if possible.

DO attend national and international meetings as funding and time allow.

2.3 What Makes a Good Research Topic?

Selecting an appropriate research topic is one of the most challenging and important issues in writing a research proposal. But this is also one of the most difficult areas in which to give advice. Different fields of research have different cultures and approaches, and you need to be familiar with the particular research culture in your own domain. Certainly all your professional preparation is important, as is using all the tools discussed in Section 2.1 to keep you up-to-date with important issues.

There are numerous factors to consider in deciding on a research idea for your proposal. It should be **original**, **interesting**, **exciting** and **not incremental**. Ideally, the reviewer on reading your idea should ask himself or herself, 'why didn't I think of that topic?' The subject must be consistent with the goals of the funding agency, as spelled out in the CFP. And, if the CFP requires it, links to possible applications and with industry need to be established.

Some general approaches that apply to many proposals are:

- Providing new knowledge that moves the field forward.
- Providing a method of solving an outstanding problem in the field.
- Developing an improvement in the current technology and indicating the resulting new opportunities.

Providing new knowledge

A survey of the literature, discussed in Section 2.3, will place your topic in context and show how it might advance your field. There are always important questions in any field that beg for answers. However, you need to be familiar with the current status of your field to be able to ask these questions, and to decide on a strategy to answer them.

If the project is not going to move the field forward, reviewers will not likely recommend funding. If it does not have a broader significance, it simply becomes a hobby.

In some cases there may be a controversial issue requiring clarification. If you can think of a research question that you can ask with an answer that might resolve this issue, you have a good basis for a proposal. Even better is the case where the question you pose can challenge some fundamental concept in the field, but this is a more onerous task. The burden of proof will be much more difficult, and you must be certain that your approach has not been tried before and that you are not wasting your time.

The goals should be ambitious, but not too ambitious

There is a natural tendency, especially for researchers without much experience in writing proposals, to want to solve the major problems in the field. They will choose broad topics that outweigh the resources, both in personnel and funding, that are available. Normally the CFP for any particular competition for funding will give an indication of the magnitude of the budget available, and this will give the proposer a good idea of the possible scope of a project for this competition. There is a major difference in expectation between a large 'center-type' grant involving many people, and a single investigator award.

This is not to say that the author of the proposal should not be ambitious in his or her proposal. Reviewers normally like proposals that reach into new territory and attempt difficult and challenging projects. If, however, a project is too ambitious and tries to do too much with available resources and time, reviewers will usually judge that the author is inexperienced or has poor judgment and they will mark the proposal down. A limited project that has an excellent chance of success can provide the basis for future proposals building on the success of the current work.

A common input from a number of reviewers was that many of the proposals they reviewed, especially from young investigators, were far too ambitious. The investigators tried to do too much with the resources of time and money that were available.

It is also important to ensure that the research question you pose can be answered with the equipment available or that you can develop the technology to answer the question. This development may be a major part of the proposal, and you will need to face the question of what to do if the technical development stalls or is not possible. Not every project will succeed, but you need to convince the reviewer that your approach has at least a reasonable chance of success.

An example of the danger of choosing an overly ambitious research idea, *viz.* nanoscience.

One of the current exciting areas in physical sciences is nanoscience i.e., science at the scale of nanometers (10^{-9} m). This is approximately the molecular scale and has relevance to a wide variety of topics, from delivering medicine to specific organs, to manufacturing. As a result, it has become fashionable to wish to participate in this rapidly advancing area. However, many large laboratories in many countries are engaged in research in this area, so that attempting to start a research program in nanoscience, unless there are very large resources available, is very likely to be doomed to failure. If resources are limited, it is usually more advantageous to choose an area that is less competitive and in which fewer people are working.

What does it mean for the research idea to be 'original'?

All true research must be original and this is a common requirement in many, if not all, CFPs. Certainly you need to be sure that you are indeed doing research and not simply repeating work done previously. Nothing will kill a proposal more quickly than the reviewer realizing that the work you are proposing has already been done, or is simply derivative in nature. As mentioned earlier, choosing a topic that is both **original** and **important** and **NOT incremental** is definitely an advantage.

The importance of originality in formulating your research question — coming up with an original approach can make a big difference to the reception of your proposal by reviewers.

Dr. F, who we interviewed, described a proposal that he reviewed some time ago. The proposal was from a young person at the beginning of her work (she has subsequently had a great career). She proposed to study the effect of alcohol use by means of drosophila (fruit flies). She proposed to select drosophila that were either more or less susceptible to alcohol. To do so, the principal investigator (PI) took a heterogeneous group of flies and placed them in a flask and tubing that she had designed containing alcohol. When the fruit flies became intoxicated they fell to the bottom of the tube and were readily collected. By this means, she was able to screen out mutants that were more or less resistant to alcohol. Some of these same genes turned out to be present in humans. It was a very clever but simple idea and worked really well. The reviewer gave it a good score, and he turned out to be right. It helped the PI get her career get off to a great start.

There are, however, cases where the unique situation in a particular country might justify using well-known techniques that have worked in other places, now applying them to a different environment. The availability of family data over many, many generations in Iceland or the Republic of Kazakhstan for example, permits interesting genetic studies, even though the techniques were developed and used elsewhere. Similarly, the nuclear disasters at Chernobyl and Fukushima have given rise to interesting environmental studies using well-known principles, now applied to very different and unique situations.

An example emphasizing the 'local' originality of the project.

Dr. S, who we interviewed, had read a proposal from researchers in a developing country with a higher than normal incidence of women's deaths from breast cancer, and with a low rate of women seeking screening for breast cancer. They recognized the link between these two facts and sought to address them. They knew of a project in another country that had established and successfully addressed the complex cultural factors that prevented a segment of the female population from seeking breast cancer screening even when it was readily available. The researchers in the developing country saw parallels between the situation of the women of their country and the situation of women in the other country, and proposed to adapt the intervention to their own cultural situation. They set up a careful, comprehensive, and methodical project to address the factors that disinclined women to get screening for breast cancer. They were able to encourage women from a young age to undergo screening. The 'originality' of this project was simply in recognizing how to replicate and adapt a project that had succeeded elsewhere in order to meet a local need that had never been addressed. It was a very well developed proposal, and therefore was ranked highly and ultimately funded.

Therefore, it can be important to examine any unique characteristics of your local environment in looking for appropriate research questions. You may be able to make use of techniques available elsewhere, but apply them to a unique local situation to craft an original project which has local value and possibly even wider applicability.

Hypothesis-driven research

A hypothesis is formally defined as a tentative explanation accounting for a set of facts that can be tested by further experimentation. In other words, hypothesis-driven research asks a question that can be answered unambiguously by experiment. Features of a good hypothesis are that it should be **simple**, **clear** and **testable**. It should also lend itself to constructing a set of experiments that will prove it either true or false. Good hypotheses should **not** include words like 'may' 'might' or 'could' since these make the statement impossible to falsify (i.e., show that it is untrue). Nor should you include words like 'and' and 'or' since these make it difficult to distinguish which parts of the hypothesis you are testing. Newton's statement that '*objects attract each other by means of a gravitational force*' is an example of a good hypothesis. It is simple, clear and testable.

Here are two hypotheses which were presented at a junior science fair[1]:

(a) Aphid-infected plants that are exposed to ladybugs will have fewer aphids after a week than aphid-infected plants that are left untreated.
(b) Ladybugs are a good natural pesticide for treating aphid-infected plants.

The first statement (a) is a much better hypothesis than statement (b). It obviously leads to an experiment to test the hypothesis. Statement (b) is a comparative statement, i.e., it raises the question of 'good' in comparison to what? The potential for ladybugs to reduce aphid infection may be an important motivational concept for carrying out the research, but (b) is not the best formulation of a research hypothesis.

It is tempting to write about planning to 'study' a subject or 'investigate' an issue' but unless one can come up with a statement of a research question or hypothesis, in reasonably straightforward language, your proposal is likely to be in trouble.

This project seems to be just data gathering without a research hypothesis. There is no research question. It's fine to measure the incidence of heavy metals in the bottom sediments of a river, BUT so what? If you are not going to use the data to answer a question, then it is not research.

The US National Institutes of Health (NIH) insist on their PIs enumerating a number of specific aims in any proposal submitted. The following example gives an indication of a series of aims that would convince a reviewer that the goals of the proposal are clear. Note also the amount of detail in these statements, rather than vague generalities.

Examples of clearly laid out research questions.[2]

Reviewers appreciate the clarity and specificity of the formulation of a research question.

Specific Aim 1. To establish whether or not modulation of intracellular ubiquitin levels affects Thymidylate synthase (TS) turnover. Transfected cell lines exhibiting altered ubiquitin concentrations will be utilized for this purpose.

Specific Aim 2. To determine if TS mutants with altered intracellular stability are differentially ubiquitinylated. Mutants that have already been produced and partially characterized will be harnessed for this purpose.

Specific Aim 3. To determine if the surface loop of *E. coli* TS is responsible for that enzyme's relative resistance to ligand-mediated stabilization. This is to involve attempts to "humanize" the bacterial enzyme.

Many proposals do not provide a clear description of the research question they are seeking to address. Often the author of an unsuccessful proposal will call the funding agency to ask about the reasons why the proposal was declined. One technique that a program officer may use is to ask the PI to read over the first page of their proposal and state the research question that the proposal was asking. There are usually two responses. Commonly, the PI fumbles around verbally, unable to come up with a crisp research question. This leads the program officer to suggest that more work is needed to clarify the research hypothesis in the proposal. Alternatively, the PI may indeed be able to formulate a simple and succinct answer in which case the program officer may ask why the research question had not been expressed as clearly in the proposal. It is critically important to have a research question clearly formulated and to state it early in your proposal.

I have some difficulty finding a "research plan" in this proposal. Collecting data seems to me to be the plan!!

— Quote from a reviewer

The next example illustrates the lack of clarity about what research question is being asked.

This is an example, based on an actual proposal, of an attempt to formulate a research question but it leaves the reviewer with little or no information about the research hypothesis being tested. The proposal fared badly given the lack of a well-formulated hypothesis.

The first project objective is to collect as much data as possible for as many types of drivers as possible. The second objective will be to subject a limited sample of drivers to simple training in eco-driving, and subsequently evaluate the effect of this training on their driving behavior and carbon footprint. The first objective will be limited by the amount of data loggers that can be purchased as part of the fund. This is currently capped at 50 data loggers due to the budget. In order to increase the sample size to a statistically solid 150 samples, it is proposed that three consecutive measurement campaigns are conducted over the course of 3 months. Due to the relatively smaller sample size, the second objective will be difficult to achieve, hence care must be taken when selecting the subjects who will take part in the training in order to minimize the variables.

In projects in biological sciences and medicine, there is a very definite insistence among reviewers that the research needs to be 'hypothesis driven'. The research topic must be presented as a testable hypothesis that can be answered unambiguously by the research proposed.

When asked what makes a good proposal, one of our interviewees replied: Does it have a testable hypothesis? Is it a question that you can answer by experimentation, and that you can actually test your hypothesis?

— Quote from a reviewer

In some other fields of research, this insistence on 'hypothesis driven' research is adhered to less strictly. Nevertheless, the author should always try to be as specific as possible about the question or questions that the proposal addresses. Ideally, these should be expressed in a few simple sentences that make the question being asked very clear and very specific.

The project provides knowledge that has useful outcomes

Another aspect of a proposal that will help convince the reviewer of its merits is if the project has some clear benefit either to other fields (for example, developing a device or technology that will improve medical diagnosis) or if it has some potential practical outcome that might be beneficial to society. Researchers in the medical and biological sciences have an advantage in this regard, since the research in these fields generally benefit health. However, research in many other fields also has the potential to provide societal benefits, even if these are sometimes in the longer term. The example that is often given is the internet, which was initially developed to facilitate the transfer of large quantities of data in high-energy physics experiments, but has since become an indispensable tool of commerce, and part of daily life.

In addition, many countries that support research funding in the sciences and engineering do so with the expectation that these will lead to economic and social benefits for their citizens. This is often the case for countries with developing economies, but is increasingly true in all countries. In such cases, the CFP may contain language that encourages — or sometimes even requires — practical applications. Pointing to such examples will usually be beneficial. Therefore, if you can anticipate practical outcomes of your work, or if you know of companies that have an interest in these outcomes, drawing attention to this will add to the significance of your proposal. There is a more extensive discussion of impact in Chapter Seven.

The research proposal must respond to what the agency wants

While you may believe that you have an important research idea that will move your field forward, if it does not match what the funding agency is looking for your proposal will not be successful. Therefore, you must read the CFP carefully, and perhaps talk with the relevant Program Officer at the agency and make sure that you understand the funder's priorities. If you are seeking funding from a particular agency then you will need to align your goals with theirs. Dr. William Harris, the founding Director General of Science Foundation Ireland, used to call it the golden rule, *viz*, '**He who has the gold makes the rules!**'

For instance, if the agency is looking for proposals in animal cell technology to increase the expression of proteins in animals, there is no point in submitting a proposal for research in diabetes. Or if the CFP states that a 20% match of the total budget from a company is required, and if your proposal does not have that match, you are wasting your time in writing the proposal. The lesson is to read the CFP very carefully and design your proposal to address the priorities of the funding agency.

DO choose an original research topic, especially one that applies to your local environment.

DO try to avoid incremental research.

DO refer to any possible practical outcomes of your research project.

DO read the CFP carefully and make sure that your research project is consistent with the priorities of the agency as given.

DO NOT be too ambitious in your goals. Keep your project within the scope of the resources available.

2.4 Literature Review: Why this is an Important Aspect of Your Research Idea

As mentioned earlier, it is important to establish for the reviewers that you are familiar with the current status of your field. One excellent opportunity to do so is a literature survey. A concise but comprehensive survey can establish that you know the current work, as well as the outstanding problems in the field. Ideally, the reviewers of your proposal will themselves be very familiar with the field so that they can readily judge from your literature review that you know well what you are discussing and are not trying to cover up a lack of knowledge.

The literature survey also provides the **opportunity to place your research idea clearly in context**, to show that it builds on previous work, and to point out the remaining problem that it will solve, and the new understanding that it will bring. It is important to indicate to the reviewers that the research question that you pose is relevant, current and important.

Since research in almost all fields of science is a truly international endeavor, it is critical therefore to be familiar with and to quote relevant papers in a variety of international journals in your literature review. Using only local (in country) references will suggest to the reviewers that you are not really abreast of the current international science and therefore that it is unlikely that your research question will make an important contribution. Fortunately, with most journals now available online, it is much easier to have access to the latest results from a very wide range of international journals.

> *Only local references are cited. There have been many advances in this technology in recent years. I would have expected references from the leading mining journals. There is a very strong theoretical/modeling network in this area of hydrodynamics that should be referenced.*

A similar problem exists with using references that are not current. Nowadays, most science fields move very rapidly, and situations can change quickly. Therefore, if you do not refer to papers published within the past few years, the reviewers are likely to conclude that your knowledge of the field is not up to date. If there have been no relevant papers published recently, you should state this explicitly in your survey. However, be careful that you are correct since the reviewers may know of any counterexamples that exist.

> *I always look at the references in proposals that I review to be sure they are up to date. Also if I know that the work has already been done, this will kill the proposal. Reading the literature is critical.*

(Another mistake, unfortunately fairly common, is that the first task of the proposal is designated as 'carrying out a review of the literature'. This immediately gives a bad impression to the reviewer, as he or she would expect the authors to carry out a comprehensive literature survey **BEFORE** submitting a proposal, and not after it is funded. Otherwise how would the PI know that the proposed project is original and significant?)

This piece of a literature survey is an excellent example of how to provide such a survey. The references given in the survey [6] through [15] were all to international journals of high quality.

Brief survey of previous research

The use of carbon nanotubes in solar energy applications is a very active and fast-growing research field.[6] For example, carbon nanotubes have been implemented as electron acceptor materials in the photoactive layers of organic photovoltaic devices,[7] or as transparent electrodes for electrical current collection from the surface of thin-film solar cells.[8] Recently, high efficiencies have been reported for polymer/carbon nanotubes hybrid photovoltaic junctions.[9,10] In one case, the high efficiency was attributed to an effective dissociation of electron-hole pairs (excitons) at the nanotube/polymer interface,[9] and in the other to the polymer doping of the nanotubes at the interface leading to a built-in voltage that drives the efficient exciton dissociation.[10] However, the use of carbon nanotubes as the main photovoltaic element in a cell was only demonstrated recently, as mentioned earlier in section I.[5] The later device, as mentioned earlier, is based on the formation of a p-n junction along a single nanotube by electrostatic doping using a pair of split gate electrodes,[11,12] and exposing it to a focused light beam. Very recently, some other groups confirmed experimentally the photovoltaic effect in a single carbon nanotube,[13] few nanotubes connected in parallel,[14] and nanotube films.[15]

DO place your research idea clearly in the context of the current state of the field.

DO indicate why your research idea will make an important contribution.

DO make sure to include relevant papers from international journals in your literature survey.

DO make certain that the references in your literature survey include the most recent appropriate references.

DO NOT *only* refer to local (in country) literature.

DO NOT propose a 'review of the literature' in your work plan.

2.5 Presentation of the Research Idea

In many competitions for funding, large numbers of proposals are submitted and reviewers finish up reviewing quite a few. As a result, reviewers often find that all proposals in a particular area start to look alike. If your proposal looks a lot like the others it could be passed over. Remember this is a **competition for funding** and only a modest fraction of the submitted proposals are successful. Therefore, it is important to try to find some way to make your proposal stand out for the reviewers in a positive way.

One thing that is essential is to state your aim(s) clearly and briefly at the very beginning of the proposal, in the form of a testable hypothesis if at all possible. Reviewers will be looking for this, and if they do not find it early you will have a hard time getting their attention as they read on.

The importance of clarity cannot be overstated. Even though you may think that you have stated your research question clearly, you need to be sure that a reader, who may not be intimately familiar with your area of research, will also find it clear and unambiguous. One very simple technique is to **read the statement of your research idea aloud**. Often this will help you to realize that the statement as written is not as transparent as the one you have in your head. Another good way to test for clarity is to have a friend or colleague read the statement to see if they can understand it easily. We will return to the importance of other readers later in Chapter Four.

There are many ways to turn off a reviewer. One of the easiest (and one of the most annoying for reviewers!) is to have incorrect spelling and poor grammar. The reviewers we interviewed were unanimous in finding this annoying. If reviewers find poor spelling and grammar on the first page, they will often conclude that your approach to research is also careless, and they will be less inclined to read the remainder of your proposal with a positive attitude. Therefore, you should use all the tools available to make sure your use of language is correct, especially the statement of your research aim(s).

2.6 How Important is it to Present Preliminary Data as Part of the Proposal?

The use of preliminary data in proposals varies somewhat between scientific fields. In the general area of biological and medical sciences, preliminary data, which gives support to the proposed hypothesis, is becoming more and more important. Most reviewers in these areas will mark down a proposal severely if there is no preliminary data included with the proposal. This is usually not a problem for established researchers who have ongoing funding, but can be an issue for a new researcher who does not. In such cases, the usual source of funding is from internal funds within the institution, but, of course, these are usually quite limited. In some cases in biological and medical science, it may be better to delay submission of a proposal until preliminary results can be obtained.

In other fields of science and engineering, the inclusion of preliminary data is not so critical. However, if data are available, even if unpublished, it is usually useful to include such data in the proposal. **The more controversial the research aim of the proposal, the more useful it is to supply supporting data**. Similarly, if your research goal depends on a new technique or new piece of equipment, presenting even a small amount of preliminary data will help convince reviewers that you can achieve the goal.

2.7 Multidisciplinary Research Questions

Many research questions, particularly those that relate to real world situations and are, therefore, of a more applied nature, often involve more than one academic discipline. This poses special problems in developing a proposal, since it often means involving collaborators with expertise in different disciplinary areas to address the research question.

Reviewers will usually need to be convinced that the various collaborators are really engaged in the project, since their participation is essential for success. Letters from collaborators attached to the proposal is one way to deal with this issue. Or simply stating exactly how they will participate helps. If some of your collaborators have already participated with you on other proposals and have even published papers with you, this provides some evidence that you have a good working relationship. You need to point this out. In any case, you will need to pay some attention to making sure that the reviewers are certain that all your collaborators from various disciplines are willing to do their part to make the project a success.

In this proposal, the reviewer noted that the PI and a postdoc were in the budget to spend some time in a laboratory in Japan. This made sense to him, because this is where scientists from XXXX can learn forefront techniques and bring those back home. It also gives them exposure to new ideas.

How do you find appropriate collaborators? Often the problem itself will suggest not only the area, but even specific people who are interested in the same problem. There are, of course, advantages in working with collaborators in the same institution. You can meet easily face to face on a regular basis to report progress, discuss issues and solve problems. However, the internet has now made it possible to communicate readily across considerable distances that would have been difficult or impossible even a few years ago so this broadens the scope of possible collaborators considerably.

The issues around working with collaborators will be explored in more detail in Chapter Six.

2.8 Should the PI Meet with Agency Staff before Preparing a Proposal?

Every funding agency, whether governmental or private, will have its own priorities for funding research. While this information is generally available either in print or on the internet, we believe that it is also very useful to discuss particular ideas with program staff of the agency. This will often help give the researcher a clearer understanding of the current priorities in the agency, and could suggest a particular research idea that fits well with the priorities of the agency.

Meeting with agency staff and discussing your research also gives them an appreciation of your motivation and enthusiasm for your work. While most agency staff try very hard to be objective and professional in their decision making, it cannot hurt if they can put a face and a voice to a particular name. It is surprising how few researchers contact Program Officers in person. More experienced researchers often do this.

Agency staff are often helpful in discussing an unsuccessful proposal that the applicant intends to resubmit. They can help interpret the earlier reviews and point out directions that may prove more fruitful. In cases where a proposal has been discussed in a panel review process, the staff may be able to provide additional information that was not written down in the reviews. These insights can be helpful in preparing a resubmission.

2.9 Summary

- A good research idea is critical to a good proposal. Good ideas move the field forward and therefore it is important to be very familiar with the current status of the field.
- Keeping at the forefront of your field means reading the published literature, attending seminars and communicating with colleagues both locally, within your institution, and also at national and international conferences.
- Don't try to solve all the problems in your field in a single proposal. Be aware of the limitations of resources available in a particular competition. Make sure your research aim is specific and addresses a particular scientific question.

- Express your research aim as a testable hypothesis as far as possible.
- Emphasize the importance of your project early in the proposal to catch the attention of reviewers. The importance can be because of its potential impact on the science or the potential practical outcomes.
- Provide preliminary data, particularly if your field is biology or medicine. In other cases provide data if available. This is more important if your project is controversial and the basic concepts might be questioned by reviewers.
- Take the opportunity to visit the staff of the funding agency to see if they can give you additional insights into what the agency is looking for currently.

DO present a crisp, well-formulated research question early in your proposal.

DO make sure that you indicate, using up-to-date international literature, how your proposal builds on what is already known about the topic.

DO emphasize the significance of your project by pointing out how your research question will contribute to the knowledge base and/or to benefit of the country supporting the research.

DO make sure that you provide evidence of collaboration if this is needed to address a multidisciplinary research question.

DO NOT only refer to local literature in reviewing the field.

DO NOT simply state that you are going to ‘study’ or ‘investigate’ some phenomenon as your research goal.

DO NOT assume that the reviewers will be familiar with the specific field of the proposal.

DO NOT annoy the reviewers by having poor grammar or poor spelling especially in expressing your research idea.