PART ONE
STARTING YOUR RESEARCH

ONE

GETTING STARTED: THEORY, RESEARCH QUESTION AND RESEARCH DESIGN

Chapter Contents

Aims

By the end of this chapter you will be able to:

- **Understand key elements in formulating a research question.**
- **Distinguish between deductive and inductive research designs, and be able to address the questions that social researchers need to ask when choosing which approach to adopt.**
- **Distinguish primary and secondary data, and be able to critically evaluate their relative advantages and disadvantages.**
- **Identify logical fallacies that recur in research and in everyday life, and which distort understanding.**
- **Comprehend the basic elements of sampling.**
- **Understand key elements of data validity.**

Getting into the water

This chapter, along with Chapters 2, 3 and 4, set out issues that all social researchers need to think about, decide upon and carry out in order to begin an empirical research project. In some respects this is like getting into the water when going swimming. There is an advantage to just jumping in: the best way to learn is to have a go. But just jumping in without having learnt some basic things might lead to drowning, so here we offer some basic guidelines on what to do. These chapters are about 'getting into the water' safely and with confidence. Doing social research requires that you do a large number of things, seemingly at the same time. This, at first, seems confusing. Those who have been doing research for a while tend to take it for granted, and so they are not always aware of every aspect of what they are doing when they do it. This can confuse the beginner even more. Getting into the loop is about picking up the taken-for-granted routines of the more experienced researcher, and practising them. Once you are familiar with the steps, processes and short-cuts you will no doubt develop your own style, your own routines and your own agenda. The way things are set out here is to help you get started. You will always have to bear them in mind, but after a while you may do things in your own way. This chapter will start with a discussion of how you can generate a research question. It will then discuss how to begin converting this research question into a research design, which involves decisions about testing or exploring; using primary or secondary data; causal or descriptive approaches; interviews, surveys, archival data or observations; issues of validity, reliability and generalization; and evaluation, participatory or action designs. It will finally address the question of writing a research proposal that will allow you to actually do your research. Box 1.1 outlines a few key distinctions that you will need to familiarize yourself with before you read the rest of this book.

BOX 1.1 HINTS AND TIPS

Setting out in the minefield of terminology

You will need to check the glossary of terms in the back of this book to start learning the meaning of the words that commonly litter this and any other book about social research methods. However, just to start you off, it is useful to remember that so much of social research hinges on distinctions between research that seeks numerical values and research that collects words. The former is called quantitative and the latter is called qualitative. Much hinges on this division as those that collect numbers need to carry out different kinds of data collection, such as a questionnaire, where a qualitative researcher would be more likely to use an interview to collect more open-ended verbal responses. Issues then arise over whether you want a large sample with short answers (such as can be gained from quick closed numerical questions) or a smaller sample of longer answers (such as can be gained from open-ended interviews). Do you want highly prestructured data collection or space to explore a topic? Will you try to measure how one thing influences another or only whether some things go together with others? These questions all go to the heart of how social research should be carried out, and choices should be informed by what you want to find out. Words like inductive, deductive, causal, exploratory, qualitative and quantitative, naturalistic, controlled and structured should all be looked up in the glossary if you are unfamiliar. Do this now or when the words come up in the text.

What is social research? Science, theory and data collection

Social research has many forms, and this book seeks to introduce the reader to their basic forms and logics. Part I sets out the key questions that the social researcher must address when starting a research project. In so doing, Part I will outline the basic properties of social research. While practical in nature, the first four chapters will highlight more abstract issues and debates, in particular the relationship between theory and the research process, both in conduct and in choice of method. These debates hinge around two questions:

- Is social research a science?
- Can humans be studied usefully in a scientific manner?

These two questions will be briefly addressed now, before moving on to the practical questions of how to (1) formulate a research question; (2) select a research design; and (3) present a research proposal.

Science is, in the popular imagination, the experimental method. The experiment is the stereotypical image of scientific method. Some social researchers use the experimental method. However, most social researchers do not! Experimental method is the establishment of controlled conditions in

which the effect of variables on other variables can be measured. Regulation of inputs allows accurate estimation of causes in the variation of outputs. Experimental method requires an initial prediction about how variation of inputs will affect outputs such that this prediction can then be tested. This prediction is a provisional theory (or thesis). This is called a hypothesis. A variable is anything whose amount can vary, and which is defined in such a way that its variation can be measured (and in an experiment also controlled in this variation). A number of variables may be specified. In the classic experiment (Box 1.2) all identifiable variables are held constant (controlled conditions), bar two variables. These are the independent and dependent variables set out in the prediction/hypothesis. The hypothesis predicts that variation in the independent variable causes variation in the dependent variable. With all other things held constant, the experiment is designed to allow this hypothesis to be tested. The hypothesis is drawn from prior examination of research on the subject. As such the experiment is theory driven (in other words, the data collection is designed to fulfil a need for the information required to answer a theoretical question). This approach to the relationship between theory and research is called deduction. Hence the experimental method is called hypothetico-deductive.

BOX 1.2 CONSIDER THIS

A classic experimental design

It is commonly suggested that students underperform in assessments due to lack of sleep before exams. They stay up all night revising. It would be possible to select a group of students with otherwise similar characteristics (age, gender and previous exam performance, for example). These students could all be given the same amount of preparation in the week before an exam. Then, on the night before the exam, all the students would be kept together in a controlled hall of residence. The group could be divided into subgroups. One group would be required to go to bed 10 hours before the exam, the next 9 hours, the next 8 hours etc., with the time not spent sleeping being given over to exam revision. Who would do best? Those who had 10 hours sleep and did no additional revision, those who had 8 hours sleep and 2 hours revision, those who had no sleep and 10 hours revision? Or would the best performance come from somewhere in between?

It is important to note that much research in the physical sciences is not strictly speaking experimental. Much of geology, astronomy and biology deploy methods of data collection beyond the laboratory. Geological and evolutionary time, galaxies and ecosystems cannot be replicated in controlled conditions. This is also the case for many aspects of social life (Box 1.3). Science cannot be defined exclusively in terms of the classical experiment.

BOX 1.3 CONSIDER THIS

The arrival of television: naturalistic experiments

Does television damage children, cause violent behaviour, encourage sexism, consumerism, political apathy, anorexia, body dissatisfaction and/or laziness? Even if those who watch violent TV are more likely to commit violent acts, it could just be that those with a liking for violence choose to watch such images, rather than being caused to be violent by such images. How can we tease out what is causing what? One way would be to compare a location where television is not watched with a location where it is. This may be hard to do now, as television is almost everywhere, but historically and geographically this has been done often. It may be possible to compare, for example, one prison where television is allowed with one where it is not. The ideal naturalistic experiment is where the same place can be studied before and after the introduction of the variable you think is going to make a difference (such as TV). Of course, it has to be assumed that the differences between places or in a place before and after the introduction of a particular variable are only differences in the variable itself, and not in other significant factors. The fact that a place gains television may be linked to increased income, which may impact on diet, and it might be the increased food intake that caused changes in body image, not television. Those seeking to carry out naturalistic experiments are always having to reflect on such possibilities.

However, despite not always using the experimental method, much of the remaining physical science research does deploy another form of hypothetico-deductive research (that is, research where a prediction is tested through the variation of observed conditions). This may be through comparison over time or between locations. If different levels of a particular variable exist in different locations or at different times, it may be possible to measure the levels of other variables in those times and places to see if patterns (or correlations) exist. Whilst an element of control is lost, it may still be possible to show that variations in one factor go along with variation in other factors (even if what is causing what is harder to pin down). A hypothesis can be stated. The researcher can then go looking for the conditions necessary to test that hypothesis. Data can be collected and results analysed which will then support or challenge the hypothesis. This is still therefore hypothetico-deductive research.

A far greater amount of social research adopts this approach. Proponents of this type of social research tend to see themselves as scientists. However, some forms of social research are not hypothetico-deductive and pursue an exploration-based approach. Sometimes this is to identify what is going on when existing knowledge is insufficient to generate hypotheses. There are plenty of such examples in the physical sciences. Sometimes, this exploratory approach is adopted as a rejection of the hypothetico-deductive method, its predictive process and causal assumptions. This raises the question of whether scientific methods are appropriate to study humans, or whether humans possess qualitatively different characteristics to

physical objects (most particularly consciousness and choice) that invalidate predictive forms of research and the predictive model of explanation which hypothetico-deductive research is based upon. These issues will be discussed later (see section 'Testing or exploring' later in this chapter, and see Chapter 4). These are questions that will recur throughout the research process, but which cannot be resolved at this stage.

Identifying a research question

While a hypothesis is a proposition to be tested, rather than a question to be answered, hypotheses are designed to focus attention within broader research areas or questions. While some research questions are very specific and others far looser in definition, the process of identifying a research question is always an essential first step in any project.

Social problems, political issues, personal motives?

A researcher may enter the process of identifying the research question at a number of different stages. Ironically, the student conducting a research project for their studies and the well-established research professor may have more in common here with each other than either may have with the majority of researchers in the middle. The privilege of starting from first principles, rather than being brought in at the middle or towards the end of the research problem identification process, is most often denied those neither well established nor researching for study.

Identification of the research question may have many levels, only some of which will be within the researcher's power to alter, at least in the first instance. Issues may become 'ripe' for research in the minds of those able to fund such activities for a number of reasons. Bodies engaged in education, health care, law and order, social work, economics, urban planning, commercial and governmental administration and so on will, for various reasons, come to the view that research may help them address or more clearly identify problems. Research may be funded by charities on issues of concern to them so that findings stimulate awareness and debate about those issues. These bodies will form an opinion about what needs to be researched and such motivations play a crucial role in the identification of research questions.

However, even while such factors play a crucial role in directing research, the question of how such research is to be carried out requires the researcher to develop the identification process from an idea to a practical activity. It is here that the researcher's own interpretations of the 'problem', and the best way to research it, come into play. When the researcher can claim a degree of expertise both in the subject to be studied and in the methods by which such a subject can best be studied, they are in a position to introduce their own definitions of themes and

interpretations of 'problems'. To this extent the more developed researcher may become proactive in regard to seeking funding from potentially interested parties for projects the researcher is personally motivated by. In this way the researcher may move 'upstream' in the question identification process. When a researcher's own previous research comes to define how potential research funding bodies perceive an issue, they may consider themselves to have become the source of the stream itself. Most researchers are not in such a fortunate situation, but, as Tim May (1997: 27) points out, the relationship between theories of what problems exist and methods applied to investigate their existence is always a two-way street, even if the density of traffic in each direction varies.

Whether the researcher is contracted to research a particular topic, doing a project within or on behalf of an organization, bidding for funding from a public or private agency, or conducting research as a training exercise within an educational context, the first step in identifying the research question in a practical fashion is to find out what has gone before (see Chapter 3 on Literature Searching and Reviewing. Those interested in the intricacies of gaining funding, and in the politics of such processes, will find many useful discussions (for example, Hammersley, 1995; 2000). Here we will move on to the issue of generating a research question. We will then look at the two types of research question: hypothesis testing and exploratory investigation.

Sources of a research question

Sociologists often refer to the title of C. Wright Mills's (1959) book *The Sociological Imagination* as though this referred to some particular source of creativity that being a sociologist somehow confers upon them. For Mills such an imagination was the combination of two things. First, it was necessary always to ask how what appeared to be personal problems might be better understood as social issues. Second, there arose the application of the researcher's craft required to investigate such suggestions. Mills was famous for collecting newspaper cuttings every day from which he sought to identify contradictions in everyday representations of social life – contradictions that might best be overcome through social research.

The media then may generate issues that can be turned into research questions, but personal experience may do too. Prior research, theory and literature around a topic may generate a question in themselves or in the clash with media representations or personal experience. Newly available access to sources of data (whether these be secondary sources, archives, groups or locations) may make certain questions that were previously hard to answer newly attractive. Just as access opens up questions, so the generation of research questions is bounded by the limits of what data can feasibly be collected. Legal decisions and new policy initiatives may also throw up new questions. In a similar vein, policy and law makers may want research into areas they define as social problems, or to evaluate the consequences and/or effectiveness of their proposals and/or actions. The advent of relatively portable audio and then video recording devices have historically changed

the character of research practice, and even opened up new research questions based on new data collection possibilities. The development of personal computing and the internet represents another such transformation in data collection, but also reflects a significant potential change in the character of social reality, not just its 'researchability'. Social change of course represents another key generator of research questions, as do comparisons between different locations.

As such, whilst the origin of a research question may be from many sources and the combination of many different elements, constraints of feasibility and relevance also shape the question formation process. Feasibility refers to access, ethics, time and other resources that the research has to take into consideration. Relevance refers to a combination of factors too. Is the research significant, either in policy terms or in understanding important social issues? Does the research question have any relevance to existing research and literature in the field? Will the question maintain the researcher's own interest? Policy or social relevance, theoretical precedence and personal passion about a topic are essential to getting research off the ground and sustaining it, even while all these elements generate scope for bias that needs to be reflected upon in the process of research design, data collection and analysis.

Talking to people is another key source for generating a research question. This may be informal discussion with colleagues, tutors, family or friends; you may want to organize a discussion group, a focus group or a Delphi group (a focus group of experts in a chosen field); or you may want to set up a discussion board online. Academics and other professional groups routinely attend conferences, often more for the informal 'chatting' opportunities these events provide than for the formal papers they could just as well read at home. Robson (2002: 49, 57) makes the useful suggestions to start where you are but to trawl cognate fields to see if other people do things differently. You might be a geographer, but perhaps the psychologists have something you had not already thought of.

What makes a good research question?

Nicola Green (2008: 47–9) suggests the key to a good research question is that it is 'researchable', and proposes six elements. A good research question will be: (1) interesting; (2) relevant; (3) feasible; (4) ethical; (5) concise; and (6) answerable. Interest to the researcher sustains research practice through the hard times, while relevance to the wider society or to the academic and/or policy community is necessary to maintain funding and esteem. Feasibility in terms of time, topic, place, costs, skills, access and information is crucial. Maintaining ethical standards regarding topic, access, and respect for participants in the collection, analysis and use of data, and in relation to the researcher's own wellbeing, is also essential if research is to be successful. Research must be concise, that is well articulated, conceptually clear, theoretically framed, and able to translate abstract ideas into empirically measurable categories about which data can be collected. Finally, a good research question should be posed in such a way that it would be possible to know what it

would take to answer it. Green (2008) notes the importance of mapping the interrogatives – the who, what, when, where, how and why questions. The first four of these are descriptive, the fifth maps process, whilst the sixth refers to causation. As will be seen below, refining the research question allows the researcher to identify whether they are primarily concerned with description, process or causation.

The good research question is always a balancing act. You need to be relevant to what has gone before, but at the same time show that the research you want to carry out will add something new. You need to be concise and yet you do not want to be so pre-emptive as to close down the very originality that new empirical data collection might bring. There is a temptation to be conservative (with a small c) in wanting to ensure that the research is doable, but at the same time there is a temptation to be radical (with a small r) in wanting to do what has never been done before. It is easier to describe, but perhaps more interesting to explain why something is the case. As such, the glory of novelty and the shame/difficulty of biting off more than you can chew should inform your decision as to how you develop your research question.

In reverse fashion it should be pointed out that some things make for a bad research question, and these boil down largely to 'letting the tail wag the dog'. It is not enough simply to do a particular research project because you can. If you formulate your research question simply because you have a particular access, or because you are particularly good or experienced with a certain data collection and/or analysis technique, then the research is likely to be limited and unoriginal. Despite earlier suggestions that research should be doable, it is not enough to just do what is easiest to do. 'Do-able' is a necessary condition, but it is not a sufficient condition, and it should be a criterion for evaluating a research question, not the principle on which a question is initially selected. Questions of principle, concerning of relevance to social problems, policy and academic development, come first. Practical questions of feasibility (time, money, access, skills) should come second, but are still important.

The value of a good research question

For the same reason that a good research question maps the interrogatives (the what, when, where, who, how and why questions), the value of so doing lies in defining whether your research will explore, describe, explain or even challenge the object of its attention. Exploration seeks to find out what is going on in a situation in the absence of any prior account. Exploration involves description, but exploration involves not even knowing in advance the full range of what it is you will seek to describe. Description seeks to capture the what, where, when and who of a situation, often in the absence of any prior or sufficient explanation of what is going on. Explanation requires a descriptive mapping of the situation, but involves the addition of seeking to explain relations between the phenomena being described, in particular the possibility that certain features of a situation cause others. Critical researchers may go one step further in seeking to suggest that

research can identify the causes of problems and encourage improvements. The value of a good research question in the first place lies in helping clarify what your research is seeking to achieve.

The establishment of a research question will act as something of an anchor during the course of subsequent stages in the research process. Your research question will guide the search for prior literature (see Chapter 3) and inform the way you filter and review such work found. The research question, perhaps reframed in the light of your literature review, will then determine the research design you select, which itself determines your data collection, analysis and interpretation. Each step along the way will involve reflection, and may involve modifications, but a well-formulated research question should guide you from beginning to end. It is too easy to be moved off along various tangents. Difficulties in data collection may lead to certain data being easier to collect or certain groups easier to collect from. Without a robust research question to guide the research process it is easy to be swayed by such conveniences and complications. Your research question should act as a guiding star, something to navigate by. In this regard it is not something that should change at every twist and turn.

Refining your research question: from research question to research design

Keith Punch (2005: 33) usefully distinguishes between areas, topics and questions. A research area is very broad (such as 'class', 'work' or 'family'). A research topic will be narrower (such as 'social mobility within a class structure', 'the relationship between skill and reward at work', or 'divorce'). Research questions operate at an even more specific level. What is it about mobility, skill or divorce that you want to find out about? Punch observes that a general research question may define the key relationships and issues you want to investigate, whilst specific research questions and actual data collection questions involve a further level of focus and detail. This is the transition from research questions to research designs.

Nicola Green (2008: 50–9) suggests a four-step movement from general interest to something narrow enough to have moved from a research question to being the basis for a research design. Whilst step one involves going as wide as possible in terms of sources of ideas and discussions/brainstorming to develop these, step two involves narrowing the list, noting recurrent themes and less common ones, themes that seem to go together and those that seem at odds, core elements and less significant ones, clusters and nested themes that can be merged together, those that are answerable and those that are too abstract or ephemeral. The third step involves drawing out the character of the question you want to ask (descriptive or causal etc.) and, from this, addressing the question of what information (data) would be needed to answer such a question. Step four, a review, involves asking whether the revised research question meets the six criteria for a good research question set out earlier. If so, the research question should naturally flow into providing the basis for a research design, a practical strategy for collecting data.

Turning a research question into a research design is sometimes referred to as 'operationalization'. Sometimes this term is used to refer more narrowly to the process of taking theoretically informed concepts and turning them into empirically recordable objects. The concept of class for example is elusive, and to measure its relationship to health for example would require that each individual be assigned a 'class' value based on a robust scale that could be clearly measured by collecting particular pieces of information. Operationalization in its narrow sense refers to the move from ideas into the realm of empirical measurement. In deductive forms of quantitative research a variable such as class would require definition in advance to allowed individual cases to be measured along a scale of values within the variable. Essentially, in such research concepts have to be translated into variables that are both internally homogeneous and externally discrete. Internal content should be sufficiently 'the same', and 'the same' things should not be able to fit into two values of a variable. How and where the boundaries are to be drawn is therefore fundamental. If you can specify these in advance of collecting any data, you can reasonably conduct a quantitative and deductive research design. If the content of meaningful categories and the boundaries between them cannot be clearly and confidently asserted in advance it is better to adopt a more inductive and qualitative exploration of the research question, whether in the form of a pilot exercise in advance of a more deductive project, or as a stand-alone piece of qualitative research. Qualitative forms of inductive research seek to operationalize concepts only in the process of data collection. It is in the act of exploring that provisional categories are fleshed out into substantive classifications of the field. These forms of operationalization will be discussed in greater detail in Part II.

Testing or exploring?

Researching the existing literature (see Chapter 3) will give you some sense of what has been said before, what the key findings and key disputes are, and perhaps will have left you with a sense of what is missing or still needs further investigation or clarification. Similarly, whether the original motivation for your research was personal, moral, political or intellectual fascination, or the interest of the organization funding the research, this will have given some focus and direction to the research, even if only to establish some of the initial keywords used in your literature search. So, your research will have some degree of focus already, but the degree and nature of that focus must now be clarified further.

At this stage you will need to ask yourself the following questions:

- Do I have a hunch (in other words a hypothesis) about what is going on here?
- Does that hunch/hypothesis suggest to me what the key causes and effects are?

You do not need to be sure. If you knew for sure that increasing amounts of *X* led to increasing amounts of *Y*, it would not be necessary to research it. The purpose

of at least one type of research (hypothetico-deductive research) is to test hunches. Whether the hunches are supported in the final research or not, we have a result. Research that is based upon the idea of testing hunches is to be distinguished from research where we are setting out only to explore what is present in a particular situation.

Testing a hunch requires that we can state it in such a way that it can be compared with reality. This formulation of the hunch is then a prediction. This is not the same as a question. A question is open ended, while a prediction states an expected outcome. What is open ended, in the case of a prediction, is whether this expected outcome conforms with the actual outcome. Will the prediction be correct? A hunch is a theory that has not yet been supported with evidence. In research terms this is called a hypothesis. What distinguishes a hypothesis from other kinds of ideas is that a hypothesis is designed to be tested, and so must state clearly the elements involved (measurable categories of actions, objects or actors) and the nature of the relationship between them that is being predicted (cause, mediation or correlation). These practical matters will be dealt with in greater detail in Chapter 4. Here it is only necessary to be aware of the distinction between testing and exploring, and the logic behind choosing either one or a combination of the two. So what is exploratory research and why choose not to test a hypothesis?

On completing a review of the literature you may feel that there is a reasonable case for suggesting that *X* has a relationship with *Y*, and even that the relationship is a causal one. This may not have been actively tested in the previous research you looked at, or such testing may have been long ago or in a different location, thereby warranting your wish to carry out such tests. Alternatively, you may feel that the literature does not leave you with a hypothesis that can be tested, only a series of open questions about what is going on. If this is the case, it is not going to be possible to draw up a testable hypothesis. You have no tentative predictions, only questions. In this instance you will want to adopt an exploratory approach. Without a prediction to test, the design of exploratory research will be more open ended. Because of this, exploratory research tends to collect more qualitative (interpretive) data, though this is not always true. Testing a hypothesis is more often associated with the use of quantitative (numerical) methods. Chapter 4 examines the qualitative/quantitative distinction in more depth.

The key to hypothesis testing is the belief that the existing literature is a reasonable source of predictions. Exploratory research designs tend to occur when predictions cannot be gleaned from the literature. However, some argue that it is not just a question of 'if and when' the literature cannot generate reasonable predictions, but a question of principle, and that theory should not determine the structure of research. In such a rigid way as is required for hypothesis testing. Such researchers argue that theory should be built up from exploration of reality, not used to predict it in advance. This is an inductive (as opposed to a deductive) approach to theory building and research. Here it is enough to say that all good research combines elements of prediction and exploration even without using the terminology. In using a literature review, all researchers to some degree are guided

in their work by predictions of what is useful to research, where and how to look and what to look out for. Even the most 'inductive' researcher cannot avoid this. Yet at the same time the use of some form of exploratory research is standard practice in even the most rigid hypothesis testing research. The pilot study, where researchers seek to explore the extent to which those they seek to research support the hypotheses, may take many forms. Some are more open than others, but all are forms of preliminary exploration (see Chapter 4). For now, suffice to say, while differences are great, they are not always as great as might first appear. So then the question for the researcher is: should I generate a hypothesis or adopt a more exploratory approach? In part this will depend on what you have found in your review of the literature, but it will also depend on your stance concerning the nature of human action – causation or choice (see sections 'Causes, meanings and probabilities?' later in this chapter; and 'The deeper divide' in Chapter 4).

Primary and secondary sources

The process of social research outlined in this text focuses predominantly on the designing, processing and analysing of data collected by the researcher, known as primary research. Depending on the area of research and the research question, it may be appropriate to consider using and searching for existing data. These data can then be examined and analysed, a technique called secondary analysis.

It is worth taking a few lines to discuss what exactly is defined by secondary data analysis. Compared to primary research, much less has been written on secondary analysis. Hakim provides the traditional definition of secondary analysis as 'any further analysis of an existing data set which presents interpretations, conclusions or knowledge additional to, or different from, those presented in the first report on the inquiry as a whole and its main results' (1982: 1). Dale et al. (1988) suggest that secondary analysis is a broader term that simply entails data being analysed by someone else other than the original researcher. The most famous piece of secondary data analysis in the history of sociology is Emile Durkheim's (1952) use of suicide statistics from various regions of Germany and France in the mid nineteenth century to highlight that patterns were consistent over time, but varied across location, such that certain social facts clearly increased or decreased the incidence of self-inflicted death. In the UK, secondary analysis emerged during the 1960s and 1970s as a product of the large surveys undertaken by government departments and agencies. These developments were paralleled in other industrial societies across the world. Surveys such as the General Household Survey, Family Expenditure Survey, British Crime Survey and British Social Attitudes Survey and the 10-yearly Census were conducted by government to inform economic and social policy. The UK government did, of course, collect data on the population before this date. The first Census was in 1801. Since the 1960s, the number of surveys and the coverage of the surveys have broadened considerably and the availability of data for secondary analysis has been improved through the development

of websites detailing the original survey and data, for example, the Economic and Social Research Council (ESRC) Data Archive at Essex University (www.data-archive.ac.uk).

The decision to undertake a primary or secondary research design should be determined by the theoretical and conceptual nature of the research question. Beyond this, secondary analysis can often be restricted by the availability and quality of existing data. Given the historical nature of secondary analysis, with its roots in government surveys, the majority of data available are quantitative, numerical data, derived from questionnaire and structured interview-based surveys. Ongoing initiatives are being undertaken to redress the balance through funding for a qualitative data archive (www.qualidata.ac.uk) which aims to collect interview transcripts, diaries, participant observation notes and so on.

Causes, meanings and probabilities? Logic, relationships and people

A naïve or simple conception of causation suggests that when *X* is said to cause *Y*, what is meant is that *X* makes *Y* happen. This implies a mechanism at work, and this idea of mechanisms is not accepted by many in the social sciences who suggest human action is either too complex or too qualitatively distinct from physical events which seem more easily reducible to mechanistic accounts. Are these objections legitimate? Statements like '*X* causes *Y*' seem to suggest either that every instance of *Y* is the result of a prior instance of *X*, or that every instance of *X* will result in the production of an instance of *Y*. The first is a logical fallacy. The second is false on the grounds that no singular action is ever 'sufficient' to explain an outcome. First, it is logically incorrect to say that because *X* causes *Y*, all *Y*s must result from *X*s. Exams cause stress, but not all stress is caused by exams! Second, in conditions of complexity (reality) it is incorrect to assume that because *X* causes *Y*, all instances of *X*s will lead to *Y*s. Exams cause stress, but not all exams are experienced as stressful because intervening factors can influence the outcome in some cases.

As mentioned above, the first example is an instance of a logical fallacy: the fallacy of 'reversal'. Just because something may cause another thing to happen does not mean it is the only possible cause. Other logical fallacies are those of 'composition' (that is, if one woman can become prime minister then all women can, or if one person is bad then all in that group must be bad), and 'association' (that is, if storks nest before babies arrive, storks must cause babies to arrive). Logical errors of this sort characterize much of everyday consciousness and political rhetoric as well. Social researchers are not immune, so care must be taken to avoid such logical pitfalls when posing a hypothesis or deciding whether to pursue a causal hypothesis. For further discussion of logical fallacies, see Sayer (1992).

The second example (where a causal agent does not lead to the same effect every time) is a manifestation of complexity, and raises the issue of necessary and sufficient conditions of causation. Sometimes when a light switch is flicked the

light comes on. Sometimes it does not. It is not enough to say that flicking the switch causes the light to come on, although it is a part of the causal process at work. There are other links in the chain, and if any of these are out of place the sequence is not completed and the effect does not happen. Necessary conditions are those that are required for an event to occur, but no single one of them is sufficient on its own. As such, causation in conditions of complexity never operates by means of single links where *X* will always cause *Y*. The weather is a complex set of interacting systems and subsystems. Because of the extent of its complexity it is not possible to predict with absolute certainty what certain conditions will lead to. Causation is too complex to map outcomes with absolute certainty. Within such complex systems prediction is not always possible, even where a fairly clear idea of the causal factors and mechanisms has been developed. Open systems defy absolute prediction, but this is not because they are beyond causation.

Tendencies are one way of describing the existence of forms of causal association that are never absolute because of the complex interaction of many necessary conditions. There is a tendency for class background to affect educational performance, but this is never absolute, as there are many factors in an individual's life that may alter their chances, even if these factors are largely stacked in favour of those from more affluent backgrounds. Intervening factors are often called mediations. Tendencies can be expressed in the form of probabilities rather than in terms of absolute causation. Modern statistical techniques were largely developed to aid researchers in the human sciences deal with the fact that complexity never allows for singular causal agents to have 100 per cent outcomes.

So far, then, the objection to simplistic (*X* makes *Y* happen) causal explanations of human action can be accepted on the grounds that reference to mechanisms may imply too simplistic a set of causal processes than are in fact at work. However, if we avoid logical fallacies of causation and recognize complexity, while limiting the scope of prediction and prohibiting the use of simple monocausal models, these objections are defused. Are there other grounds for resisting causal explanations in social research?

One suggestion is that human action is intentional, and that intentions are future oriented. Can a future state that motivates a present action be called a cause? As causes must come before effects, the future cannot cause the present, and so, it is argued, intentional action is best not understood in causal terms. This is a logical error, as it is not the future that causes an intentional action, but the intention itself, which can be firmly located prior to the intentional act. Another suggestion is that causes refer to external forces acting upon an object. Human actions emerge from the workings of inner states. It is suggested that it is meaningless to suggest that something caused itself. Could this argument be applied to a video-recorder? Having a complex inner mechanism, a video-recorder acts upon itself. Causal mechanisms operate inside the box. A third suggestion is that beliefs and meanings are linguistic entities rather than physical ones. Whilst language may have rules, structures and even devices and mechanisms, these are not the same as physical rules and mechanisms. As such, using the term 'causation' to describe the influence of an idea or the strength of a belief may be misleading. Certainly, using

the kinds of mechanism appropriate to physics to explain language would be unduly reductionistic. Many biologists would say the mechanisms in physics are not sufficient to explain biological phenomena. It may be that language is simply another level of causation that has its own set of mechanisms. Perhaps it is fundamentally distinct? It is not really important to decide here whether language, human consciousness and intentionality really transcend causal logic and explanation. Language and conscious intentionality can be seen either as mediations in the causal process or as something distinct from causal mechanics. Either way, language and conscious intentionality play a part in the outcome of social affairs, even if the extent to which this is the case is open for dispute. Whether you reject causation as key to understanding social life or accept it, there will always be a role for asking people what they think is going on, even while it may well be the case that other important processes operate 'behind their backs' as it were. A false belief as much as a true belief, and a caused belief (if such a 'thing' exists) as much as a freely chosen belief (if such a 'thing' exists), have implications for the behaviour of the believer and their social world.

Data: asking, looking, reading and recording

What are data? While there is a great deal going on beyond that which researchers record, what goes on 'out there' is not data. Data are not what is out there to collect. Data are what is actually recorded by the researcher. As such, data are not naturally occurring 'stuff'; they are in a very important respect what researchers manufacture in their work as researchers. Why make this distinction? Well, fundamentally it is to remind you that what the researcher records is not reality itself but a 'reflection' of that reality, shaped by the tools they use to generate and record it. This is important to remember. It is nice to imagine that the camera or the human eye gives a 'picture' of the world that never lies. This is not true. The camera must be pointed in one direction rather than another. The human eye (and the sensory system of which it is a part) is selective. How the researcher chooses to direct and select will shape the data they collect. How they choose to record what they collect will involve classification, and this classification also shapes the data that are collected. How they choose to sample will affect what it is they collect. How they choose to frame their questions or structure their observations will influence the form and content of their data. In this respect, data are a product of research and not something that researchers simply collect. Data are the output of research, not the input. Research is in many respects therefore a kind of manufacture, and requires all kinds of tools and apparatus. This may also be called a form of technology. These tools may be physical objects (such as cameras, tape-recorders and computers, or in the case of the physical sciences, microscopes and spectrometers). Tools refer also to forms of structured interactions, such as the interview or the observation. The survey questionnaire and the experiment are tools that fuse both physical and social elements (a carefully structured text on paper or a controlled laboratory). All of these tools (or technologies) act to stimulate and filter events and actions so as to generate materials that can then be recorded as data. Even the most

naturalistic forms of research (such as an ethnographic field trip where the researcher lives with a community to observe their everyday lives) involve complex designs and tools (choices over where to visit, how to live, how to ask questions and how to record findings). There is no such thing as the totally unstructured interview or observation, even if some forms of research adopt far less pre-emptive structuring than others.

As will be discussed in the following chapters, all forms of social research involve a lot of planning. All data collection requires the development of tools and technologies of both a physical and a social kind. While social research can be divided in terms of the type of data collected and the degree and form of structure imposed in the collection and recording of those data, all data are manufactured. The types of data are observational based, question asking, and the collection of 'textual' materials (these materials may be diaries, letters, photographs or receipts and so on). The degree of structure refers to the deductive and the inductive forms. This allows the generation of six ideal-typical forms of primary data collection (research designs):

1 deductive observation: such as the experiment
2 inductive observation: such as the ethnographic study
3 deductive questioning: such as the survey questionnaire
4 inductive questioning: such as the in-depth interview
5 deductive textual study: such as quantitative newspaper content analysis
6 inductive textual study: such as qualitative content analysis or discourse analysis.

Research projects may adopt a combination of methods to achieve specific ends. This is often called triangulation. Observation records what people are doing at the point of observation. Interviews and questionnaires record what people say or write at the point of response. These two things are different. Your choice of method needs to reflect whether you are more interested in action or talk, or your best judgement as to what method will best give insight into an issue. It should always be borne in mind that what people do and what they say they do are not always the same thing. Similarly, what people say and do and what people say and do when they are being observed are not always the same things.

What, and how much, is good enough? Validity, reliability and generalizability

Spending a large amount of time observing or interviewing a small number of people offers greater opportunity to know them better. Spending less time with each person or group, and so allowing the research to involve a larger number of people, offers greater opportunity to claim that what one finds is not idiosyncratic. This tension cannot be washed away with a single formula. What is to be done? The tension is often described as one between validity and generalizability (or between internal and external validity).

Validity refers to the closeness of fit between data and reality. Are your data really showing what is 'out there'? Validity can be divided into two parts. The first part refers to the fit with those you actually studied. Do your data actually express the reality of their lives and beliefs? This is what is called internal validity. The second part refers to the fit with the wider world. Do your data really show the reality of the wider population from which your sample was selected? This is external validity (sometimes called generalizability). Population does not refer to everybody. Population refers to everybody in the group you claim to be researching. If you claim to be studying the French, then your population is everyone who is French. We do not need to worry too much about this here, but defining such a group, or any group, is not a straightforward exercise. If you claim to be researching the homeless in Plymouth, your population is every homeless person in Plymouth. What counts as homeless and Plymouth requires interpretations that can be practically measured and defended as accurate. This is not always easy, especially when the group researched is not readily identified. Criminals and racists are not always forthcoming to be recorded, so these populations are largely hidden. (See section 'Reliability and validity' in Chapter 15 for more detail about types of validity.)

In-depth interviewing and long-term observation allow for greater internal validity (though they do not ensure it). Inductive approaches may also allow greater depth of understanding as the researcher is freer to allow the researched to dictate the direction of the research. However, the downside to this is that time spent focused on a small group limits scope for a greater number to be included. This may lead to a loss of external validity. In addition, inductive forms of research that do not impose a strict order on interviews and observations generate problems of reliability. If each interview is different, each interviewee may have greater scope to develop their own interpretation of reality, but it becomes harder to compare one interview with the next. A structured observation or interview/questionnaire allows clearer comparison. Deductive researchers tend to emphasize the value of reliability (or uniformity) in generating comparable results. They also place greater emphasis on the need to gain a sufficient number of respondents to allow reasonable claims about the whole population concerned. Both these concerns hinge around an emphasis on external validity. Inductive forms of research tend to emphasize internal validity. In so far as inductive research is less concerned with testing a hypothesis than it is with exploring a field, it is less concerned with making generalizable claims.

Gaining external validity is not just about getting as large a number of respondents as possible. A well-chosen but relatively small sample is far more useful than a larger but badly chosen group of respondents. A census, where every member of a population is researched, may sound ideal, but it is rare to have the opportunity, and as rare to have the time to analyse all the data that would be generated. So what counts as a well-chosen sample? A well-chosen sample seeks to mirror the population the researcher is interested in. The first question here is whether it is possible to say who the members of a population are. It is far easier to say who the prison population is than it is to say who the criminal population is. Even if we could define what a criminal is (do you count all those who have ever broken

a law?), they are not a group who openly advertise their identity. The most valid sampling method is called the random sample. This requires that the whole population have an equal chance of being chosen, and this requires that we can identify them all. A sampling frame is a list (or even a hat) containing the names of the whole population from which a sample can then be drawn in such a fashion that all have an equal chance of being chosen. This is the meaning of 'random' in a random sample. Random in this context does not mean stopping the first person you meet on the street. A school register is an ideal sampling frame if your population is all the children at that school. Other such lists exist for other populations. But many populations do not have such records, or where they do exist you may not always be allowed access, and in such cases random sampling (strictly speaking) is not possible. Researchers have devised numerous approximations of the random sample to deal with different situations, and these will be discussed in more detail as the book develops (see Chapter 14 for a full account of sampling methods). Here it is only necessary to mention the extreme opposite of the random sample. This is the snowball sample. Where a population is hidden and not much is known about who is and who is not a member, it may be suggested that exploratory/inductive methods be best used. The snowball sample is highly inductive. Where no sampling frame exists and so where a more prestructured selection of sample members cannot be achieved, the researcher may use their first respondent's personal networks as a means of gaining access to other members of the population. This raises many serious questions about external validity, but in an exploratory research project it may be the only way to generate a sample.

Finally, how big does a sample need to be in order to be a good sample? As was said above, size is less significant than good selection methods, but having enough respondents to fulfil the purposes you require is still essential. This will be discussed in more detail in Chapters 5 and 14.

Evaluation, participation and action research

Evaluation research seeks to measure performance. In social research this will usually involve the evaluation of an organizational strategy or the delivery of a service. Performance may be measured in terms of objective indicators (increased sales, declining absenteeism or the reduction of crime in an area) or in terms of more subjective perceptions (customer satisfaction, employee contentment or perceptions of safety). Evaluation research is more interested in practical objectives than in purely theoretical motives, but of course it is the researcher's job to design the best method, and this will involve consideration of past research and theory in the area being researched. As such, evaluation research follows the same processes as other forms of research. In so far as evaluation research tends to start with a clear sense of what is of interest, it will tend to be more deductive in nature. However, especially with regard to the more subjective indicators of performance (which may be less easy to establish in advance), more inductive and exploratory forms of research may be adopted to investigate perceptions and experiences. For more

detailed discussions of evaluation-based research, see Rossi et al. (1999), Pawson and Tilley (1997) and/or Clarke and Dawson (1999).

Participatory research takes two basic forms, though a combination of these two creates a third. In the first, the researcher seeks to participate in the everyday practices of those researched in order to gain a better understanding of their life and experience. Such observation by participation is generally led by the routines and practices of those researched. It therefore tends to be inductive, but more deductive forms of participant observation can be used. Participant observation is an extension of the classic ethnographic method of non-participant observation. However, a researcher may take up the role of participant observer with a pre-structured set of questions they want answered, but which they feel can best be investigated by means of observation in natural settings rather than via questionnaires or surveys. Participant observation may be overt, covert or partially covert. It can be claimed (on 'consequentialist' ethical grounds: see section 'Sensitivity in the conduct of research' in Chapter 2) that not telling those being observed that their fellow participant is a researcher may be justified. This may, in certain situations, be true. However, as a first-time researcher it is not advisable to choose a topic (such as researching the cultural practices of international gunrunners) where revealing your identity as a social researcher may undermine the validity of the research (and the viability of your health).

The second form of participatory research involves the recruitment of those researched in the conduct and even the construction and evaluation of the research. Involving participants in this way may allow insights not available at the outset, and is a logical extension of inductive principles. Nevertheless, just as inductive methods can sometimes be used at the start of a research project to get a sense of the field prior to the development of a more deductive design, so the initial involvement of participants in developing the research agenda can give way to more deductive forms of participatory research. Such research is almost always overt.

A combination of these two strategies may be adopted. Here the researcher participates in the routines of the researched, and the researched participate in the routines of the researcher.

Action research is an extension of evaluation research. Action research is designed to facilitate the development of the goals of an organization rather than simply to measure the level of success in achieving such goals. Such a form of research presumes both that the goals of the organization are clear and that they are goals the researcher feels are appropriate for them to become involved in promoting. Where funding is involved this may lead to pressure on researchers to accept goals as defined by those in the organization in a position to offer the funds. It should be remembered that organizations are not homogeneous and those at the bottom may not see things in quite the same way as those at the top (David, 2002).

One particular brand of research is participant action research. This is the combination of action research with a form of participation (Whyte, 1991a; 1991b). The researcher seeks to facilitate the goals of those they are researching. This is the meaning of the term 'action research'. The researcher also seeks to participate with those being researched and to recruit the researched into the process of research

design and conduct. This is the meaning of participatory research. Participant action research is a form of advocacy research and assumes the legitimacy of the standpoint of those being researched. If the researcher aims to facilitate the goals of those researched, there must be a presumption that these goals are legitimate. This fusion of research and advocacy parallels many debates within feminist research over the most appropriate research methods to take forward feminist intellectual and political goals.

Writing a research proposal

For various reasons students, professionals of various kinds, academics and others will find themselves wanting or needing to apply for either permission or resources or both for the conduct of a research project of their own. This is the business of writing and submitting a research proposal. Approval may be needed to access certain groups and locations, or to work with various organizations. Whether you are working for an organization or seeking to work within an organization, or aiming to gain funding or other support and approval from particular organizations (such as government departments, businesses, universities, hospitals, schools and charities), these organizations will have particular rules and procedures that you will need to work within. Your research proposal will often involve an up-front demonstration of your awareness of such rules and of your willingness to work within them.

Your research proposal is the way to convince others of the validity and value of your suggested project, but it is also an important means of bringing together these two elements in your own mind. The question of validity relates to the 'truth' content of what you seek to undertake. Does the research question translate into a design that will provide the data capable of answering it? The value question relates to the usefulness of such research, whether in advancing pure knowledge or in developing some kind of policy/practical problem solving application, or both. The questions of 'What?' (what to study), 'How?' (how it will be studied), and 'Why?' (for what purpose it will be studied) come together in the research proposal, just as they do in the movement from a research question to a research design, only with one additional feature. This time you have to convince someone else. As such, you have to make the proposal both clear and impressive.

To write a research proposal involves outlining elements of theoretical and empirical background, design, data collection, ethics and analysis that are yet to be discussed here, and which make up the rest of this book – so don't jump straight in. One thing that a research proposal needs to demonstrate is the prospective researcher's grasp of the field. As such, you will want to have worked your way through this book, at least to a degree, before actually attempting to submit your own proposal.

Whether you are an undergraduate student undertaking a small piece of research within a single course or module, or preparing for a final-year dissertation project; whether you are a professional conducting research within your own organization;

whether you are in the process of applying for funding to conduct a graduate-level research degree (such as an MPhil or a PhD); or whether you are an academic seeking funds or authority to empirically investigate theories in your field; the first administrative hoop will be getting approval for your research proposal. However, you should think of the exercise as your way of proving to yourself that your project is worthwhile, and in the process of getting approval you are likely to increase the actual validity and value of the project. There is no better way to get something clear in your own mind than to have gone through the process of explaining it to someone else. If you can convince someone else that the exercise is worthwhile, you can be very confident that it is. Unless you really are your own harshest critic (and few people are) then the approval of others, particularly those with some qualification to pass judgement, is better than just convincing yourself.

What do they want?

The first thing to think about when constructing a research proposal is the framework and regulations of the organization and system you are submitting the application to. There are seven elements to think about:

1 *Format*. Is there a set application form or guidance on formatting your application? Are you given guidance/instructions regarding how to write and the length required (minimum and often more importantly maximum word lengths)? Are you required to break the proposal down into specified sections with a specified sequence? Where applications are competing for acceptance/funding, the best way to have your application rejected at the first hurdle is not to conform to the guidance/instructions.

2 *Deadlines*. Is there a cutoff point after which applications are no longer accepted for consideration? Again, if there is stiff competition for acceptance/funding you are as well to put your application directly into the recycling bin if you can't get it in on time. Where the penalty for late submission is a deduction of marks or a delay in getting under way, it is only you who loses out. It is sensible to keep an eye on the clock.

3 *Entry criteria.* Does the organization to which you are applying have entry criteria for approval/funding? Do you have to have certain prior qualifications/experience? Some funding bodies and organizations are only accessible to certain nationalities, age groups etc. Check the entry criteria before applying.

4 *Focus*. Does the organization specify the parameters of the work it approves, the topics it is interested in funding or approving, the methods it accepts as valid or useful? There is no point applying to do a sociology dissertation in the physics department. They won't like it.

5 *Resources*. How much time and/or money is available? You have to take resources very much into consideration when constructing your research proposal. You will be turned down if your project cannot be achieved in the time frame being applied for (such as for an undergraduate dissertation or a doctoral degree), irrespective of whether it has other merits. One person funded for three years is not able to do what

three people might in the same time. You need to cut your clothing according to the cloth on offer. If you don't you may end up with embarrassing holes, or no clothes at all.

6 *Coverage*. What's covered? Does the funder or approver provide or pay for equipment, people, space and other resources for travel and related fieldwork expenses? Will you get the time off to do the work required in the project you'd like to do? Will your university fund that overseas trip your undergraduate dissertation really needs? If not, it is unlikely the project is going to be approved, unless you want to pay for it yourself.

7 *Ethical/political orientation*. What are the ethical and social/political principles held by the organization you are applying to? There is no point asking for approval for work that is at odds with these principles, as the answer will be no. You may then wish to consider which other organizations might say yes, but if you are already embedded in a particular company, department or university, you may find yourself having to play by their rules. Knowing what the rules are gives you greater scope to work the rules to your advantage. Rules are open to interpretation, so you may be able to make your case if you give sufficient attention to how to square your plans with your organization's rules.

Presenting yourself

The art of presenting a research proposal is to be clear and authoritative. Show that you know what you are talking about, but avoid jargon and verbiage. Be brief. There is usually a word limit, so practise the art of precision persuasion. People are grateful if you pay them the courtesy of not wasting their time. Don't treat the reader as a fool either. They hold the strings and, even if they don't know as much as you do about your specialist subject, if you can't explain that topic and its importance to them then you have failed, not them. Say what you intend to research, how and why, before you explain the background literature. Put yourself first, but link your ideas to those of others. Be ambitious but don't be unrealistic. Connect with the here and now, but avoid becoming a hostage to fortune (yesterday's headlines are today's waste paper).

Be:

- brief but not sketchy
- authoritative but not condescending
- original but not unrelated to what has gone before
- up to date but not just a flash in the pan
- realistic but not conservative.

How long have you got?

You need to impress upon the person you wish to approve your project that you know what you want to do, how this can be done and why it should. Central to

the 'How?' question is whether you have a realistic estimation of the time it will take to get the job done. If you appear unrealistic in your estimation of time management, the person granting approval may get cold feet. Derek Swetnam (2004: 21–2) gives an outline of the timeline for a 10 month undergraduate final-year dissertation. I have translated this into percentages of your total research time.

- reading, planning and setting up 30%
- searching and reviewing the literature 20%
- refining methodology and method/design 10%
- data collection 10%
- data analysis 10%
- preparing conclusions and recommendations 10%
- proofreading, corrections and binding 10%

Note the length of time taken before data collection begins. Don't imagine you will be jumping straight off at the deep end. Give yourself time. If you consider that only 10 per cent of your time will be spent in data collection, scale up the overall time it will take to carry out the project you have in mind. How many interviews a week? It soon adds up, and that time is only a small fraction of the overall time you will need. Don't be naïve. Take time seriously. Whilst there are significant differences between qualitative and quantitative research in terms of the distribution of time, the same general principle applies. Where fieldwork may require longer in the field and less time to calibrate in advance, the overall key is never to underestimate the time it takes to do research. The mark of an experienced researcher is that they appear confident enough to ask for more time to collect fewer data. The novice will often try to do too much too quickly and fail to deliver as a result.

Sequence

The first rule is to follow the guidance on formatting given to you by the organization you are applying to. If there is no specific guidance, Table 1.1 offers a standard default sequence.

Note that the length allowed determines the length of the sections, but whereas the title, the abstract and the aims and objectives tend to remain relatively similar in length irrespective of the overall length of the proposal, the research design and the background will be the sections to take up the additional length if extra words are available. Scale up your sections according to how much space you have to play with. A typical undergraduate final-year dissertation may only ask for a one- or two-page research proposal, whilst an application for a PhD place/funding will usually require (allow) three times this length. Research proposals within non-educational contexts

TABLE 1.1 Sequence of a research proposal

Sequence	Length	Content
Title	Usually about a sentence in length	The what question in a nutshell
Abstract	Usually about a paragraph in length	The what, the how and the why questions in essence
Aims and objectives	Usually a couple of paragraphs	The how and the why questions in a little more detail
Research design	Usually the largest part, as much as you have space to say	Detail the collection, analysis and ethical dimensions (and timeline)
Background	Around half the design section	Practical experience and where the research sits in relation to past and up-to-date developments in the field

may vary somewhere in between. An academic applying to research councils for grants may have to fill in dozens of pages.

All of the above may seem rather daunting and offputting. Nevertheless, it should be seen as an opportunity to clarify your own ideas rather than as an exercise in jumping through other people's hoops.

Summary

Social research takes many forms. The classical experimental method is rare in social research, but in other respects much social research adopts a scientific approach. Deductive research seeks to test a proposition, while more inductive research seeks to explore a research question or field. Hypotheses and research questions emerge from social and political issues and from the researcher's own personal and theoretical motivations. However, research needs to demonstrate that its findings are the result of rigorous methods and not simply the motives of the researcher or those funding them. Are human beings 'free' agents, or 'social' beings? Answers to such speculation shape the kinds of question we might want to ask and the hypotheses we might formulate, as well as the level of prediction/explanation we might expect our accounts of society to give us. Social life is never fully predictable. 'Data' are what the researcher collects – by asking questions, observing situations or reading human records. Validity, reliability and generalizability are all criteria by which the 'truth' of research can be judged. The quality of the selected sample in relation to the population in question, as well as the quality of the data collection instruments, will determine the depth and scope of the findings. Some research seeks not only to know the world, but also to help change it. This approach raises certain ethical and validity questions. Such approaches offer

their own solutions as well as limitations. The development from research question to research design and their combination within a successful research proposal should form a logical progression, even as each step along the way will lead to reflections and adjustments of the steps that have gone before.

Questions

1 **How far can social research motives be separated from research methods?**
2 **What is the relationship between empirical research design and forming a research question?**
3 **Should social researchers emulate the natural sciences?**

Further reading

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