Chapter 1
The Nature of Research and Innovation

1.1 Introduction

Scientific research encompasses many different styles of activity. The nature of the work varies significantly, depending on the problem and the discipline. The work practices in laboratory-based experimental studies are distinctly different from field-based observations, or theoretical investigations. Nevertheless, broad principles can be recognised that unify the approach to goal-driven research. It is the aim of this short book to provide a guide to successful practice in research, drawing on experience of major research projects and research management.

An important first step is to understand the context of a research project, and the classes of circumstances and limitations that are likely to be associated with a particular class of activity. Projects do not exist in isolation, so it is useful to look across the full range of scientific endeavour because analogies and lessons can often be drawn from other fields.

In this first chapter we look at the nature of scientific research and innovation in a general way. It is useful to develop the skill to look at a project from the outside, and so attempt to see it in a broader context. We examine the nature of styles of research and the classes of external constraints and the way that they influence the nature of the enterprise.

The second chapter is concerned with the full life cycle of a project from concepts, through planning to execution. We draw on both research and management principles. We examine the construction and assessment of project proposals, and the practical problems to be faced once a project is under way. Project development (either formal or informal) lies at the heart of research, and presents many challenges — in managing the process, the people involved and the reporting requirements. Forethought helps greatly with project management and, in the third chapter, I provide insight into project planning and tracking based on successful projects.

An important part of research activity is the effective communication of results, so that work is known and linked to the broader research scene. In the fourth chapter we consider modes of communication from preparing a paper for publication, delivering seminars or conference presentations and establishing an internet presence for a project.

The fifth chapter is directed to a range of issues that arise from the nature of research and the necessary human interactions. We consider relations within a research team and, more broadly, recognising incipient problems and more fundamental ethical issues.
The sixth chapter presents two case studies, based on the author’s experience, to illustrate the need for dynamic management and adaptability to achieve a successful outcome. In reality few projects proceed entirely as expected, and some planning for contingencies can markedly improve outcomes. Changes along the way can be positive, for example, when new methods become available, rather than just negative, as when the external environment changes. In any case, adaptation is the key to resilience and long term success.

1.2 What is research?

Scientific research is characterised by a systematic approach to the generation of new knowledge, building on previous work yet subjecting it to close scrutiny to determine any failings. Irrespective of the type of research activity (theoretical, experimental, observational) the results have to be substantiated, and the work must be reproducible. The continual process of testing and crosschecking from many different quarters leads to a consensus on the major aspects of scientific fields.

The presence of this consensus position provides a good basis for the broad advance of a field, but can provide a barrier to the acceptance of new concepts, results or methods that lie outside the current framework. It can take persistence to deflect the path of a field, but once a new framework has been accepted it becomes the new norm. Thomas Kuhn has termed this schema of gradual evolution, punctuated with major shifts in viewpoint, a ‘paradigm shift’. The basis of observational and experimental results remains unchanged, but is now interpreted in new ways. Commonly it is the advent of new classes of result, which are discordant with the existing framework, or are only representable in a clumsy way, that forces ultimate change. The Copernican revolution to a heliocentric solar system, the development of quantum theory, and the adoption of plate tectonics in the Earth sciences represent three such major shifts in conceptual framework. The decoding of the nature of DNA has radically changed the nature of biological research, and modified the emphasis of the research endeavour leading to a shift in focus.

Yet, the new standpoint is not immutable. Our conventional structures are best regarded as working hypotheses under continual test. When discrepant results arise, and are validated by independent work, the early steps generally involve some patching of the structure, for example, by introducing additional complications. Then, finally, a new concept emerges to explain the full suite of results in a more elegant way. It is not given to most researchers to effect major shifts in their fields, but nevertheless results can have unexpected significance.
Current science represents a large and complex enterprise, and research has acquired a varied apparatus of funding schemes and expectations. I hope that the advice in this book will help researchers to recognise that many aspects of the research environment are common to all fields. Good planning and organisation cannot replace scientific insight, but will help to maximise research returns.

Building new research results into innovative products and services is undertaken in a somewhat different way than unconstrained research. The funding sources tend to come with stronger expectations and greater restrictions. I will therefore try to indicate the differences that arise in different circumstances, whilst emphasising generally applicable principles.

1.3 Styles of research and innovation

In many contexts in research we are required to classify the nature of the work being done, such as at the time of submission of a proposal to a funding agency. Such information is also frequently collected for statistical purposes. As we shall see, it is not entirely straightforward to classify the nature of research, and the pattern of research activity can change over the course of a project.

Many researchers spend their career in individual or small-group activities. Yet, there is an increasing tendency towards larger collaborative ventures that cross disciplinary boundaries. Larger programs bring with them the need for more explicit organisation and management, which tends to impose more constraints on constituent projects.

1.3.1 Categories of research

The first type of situation in which a researcher will come into contact with classifying research is usually in submission of a paper for publication. A preliminary sorting in relation to the scientific discipline base will have occurred with the choice of the journal, but then the authors are expected to provide specific information on sub-fields that is employed in the review process. Similar considerations enter in the submission of grant proposals.

Discipline and objectives

Research activities can be classified by scientific discipline, or the goal of the work such as mineral exploration. Indeed both types of information are complementary and help to place the nature of the work in context.

A wide range of classification systems are used in different countries
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Table 1.1  Field of Research Codes for Earth Sciences (ANZSRC 2008)

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across the world. There are similarities in style, but the way in which disciplines are recognised and the choice of discipline groupings vary considerably. Classification schemes designed originally for statistical purposes are often employed to specify the character of research in grant proposals. A short extract from the Australian and New Zealand Standard Research Classification (ANZSRC), 2008 Fields of Research Codes for the Earth Sciences is shown in Table 1. An attempt is made to break down the main topic into a set of specified sub-fields, with a final entry to capture what has otherwise been missed. Rather than being confined to a single sub-field, it is usually possible to provide percentage of effort distributed across a number of codes.

These classification schemes for research activity tend to remain in place for many years, and so do not readily take into account the evolution of a field; nor are they generally suited to interdisciplinary work. Revision is not simple, even where extensive consultation is undertaken, and often simply shifts the location of problems. Even so it is usually possible to achieve a reasonable alignment on discipline specifiers.

There tend to be more problems with classifiers directed at the areas that will benefit from the research, since these tend to be oriented towards specific applications. Such socio-economic objectives have taken on greater significance in recent years as many countries see a major role for public investment in research as aiming for wealth generation in the national interest. The classifications for economic activity are frequently finely graded for industrial applications with many entries. Yet, in many cases, a pure research component is relegated to a subsidiary category assigned to a broad field, for example, Physical Sciences.
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Such categorisation of research may seem esoteric when first encountered, but is frequently required for a project in formal grant applications. The specification of discipline, or of proportions between disciplines, needs to be considered carefully since it may well play a role in the assignment of assessors for projects.

Character of research

A further class of information that is frequently sought about research projects concerns the character of the research being undertaken. This is often presented in terms of choices from three broad classes of activity: basic research, strategic basic research and applied research. Even though these terms are regularly used there is little uniformity in definitions; in part because the terms may be regarded as self explanatory.

We can indicate the character of these three research categories as follows:

- **Basic** – curiosity driven though often tied to understanding a broad class of problem; sometimes termed ‘blue sky?’ research as, for example, the search for the unification of gravitation and the other physical forces in a single theory.

- **Strategic Basic** – research directed towards a clearly defined goal, can frequently include a strong component of new knowledge.

- **Applied** – research directed at a specific topic with the intention of immediate application, generally builds on earlier more fundamental work, but can often include the exploitation of existing knowledge in new ways.

The boundaries between these three categories are blurred, and an individual project may contain components of all three. The balance between the different aspects and styles of research may also change as a project evolves.

The three-stage classification of

- **Basic**
- **Strategic Basic**
- **Applied**

is too coarse to represent the nuances of many projects. A five-point scale with intermediate states is probably more representative of the gradations encountered between projects.

Alternatively one can think about assigning a three-stage classification to different aspects of the work:

- **Concept**
- **Research in progress**
- **Delivery of results**

and then derive an aggregate result.
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The perception of those working on a project and outside observers will frequently be different. Often the insider perceives a basic component that is not as evident from outside. In the author’s experience the strategic basic element is a significant component in many projects, even if not explicitly stated.

A description of research character is frequently sought in relation to statistical information on the way in which national research effort is directed. The broad classes can also be linked to socio-economic objectives: as the research takes on a more strategic or applied character it becomes easier to assign components of the research to specific categories.

Exercise 1-1:
Collect information on a group of research projects from your own or a cognate institution. Analyse the projects in terms of the discipline classifications appropriate to your research environment, and also assign the style of research. If possible, compare your classifications with those assigned by members of the particular project. Where do you differ and why?

1.3.2 Types of research activities

The way in which research is undertaken depends strongly on the style of research activity. In particular the organisation of the work and interactions with other researchers are affected by the nature and scale of the funding source. Across the globe there is a tendency to encourage large collaborative programs with a number of subsidiary projects. The increase in size brings with it both organisational and financial issues that require explicit management. In smaller projects informal management can be adequate.

Many researchers are unhappy with the concept of research management, since they wish to concentrate solely on the research component, and see management as purely associated with administrative chores. In fact most people employ informal management techniques when they make decisions about where to put in their next effort on a project. As we shall see, even modest projects can benefit from clear planning and tracking using simple tools.

Individual and collaborative research

This is the traditional model of individual, or small group, research activity, which on a larger scale merges into projects built by voluntary collaboration of disciplinary specialists. The projects are typically funded by individual or collaborative investigator grants from a national research agency or
1.3 Styles of research and innovation

Research projects can span any of the three research categories; the main differences will arise in the project definition, rather than in the way the project is carried out.

Such grant-supported projects usually have short-term funding (e.g., 2–3 years) and modest reporting requirements. Often projects are pushing at the financial boundaries to achieve their goals, so that monetary pressures can be significant.

At this level much of the management of the project is linked to the process of gaining adequate funding, and establishing good linkages with collaborators. The projects are initiated by the investigators, and to secure funding have clearly developed goals, but little, or no, formal structure.

Multi / interdisciplinary projects

Once projects begin to transcend discipline boundaries, more explicit effort is required to encourage the necessary levels of collaboration between researchers with different backgrounds. Such research activities tend to focus on strategic basic and applied issues, and are most effective where they build on disciplinary strength. Commonly, several investigators will be involved bringing together their expertise to tackle this class of broader issue. Real effort is then needed to set up the necessary interactions between the components.

At an early stage in such projects, an important step is bringing the disparate group of researchers together to establish good communications and a clear understanding of the roles of different participants. Interaction between researchers then needs to be sustained, so that disciplinary strengths can be translated into an effective synergy that bridges the disciplinary boundaries.

Such a complex project can benefit from clear research management, with coordination of financial issues between the multiple partners. There also needs to be an effective path for information flow so that the strands of work can move together.

Many such projects build on data or information from prior results, and there is likely to be a heavy dependence on the quality of metadata, the description of the content and nature of data and how it was acquired.

Such metadata become critical in many areas that exploit large databases. Considerable work is needed to ensure that all the information necessary for full exploitation in the future is indeed provided. Unfortunately this component of work is rarely given high priority, and has a tendency to be the first to feel the impact of budget shortfalls. The consequences may not be felt until much later.

As emphasis is placed on scrutiny of contentious results, and questions of
reproducibility become important, the need for effective *metadata* increases. In the policy arena assertions of suitable courses of action are no longer adequate, and need to be accompanied by explanations of how the advice has been derived and the results on which it is based. These may include experimental results or large-scale computations. In each case sufficient information needs to be present so that, in principle at least, the research can be repeated independently.

**Large research programs**

Large research programs are now funded by many agencies, and tend to have longer duration and comprise multiple research projects. The reporting requirements and financial control are more complex, so that there is a need for a structure that achieves coordination between the projects. The best results are generally obtained where the projects can influence the overall direction, and there is good communication between projects. For such large programs it is important to establish a cooperative mode of operation between the different component projects, though some inter-project rivalry can be healthy.

A major difficulty in the management of large programs is keeping all projects working on the same time frames. Often, continuing funding is dependent on meeting key *milestones* and *performance indicators*, and this can only be achieved if the various projects provide adequate reporting. Clear advance planning and well-thought out funding agreements at the beginning of the program are important. Many large research programs use independent boards to provide oversight and advice to management. It is important that the relative roles of the program management and the board are clearly defined from the outset, since otherwise much effort can be expended in unnecessary conflict.

The character of a large program depends on the nature and style of the funding source. In some cases the large program is built from the bottom up, by assembling a set of strands promoted by individual investigators. Even then, once the entire program is in view, gaps or needs for linkage may be identified that require directed projects. In other cases the broad outlines of a program are agreed by the participating institutions. Once the funding has been secured, the organisational structure of themes and projects is established. In this case individual investigators have less direct input to the nature of projects, but there are stronger external expectations with regard to project outcomes.

**Commissioned research or innovation**

Most work in this category falls into the applied field, but sometimes the starting point is sufficiently far from the specific application that it lies
in the strategic basic domain. Since the work is commissioned, there is less freedom of action. Considerable care is need with the agreed project definition so that the desired outcomes are feasible. Difficulties can arise where the client wishes to get an answer to a specific question that may represent the symptom of a problem rather than a topic that is directly amenable to attack. In such a case thought needs to be given to examining the problem to see if the project definition can be recast so it is more feasible.

The client will frequently have a fixed budget, which may not be well matched to project needs. This constraint means that project costing has to be done carefully, recognising that people costs may prove a limiting factor. It is unwise to take on a project where expectations and budget do not match.

For commissioned research, extra effort is needed to develop a research plan that satisfies the client’s needs, and yet allows sufficient flexibility to allow for development in the light of experience.

**Industrial research**

Research in an industrial context is closely linked to the economic fortunes of the entity, and can be subject to rapid change or closure if developments have made a project unprofitable, even if the science component is proceeding well. A typical enterprise will have a mix of tactical work (immediate problem-solving), and longer term evolutionary or revolutionary research. The strategic work is the class that may involve external commissioned work or collaborative projects, such as those funded through the Australian Research Council (ARC) Linkage Program.

A strong feature of industrial research is an emphasis on delivery of agreed milestones, and designated review points to assess progress against goals and financial viability. An emphasis of rapid delivery of results means that research teams are expected to be flexible and interdisciplinary. This may require team members to acquire new disciplinary skill sets in a short time frame.

The different cultures in the industrial and academic environments can lead to somewhat divergent views of the same activity in commissioned research. It therefore pays to spend some time on developing a mutual understanding before agreeing to take on such projects.

**1.4 Limitations on research activities**

Many projects can be self-contained, but few start from scratch. Commonly the project definition will have been built on previously published results and depend, in part, on earlier data, experimental procedures or theoretical
frameworks. Such dependencies need to be recognised. Deviations from expectations during a project do not necessarily imply that a project is going wrong, but do need to be recognised and monitored.

### 1.4.1 Response to external influences

The most obvious case where external influences play a critical role is in large research programs with conditional funding. Apparently burdensome reporting requirements at the project level to the central administration are frequently linked to those on the program as a whole. Coordination of many projects requires information transfer in a more extensive and detailed manner than would satisfy a project’s internal needs. Similar issues arise in collaborative projects, since the information level has to be sufficient for full understanding across all participants.

Large programs may also bring with them the requirement to work with particular facilities. In this case, the issue of coordinating such tasks as sample preparation with the availability of equipment can present challenges. Access timetables need to be clearly established and well publicised. Even within a single institution, competition for access time between multiple activities can produce problems and tensions of a similar nature.

A different type of dependencies on factors external to a project arises when there is a need to exploit prior data or samples. There is always the possibility of some class of contamination due to poor collection or analysis procedures. This makes the role of the associated metadata critical; yet often this is precisely what is missing. Even within a single research group it can be difficult to reconstruct metadata if it was not recorded in an appropriate form at the time the work was carried out. Notebooks can get lost, likewise old computer files.

Another insidious influence can come from previously collected data and analyses based on a set of assumptions that are no longer adequate. A particular difficulty arises when the original information is sifted, so that components not concordant with a specific model have been suppressed. Such outliers may well contain extra information that allows a model to be challenged.

It always needs to be recognised that there are major differences in research between the exploitation of existing data and the collection of new, specific data. Although work is required to assemble prior information in a form that can exploited for new analysis, this process is usually quicker than collecting new results. It is easy to underestimate the effort needed to set up a new laboratory program or fieldwork campaign. Thus, though
new results can meet the strictest protocols, there will be a period before they become available. 

On occasion it can be advantageous to consider outsourcing some aspects of the work associated with a project, particularly some routine activities. Often there are cost benefits in using a specialised agent, but thereby some control is lost. In particular the timeliness of delivery of results can be an issue. Planning needs to take this into account. It is not good to have a critical dependence on outsourcing, even if there are financial levers on those undertaking the work.

Where a number of investigators are collaborating, it is important to maintain communication so that the different strands of the work can proceed in an effective fashion. Frequently some component of the project will depend on an element undertaken by someone else. The consequences if a component takes longer than expected can be minimised if participants are aware of the delay, particularly if critical dependencies have been identified at the planning stage.

The nature of research is such that, unless a project is a small modification of prior work, some degree of uncertainty and uneven progress will be present. Some researchers prefer to work simultaneously on two, or more, projects so that they can progress one while working out how to overcome bottlenecks on another. Quite apart from the dilution of effort applied to each project, juggling the demands of multiple projects creates its own problems. For example, multiple reporting requirements can fall due at the same time leading to awkward deadlines.

1.4.2 Innovation within boundaries

As we have seen there are many circumstances in which aspects of a research project are constrained. The obvious case is commissioned research, but the constituent projects of a larger research program have to fit in that structure. In such circumstances it is important to understand the specific environment and structure within which the research project sits. For commissioned research there will be pressure for delivery, and it is therefore important to avoid overstating research potential at the outset. It is much better to over-deliver than under-deliver. Much depends on the framing of the questions to be addressed, and the mutual understanding of the parties involved.

In all cases where there are constraints a clear project design and time framework are essential. In particular internally designated milestones can be used as a constructive tool. These should be linked to any externally imposed requirements, but do not have to be visible outside the project itself.
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1.5 Developing a research topic

A critical component of research is the setting up of a new research topic. Even when this builds on previous work it is worthwhile to spend some time looking at the issues afresh, since the external environment may have changed.

Early in a research career it is likely that the main lines of the topic and the mode of attack will be specified, or at least suggested, by a research supervisor. Nevertheless, it is worthwhile to develop the skills of looking at the project in the light of cognate work in the field.

For a new project the first step is the definition of the questions to be addressed, and an idea of the resources that are needed to make it happen. With this in mind it is then possible to build up a specification of the topic in a way that can form the basis of a proposal, either informal as a means of guiding the subsequent research, or formal as in submission to a funding agency. In the following chapter we will discuss the steps once the topic is defined.

Much of the development of a research topic can appear to grow organically: an idea is discussed and developed to the point where it becomes sufficiently coherent for the outlines of a research concept to emerge. Alternatively, a call for proposals or the submission deadline for a funding agency may stimulate the need for a well-defined research topic conforming to particular requirements.

The set of ideas below are drawn from my experience in developing a wide range of research topics over the years. When only one piece of research is in train at a time the process can be fairly loose, but once a number of stands of research are under way at the same time, it is helpful to have each well specified. In this way interdependencies become clear, and basic documentation exists that can be helpful in recruiting the necessary collaborators for larger projects or certain types of funding schemes.

1.5.1 Framing research questions

A fundamental component of success in research depends on being able to pose a suitable set of questions from which a project can grow. This is not a simple process since relatively innocuous issues may prove to be difficult to pursue with success.

A group of topics that need to be considered in the framing of a project include:

- What do you want to know?
- Incremental ideas or new direction?
1.5 Developing a research topic

- Interesting to yourself and others?
- Important in its own right or in combination with other work?
- How much work is required?

It may seem trite to ask ‘what do you need to know’, but frequently the difference between a very successful project and a less successful one comes down to the way that the issue is addressed. For example, if there is a specific problem the obvious approach is to tackle this issue directly. However, it may well be that the most successful way is to take a more fundamental line, and search for the root cause of which the problem is a symptom. Such situations are not uncommon in commissioned research, but are not confined to this area.

The pattern of research activity is somewhat different when one can build directly on previous work, compared with striking out a new direction. Where one can exploit earlier relevant studies the initial stages can move more rapidly, but progress may flag when new issues are encountered.

It is difficult to codify the process of innovation. There is no universal formula for success in research. As noted by Louis Pasteur: *in the field of observation chance favours the prepared mind*. Results that do not fit with preconceptions are likely to be more important than those that simply corroborate expectations. Similar considerations occur in theoretical work. A chance association may trigger new insights, and open or reopen a line of research.

The rewards are generally higher when aiming for innovation, but the likelihood of a smooth flow is less. In many cases one may realise that the full importance of a piece of work can only be achieved if cognate projects are also undertaken. The issue then becomes whether it is appropriate to tackle them sequentially, with an extended timescale, or to enlist collaborators for a parallel effort.

At the outset, it can be hard to get a good grasp on how much work will be needed, and commonly this will be underestimated. This component is an important part of building up the full description of a project. Experience helps in all aspects of topic selection, but is not an infallible guide!

1.5.2 Building a topic description

Key question and mode of attack

A critical first step is a clear definition of the target of the research. This may involve testing a hypothesis based on theory or earlier experimental results, or, in more observationally oriented fields, building on conjectures to try to establish a hypothesis. The view of the scientific method developed by Karl
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Popper is that science only proceeds by ‘falsifying hypotheses’, and this is a commonly adopted viewpoint. Yet, such a representation of scientific endeavour provides little place for the creativity needed to even propose hypotheses.

When writing research proposals it may be necessary to frame the work in terms of hypothesis testing. Inevitably many promising ideas will prove wrong, yet many still have some degree of utility. Remember that, in normal conditions, Newton’s Laws provide an adequate description of physical behaviour, even though we know they are incomplete. Nevertheless the full panoply of general relativity is needed to get maximum accuracy from Global Navigation Satellite Systems such as GPS.

Once the research target is established, the style of approach to the research issues has to be worked out. Will the research attack the problem directly? Or is it sensible to make a more indirect approach, for example, by tackling a more general issue and then extracting a specific result? There is no general answer, the choice depends on the nature of the problem and the style of work required. A direct attack is often more suitable to laboratory-based experimental work. Indirect modes are common in the observational sciences, but also may be appropriate in a theoretical project where the problem is broken into a sequence of sub-problems.

A further important point to consider is whether the necessary expertise is available to carry out the project as envisaged. If not, can it be acquired using currently available resources or does it need to be brought in by engaging with collaborators?

Research landscape

Before developing a research topic too far, it is useful to understand where it fits in the general research landscape. Important questions are:

- How does the proposed work tie into current thinking?
- What are its dependencies?
- Who are natural collaborators?

There are many sources of information, including recent publications and preprints. Often meetings and conferences prove fertile ground for recognising promising lines of activity.

A good knowledge of the research landscape of your field will also allow you to recognise likely competition and their strengths and weaknesses. This enables you to see what different style can be brought to bear on the research target. The whole process can be informal, but warrants some attention.
1.5 Developing a research topic

Constraints and opportunities

It is important to be able to recognise any constraints that apply to a topic, and also any opportunities that it may provide. Progress is likely to be most effective if the topic can be broken up into stages of research effort of increasing levels of complexity. Thus we need to ask if it is possible to simplify the class of questions being addressed. If so, what is lost? Does the simplified problem contain the essence of the original.

Sometimes, particularly in theoretical work, it can be more effective to seek an approximate solution to a complex problem, rather than a more complete solution to a simplified problem. However, in the effort to isolate the influence of one aspect of a system in experimental work, it is possible to miss important trade-offs between different parameters.

Whatever form such simplification takes, it is important to understand the assumptions that underlie the approach, and the consequent limitations that they may impose. If your work will depend on methods developed by others, make sure that you are aware of any approximations or restrictions involved, and the conditions under which they are valid.

It is always dangerous to have strong dependence on ‘black box’ components in research, for example, in the use of software for data analysis. Sometimes this is inevitable, as when proprietary material is used, but it is important to understand what is going on, rather than just use it and hope all will be well.

General issues

It takes time to develop a suitable topic to the point where it represents a viable research project. Make sure that you allow sufficient time to digest the current state of the field. There is nothing more frustrating than finding that a research project has been pre-empted by other work. Of course, others may have the same types of ideas since science is a competitive endeavour, and ideas tend to have their season. Fortunately it is rare for projects to be exactly the same, and a good knowledge of the research background will help avoid duplication of effort.

In the process of selecting and developing research topics it can be helpful to seek advice relating to a research target. You will get the most value from such advice if you understand the concepts involved, and what they imply, as well as what is possible. This will mean that you can ask appropriate questions. The better prepared that you are, the better placed you will be to evaluate advice. Even good suggestions may not prove to be useful, since they may represent a different viewpoint on the problem than you have decided to take. It is easy to acquire too many opinions and be hard pressed to know which way to turn.
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Be realistic in your expectations, particularly about the time frame needed to make progress. Initial estimates tend to be optimistic, even when based on considerable experience.

**Exercise 1-2:**
Characterise your current research topic in terms of: key questions, potential interested parties, likely competitors, expected outcomes.

### 1.5.3 Research resources

The range of resources available to a researcher has substantially expanded from the traditional materials of printed journals and books. In particular, the electronic versions of journals now frequently include supplementary materials expanding the basic content, such as movies from simulations. Such extra information can often provide greater insight into the thinking behind the results presented in the paper itself. Once a research topic has been determined it is not difficult to arrange e-mail notifications from selected journals for relevant materials as they appear. Some care may be needed in the nature of the specifications employed, since it is possible to be overwhelmed by too thorough notifications.

For most topics there will be a modest number of journals in which the majority of useful articles will appear, but it has always been difficult to catch all relevant materials since some will be published in less obvious locations. In some fields there are long-established abstracting services that have carried a traditional mode of operation into the digital world. For example, the GeoRef database in Earth Sciences provides extensive historical information in an accessible form, yet also has a latency of only a month or two for new material. Such resources allow systematic searching using multiple criteria in combination, though it can take some practice to use the search procedures to maximum effect. In many areas of Physics, extensive use of preprints is made ahead of formal publication, and the arXiv facility is extensively used, both for storage and searching for new material.

A number of web resources provide access to searches of databases of scientific articles based on criteria such as the presence of a designated word in the title, the name of the author or some more complex composite criterion. The major resources of this type are:
- Web of Science (Thomson Reuters),
- Scopus (Elsevier),
- Google Scholar.

The commercial resources Web of Science, and Scopus require an institutional subscription, and provide sophisticated filtering by year of publication and
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general subject, together with information that can help to separate authors with similar names. Along with the details of the publications and potential modes of access, this class of resources provides information on the citation patterns of the publications both with respect to the papers they have cited, and to later papers that cite the publication. It can take some time before material is registered in these databases, particularly when they come from rival publishers.

Citation analysis is well developed for a wide range of scientific journals, and also some conference proceedings, but is more haphazard for books or articles in edited books. Google Scholar tends to catch more book-related material, but is not complete. It is advantageous to seek advice on the most effective ways of implementing searches in such web resources, since well-framed searches are the most likely to access relevant information.

In addition to the formal scholarly structures, useful information can often be extracted from more general web-based resources. It is important, however, to treat such information with caution since there is no guarantee of reliability or accuracy in material posted on the web. In many cases a search with a standard web search engine, such as Google, will return useful information, particularly if the combination of key words employed can narrow the possibilities. Nevertheless, you may find that something of interest lies well down in the precedence order created by the web search engine, so do not rely on the first page of results.

Wikipedia offers a remarkable range of articles on general and scientific topics, with the possibility of updates or corrections from users. Wikipedia is a multi-lingual resource, and entries on the same topic can vary considerably in nature and emphasis depending on the language in which the contribution is provided. I found Wikipedia helpful when thinking about the section of the book on project management since pointers were provided to material that I would otherwise have missed.

The websites of institutions and researchers can also provide useful information. Particularly at the individual researcher level, maintenance of sites can be somewhat lax, and so recent information may be missing. Information on research practices on websites will not normally have undergone any peer scrutiny, and so should not be relied on without independent corroboration.

The use of social media in science tends to focus on drawing attention to events or products from projects, but can provide a convenient means of keeping multiple participants in a large project informed. A recent development is ResearchGate, which provides access to researcher profiles and their publications, as well as hosting question-and-answer sessions across a broad range of topics. Searches can be carried out by name or
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keyword. As on a personal website, materials are user provided and so may need to be treated with caution. Such social products are likely to take on even greater significance in the future.

You are well advised to consult your local librarian for information on resources relevant to your particular interests. Frequently, they will be aware of materials that will not necessarily appear in simple searches. Networks of librarians are effective in sharing information about available resources.

Exercise 1-3:
Develop a resource list for your field of interest in terms of: journals, databases and other materials.