Research Projects and Research Proposals

A Guide for Scientists Seeking Funding

PAUL G. CHAPIN

With a Foreword by Alan I. Leshner

CAMBRIDGE
UNIVERSITY PRESS

PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge, United Kingdom

CAMBRIDGE UNIVERSITY PRESS
The Edinburgh Building, Cambridge CB2 2RU, UK
40 West 20th Street, New York, NY 10011-4211, USA
477 Williamstown Road, Port Melbourne, VIC 3207, Australia
Ruiz de Alarcón 13, 28014 Madrid, Spain
Dock House, The Waterfront, Cape Town 8001, South Africa

http://www.cambridge.org

First published 2004

Printed in the United States of America

Typeface Palatino 10/13.5 pt. *System* $\LaTeX\,2_\varepsilon$ [TB]

A catalog record for this book is available from the British Library.

Library of Congress Cataloging in Publication Data

Chapin, Paul G., 1938–

Research projects and research proposals : a guide for scientists seeking funding / by Paul G. Chapin.

p. cm.

Includes bibliographical references and index.

ISBN 0-521-83015-X – ISBN 0-521-53716-9 (pb.)

1. Science – Research grants – Handbooks, manuals, etc. I. Title.

Q180.55.G7C44 2004
507′.973 – dc22 2003069568

ISBN 0 521 83015 x hardback
ISBN 0 521 53716 9 paperback

Contents

1

Selecting a Research Topic

Every research project begins with an idea – a question whose answer nobody knows yet, and a guess as to what that answer might be. Finding the right question to ask is often the most difficult task facing the researcher. The best scientists seem to have a knack for formulating interesting, productive research topics. Although it is probably beyond the ability of a book or a teacher to impart this skill directly, we can identify some of the distinguishing characteristics of a well-chosen research topic.

The context constrains the choice. This book is about planning and proposing research projects for funding, so that is the context we will assume here. Other contexts, such as selecting a topic for a term paper or for a lifelong pursuit, might lead to a different choice.

A good research topic successfully balances a set of desirable but conflicting goals. These goals fall into a number of opposing pairs that you can think of as the endpoints or poles of dimensions that describe the topic.

The Dimensions of Topic Selection

1. Focused vs. **Extended**. Research is an attempt to shine a beam of light into a dark place. Like a beam of light, research can be focused more or less sharply, and the choice of focus can be a key to determining how fruitful the research turns out to be.

The topical focus must be clear and sharp. It needs to be clear enough to make the empirical content of the research question evident, and what an answer to that question would mean. It needs to be sharp enough to indicate, not only to the investigator but also to others a logical line of research to pursue the topic – what the first steps will be, and the steps after that.

Let's consider an example. Suppose you are a cognitive scientist about to organize a new research project, and you're interested in memory. How do you formulate the research topic? What is the right degree of focus, the right level of specificity? Clearly "memory" is far too broad. So you narrow the focus successively to "human memory," "children's memory," and "development of memory in children" – all still too broad because they subsume too many different researchable questions, but getting better. Looking at the role of formal schooling in the development of memory in children is getting close, but there are still enough different things going on in school that this doesn't define the question quite sharply enough. So you zero in on research on "linking teacher talk and the development of children's memory" (which is the title of an actual research project funded by the NSF in September 2002).

But the dimension has an opposing pole to consider as well. The research topic, while focused enough to be clear, also must be broad enough to be interesting. If it is too tightly constrained, the answers that emerge from the research, however well-defined and well-grounded, will not extend to the constellation of closely related questions that interest others.

In our example, the investigator might have narrowed the focus further; say, to female children, or to teachers over 40 years old, or to teacher talk at the beginning or the end of the school day. Perhaps as the research progresses and we learn more about the topic, there will be reason to narrow the focus in these or other ways. But for this subject at this time, those restrictions would only diminish the interest of the research.

To return to the light beam metaphor, if you want to illuminate a particular patch on a wall, you are best served by a light beam that is focused to the approximate dimensions of the patch, rather than one which diffusely lights the entire wall, or another which narrowly shines on only a small portion of the patch.

It should already have become apparent that the proper point on the dimension of focus depends greatly on the particular field of research, and the current state of the art of research in that field. This is true of all of the dimensions that we will discuss, and is a point that we will return to later.

2. **Novel** vs. **Grounded**. There is a canard about grant funding to the effect that you can only get funded for work you've already done, that the proposal review process is too conservative to be receptive to genuinely new ideas. In fact, the opposite is closer to the truth. It is a sure death sentence to a proposal if a reviewer can demonstrate that the proposed research has already been done. Only truly novel work can make the kind of contribution to our knowledge that merits support from a funding agency or, for that matter, attention from colleagues in your field.

Novelty in the scientific sense requires more than mere difference. A proposed experiment can be different from any performed before without being novel, if the difference is not in some key variable or variables that are essential to the result. Suppose, for example, that you are studying brain responses to auditory stimuli, and you are able to determine from the literature on the topic that in all previous studies, the subjects were either seated or prone. Now you could propose to do the experiment differently by having the subjects standing; however, to make the case that this is a truly novel experiment, you will have to argue persuasively that there is some reason to believe that bodily posture plays a significant role in the neurophysiology of audition. A pilot experiment suggesting such a role could help to make the case, and perhaps that option lies behind the canard mentioned above, but there is a big difference between doing a pilot experiment to motivate undertaking a line of research and actually carrying out the research.

While research must be novel to merit attention and funding, it must at the same time be well-grounded in established scientific knowledge. The successful construction of a perpetual motion device, or of cold fusion, to take a more recent example, would certainly be novel, but inconveniently they violate known laws of physics. Reviewers would not take seriously a proposal for one of these projects unless the proposal could make a persuasive case for revisions in the physical laws. These examples are far-fetched enough, and familiar enough, to be amusing, but in fact it is all too frequent for proposals to leap so far beyond current knowledge and technique that reviewers lose confidence in the investigator's ability to carry out the research proposed.

The literature review section of a research proposal, which we will discuss more fully later, is important here, not only for the case it makes to reviewers of how well-grounded and how novel the proposed research is, but also because preparing it can help you select a research topic that is adequately grounded and still interestingly novel.

3. **Feasible** vs. **Challenging**. It is important to select a research topic that you can realistically undertake and complete with the time and resources available to you. This is related to the matter of groundedness. A topic has to be scientifically feasible, given the current state of the field, and a poorly grounded project is likely to be *a priori* infeasible. Beyond that, the feasibility must be practical as well. Even a project that is eminently well-grounded from a scientific point of view may not be feasible in terms of its cost, its facilities requirements, or the time it will take to carry it out. This is most commonly a problem for inexperienced investigators, who may not yet have internalized a sense of what it takes to carry out a project to completion, but it can happen on a grand scale as well, as the history and demise of the Superconducting Super Collider attest.

Of course, when you are writing a proposal for submission to a funding agency, you are requesting resources to make it possible for you to carry out a project that might be infeasible without the requested support. So the feasibility of a proposed project is contingent on receiving the funding requested, but is still an important factor in the evaluation of the proposal. Reviewers have to agree that the resources available to the investigator, including the funding requested if it is awarded, match the needs of the project. Feasibility may also be contingent on other factors, such as favorable environmental conditions, and you need to be aware of these and reflect that awareness in your proposal.

The other pole of this dimension is that the project should be a challenge to carry out, both intellectually and practically. It should lie just at the threshold of feasibility. The practical challenge ensures that you will stretch the resources and make the most effective and productive use of them, and of your own time and effort, that is possible. The intellectual challenge will beget the most exciting results if the project succeeds. Again, this dimension is something that reviewers will attend to in evaluating your proposal, but is also something that you will want to get right on your own behalf, to maximize the results of your efforts.

4. Theoretical vs. **Empirical**. A common criticism of a research proposal that is inadequately grounded in a theoretical framework is to call it a "fishing expedition." A proposal of this sort defines a set of data to collect, and procedures for collecting the data, but does not explain the purpose or the significance of the effort, other than to characterize the phenomena to be studied as important. The investigator apparently expects the regularities and patterns in the data, and the significance of those patterns, to become self-evident once the data are gathered and laid out for inspection. Experienced scientists know that it rarely works that way. It is much more productive to approach the research problem with a theoretical model, however tentative and incomplete, that suggests that the world is organized in a certain way, with regard to the phenomena under study. The model will inevitably contain gaps that generate questions to explore, and the investigator will have guesses – hypotheses – as to the answers to the questions. These questions and hypothesized answers will provide the logical foundation for a plan of research that has direction and purpose. Of course, the results of the research are likely to show that the theoretical model was wrong in some way and must be revised before generating more questions to study, but that's progress, and a more valuable outcome than to have a collection of data of unclear relevance or significance.

It's possible to go too far in the opposite direction, however. Research that focuses exclusively on questions internal to a formal theory, without predicting or testing their empirical consequences, is likely to strike reviewers as sterile. There are some exceptions in fields with a highly developed theoretical apparatus such as physics or economics (and of course, mathematics is a case of its own), but in most fields, the empirical content of a proposed line of research needs to be clear.

5. Near-Term Results vs. **Long-Term Prospects**. The requisites of planning research projects to attract a funding sponsor give this dualism its importance. The typical length of funding that an investigator, particularly a new investigator, can expect in response to a successful research proposal is two to three years – five at most. If you hope to attract further funding after that, it's essential that the first period of funding lead to some useful results. In an ideal world, there might well be superb research projects that would not yield their results before ten or more years of work. But in the world we live in, significant results must appear at more frequent intervals if funding support is to continue, and you have to take this fact of life into account in selecting a research topic. It's fine, indeed desirable, to have a long-term vision of where your research is headed, but you have to divide that long-range plan up into individual stages of a few years each that can yield valuable results on their own.

On the other hand, a topic whose possibilities you can exhaust with two or three years of research is probably too narrowly defined to interest people evaluating the proposal. An exciting research topic is one that will yield results that open up new avenues for further research – and in the process, not so incidentally, provide the basis for a later proposal for renewed support.

The Goldilocks Principle

When Goldilocks visited the house of the three bears, she tried out their food and their furniture to see how it all suited her tastes. She found one bowl of porridge too hot, another too cold, but the third one just right. She sat in a chair that was too hard, another that was too soft, and a third that was just right. Ditto for the beds.

Your task as an investigator selecting a research topic is similar to Goldilocks'. Along the various dimensions that we've just discussed, you want to find the best point to situate your own work. You want a topic that is neither too broad nor too narrow, novel but still grounded, feasible but challenging, theoretically motivated but theory-transcendent, offering both near-term results and long-term prospects. On each of these dimensions, you want what Goldilocks wanted – to find the point that is Just Right.

As we've mentioned before, the Just-Right point on each of the dimensions depends heavily on the field of research and on the state of the art in that field at the time you're planning the project. A brand-new line of research that follows up some exciting recent discovery will probably be more novel and less tightly focused than work within a paradigm that has been extensively explored. Research that depends on the use of extremely expensive equipment may allow a longer time line for useful results than research that is more personnel-intensive. And so on. Your challenge as a creative scientist is to make the right choice for your field and your time.

How do you know what the right choice is? You don't, not really, not at the beginning of research, which by its nature is an unpredictable enterprise. You find out by the results how apt the choice of topic really was. But there are things you can do to help give you the best chance to select a good topic. First, be active in your field. Go to professional meetings to hear what others are doing and discuss it with them. Keep up with the current literature. Keeping tabs on what's going on in this way helps you to know what is novel and what is familiar, and also gives you a sense for such things as the appropriate level of focus. Second, as you develop tentative ideas for a research topic, share them with others whose opinions you value. You can do this in individual conversations; sometimes an informal colloquium with colleagues is a good forum for airing your ideas. The reactions you get may indicate to you problems and possibilities that you hadn't considered, ways to improve the topic, or reasons to abandon it. You might even find a collaborator in the process. Once you have selected a topic, it is going to dominate a lot of your thought and time and effort for a good long while, so it pays to get it Just Right.

Project Scale and Duration

Deciding how much time and what resources you will devote to a project might seem to be a topic for the next chapter, on project planning, but in fact these decisions are crucial in topic selection because they constrain the choice of topic in significant ways. Moreover, external factors impose practical limits on the time and resources that you can hope to employ in your project. To a large extent, you can get a good sense of these limits while you are still in the topic selection phase of the project, and you can use that knowledge to help you zero in on a topic that fits them.

Seasoned investigators with strong track records understandably have broader latitude in the amount of time and funding that they can realistically request for a project than a freshly-minted scientist submitting a first proposal. For new researchers, the norms are fairly well established. The duration of funding that you can expect to receive is likely to be two or three years (CAREER grants, which we discuss in Chapter 4, run longer). The roster of personnel that the funding will support are a Principal Investigator, full-time during the summer and possibly part-time during the academic year; a graduate student research assistant (if you're at an institution that has grad students); and one or two undergraduates. A grant can also include funding for equipment, travel, supplies, and other expenses of research. We will return to these items in the chapter on proposal writing, but they are usually not limiting factors in topic selection in the same way that duration and staff are.

We just mentioned that the duration of a funded project is likely to be two or three years. Three-year (and longer) projects and proposals actually differ somewhat in kind from two-year projects and proposals, not just in duration, and you need to decide at the outset which kind of project you are proposing. You should request two years of funding if (1) it is reasonable to expect that the full project can be completed in that amount of time, *or* (2) the project is so open-ended that you can't foresee very clearly what you'll be doing by the time the project enters its third year. An example of the latter type of project is a highly theoretical or mathematical line of research in which each interim result opens up a whole new set of possible paths to follow, and it's difficult or impossible to predict which particular branching paths you will be on by the third year. Reviewers are unlikely to react warmly to a proposal when they have to guess what will happen after some point in the funding period. Conversely, a three-year proposal is appropriate if (1) you can lay out a clear and specific line of research for three years, *and* (2) it is reasonable to expect the full project to take at least that long. An example here is a series of laboratory experiments that explore different variables in a topical phenomenon. You can describe in advance how you will do each experiment, accurately project how long each experiment will take, and show that collectively the experiments will require three years to perform. In some fields, such as ecology, the time scales of the phenomena studied dictate projects and grants of longer duration, and here three- or four-year grants are the norm.

Bear in mind that duration is negotiable. That is, you can propose a project with a stated duration, and if the reviewers and funding agency like the project but believe that it should be funded for less (or more!) time than you have requested, the agency can offer to fund a modified form of the project. It will then be up to you to decide whether you want to undertake the project as modified. This is getting ahead of ourselves at this stage, but the point here is that you should feel free to propose the project duration you consider most appropriate for your topic without worrying that the proposed duration by itself will damage your chances for funding.

The Most Important Thing

We've tried in this chapter to help you organize and focus your thinking as you select a topic for your research. After all of the considerations of focus, novelty, feasibility, duration, and the rest, however, the single most important factor is finally your own preference. Pick a topic that excites you profoundly, a question that arouses your intense curiosity. Life is too short, and good research too difficult, to do anything else. You will write the best proposal on the topic that interests you the most, and if the proposal is funded, that is the topic that you will do the best research on. Although you need to be mindful of the overall constraints imposed by funding agencies on such matters as project duration and budget, it is a serious mistake to try to guess what the funder wants and tailor your research accordingly. Chances are the funder doesn't have that clear an idea of what will be the best work anyway, and is looking to the proposals to find out. And if you should receive a grant for doing research that you don't care passionately about, you may find carrying out the project to be a burden rather than the joy that it ought to be.