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If you are going to do research you need to be curious. Being genuinely curious doesn’t just mean being open-minded about the answers to questions; it also means thinking carefully about the questions you ask in the first place. Doing research can lead to surprising findings that may challenge your current ideas and beliefs. But if you are going to conduct useful research it is important that you are open-minded about both the questions you ask and the answers you discover.

It’s important to be interested in the topic you are investigating, as it will be hard to complete your project unless you can find something that holds your interest through all the stages of the research process. But being interested in a topic and having an interest in the results you produce are very different. If you have something to gain or lose based on the outcome of a study, you are unlikely to be the best person to conduct that research, as you may prefer certain outcomes or findings to others.

You can still be genuinely curious and have ideas about the answers to your research questions. It’s very important to have a clear idea about the type of answers that are possible given the nature of your questions, and it can even be useful to make predictions about what the results might be. As I explain in Chapter 2, this is exactly how what we call hypotheses are generated. But it’s crucial that you’re prepared to be surprised by your findings and that you don’t rule out any possibilities in advance (Pole & Lampard 2002).

When I meet with students to discuss their research projects for the first time, many of them tell me they want to ‘prove’ something. Some firmly believe that they already have the answer to the ‘question’ that they are ‘asking’ and mistakenly think that research is about working out what you believe and then finding data that will support this belief. (These views aren’t just confined to students and new researchers – I have also heard academic colleagues express this view.) I try to explain to my students that this is not a good way to start a research project because research should be guided by genuine curiosity and a desire to find things out. And finding things out is what all research is really about.

Being open-minded about research questions and the results they produce is not as common as you might think. Academic communities can be quite conservative
Where Do Research Questions Come From?

and ‘habits of thought’ within disciplines or subject areas can lead to research becoming formulaic and repetitive (Sellitz et al. 1965, Alvesson & Sandberg 2013). Researchers asking new or different questions can attract a greater degree of scrutiny than those carrying out more conventional studies. Research findings that are unexpected or surprising are certainly not treated the same as results that reflect the status quo. These kinds of findings, though, can be very important and play a significant role in increasing our understanding in a particular area (Labovitz & Hagedorn 1971, Campbell et al. 1982, Alvesson & Sandberg 2013).

Although curiosity is essential for conducting good research, it’s not all you need to become a good researcher. All research needs some kind of planning, and going ahead with your research relying on curiosity alone is likely to result in a haphazard approach to designing and carrying out your project. A well planned and clearly thought out study is more likely to produce valuable and robust findings, and will require you to apply your curiosity in a systematic and disciplined way (Graziano & Raulin 2004). Over the course of the next five chapters I show you how to:

- generate ideas for a study;
- turn these ideas into researchable questions;
- start planning a research design that will allow you to collect and analyse the data you need; and
- make sure that the conclusions you draw are the most likely explanations of your findings.

In the next section in this chapter I look at where the ideas for research questions come from and suggest some activities that could help you find a topic to study. I also show you how to move from picking a topic to formulating a clear set of research questions.

The Importance of Existing Literature

Research does not, or at least should not, take place in a vacuum. You need to know how your study will fit into the broader picture, in terms of previous empirical research, theoretical ideas, and policy and practice that are relevant to your topic. If you aren’t aware of this wider context, you risk asking questions that are irrelevant, repeating the mistakes made by others or simply ‘reinventing the wheel’ by trying to find out something we already know. We make the most progress when our research is cumulative, and so we usually aim to build on what is already known in a particular area (Lewins 1992).
Most writers on research methods agree that you should engage with the existing literature to some extent before proceeding too far with your study (although there are some notable exceptions to this). Reviewing both the academic and the non-academic literature can help you work out what we already know about a topic and reading around an area can help you come up with ideas for research. It’s a good idea to take notice of the research designs and methods of data collection and analysis used in other studies, as this can help you when you come to plan your own research. The literature can also help you establish the importance of your study by setting it in a wider context and linking it to related debates.

In the following discussion, I ask five questions about the relationship between the literature and your research questions, and make some suggestions for resolving them:

• Should I start by reviewing the literature or not start reading until I have a research question?
• Where do I start reading and when should I stop reading?
• What counts as a ‘gap’ in the literature and how large does it need to be?
• What makes research ‘original’ and how important is originality?
• To what extent should I be influenced by previous findings and theories?

When Should I Start Reading?

One of the problems you might face at the beginning of a research project is whether you should formulate your research questions before reviewing the literature or use the literature to generate ideas for research questions. This is something of a ‘Catch-22’ situation because while the literature review can help you identify interesting and topical questions, it’s difficult to know where to start reading if you don’t have a question to act as a guide. So the answer to this question depends on how well you have developed your ideas about topics and questions.

If you have a reasonably clear idea about the topic you want to investigate, the existing literature can be very helpful in identifying key issues in a particular area. Searching for literature has become much easier in the last 10 to 15 years as new publications appear online at the same time as, or even before, print versions, and a lot of older printed material has now been made available digitally. You can start your search by:

• using search engines to find literature on the web;
• searching online bibliographic databases;
Where Do Research Questions Come From?

- asking teaching staff or academic colleagues; or
- simply browsing the relevant section of your library.

Once you have found a small number of relevant texts, you can ‘snowball’ your search by using the references listed at the end of each text to track down more literature in the area. The problem you will then face is when to stop reading and start formulating your research questions. This is something we will look at later in this chapter.

If you have absolutely no idea about what you want to research, it’s difficult to know where to start looking and your literature search can easily become aimless and time-consuming. It is important to have at least some idea about what you want to research before beginning to review the literature in any depth. The more focused and developed your idea, the more it will guide you during the literature search. In the next section I provide some advice on where to start reading if you have only very vague ideas about what you might want to research.

Spending time thinking about research questions at the very beginning of your research will save you a considerable amount of time when it comes to reviewing the literature. This is particularly important if you are expected to complete your research project in a relatively short space of time, as is usually the case for undergraduate or master’s level degrees. Being as clear as you can be about what you want to research before you start reading in depth is important, as it is easy to spend a great deal of time ‘bogged down’ in the literature if you are not really sure what you are looking for. You will probably find that your questions change to some extent once you are familiar with previous research findings and key debates in an area, but try not to worry about this when you are initially thinking about ideas for a study. At this stage, what is important is that you have a sufficiently clear idea of what topic you want to research to provide direction for your review of the literature.

Where Should I Start Reading?

Even when they have identified a topic to investigate, many students find it difficult to find a place to begin reading. And once they have found their way into the literature they sometimes find it difficult to stop reading and move on to the next stage of the research. After all, reading other people’s research is something they are already familiar with, and it’s much less daunting than designing and carrying out a study of their own.
Where Should I Start Reading?

Getting Advice

Because research is published in a variety of forms, with different publications targeting different audiences, it’s not surprising that inexperienced researchers find it difficult to identify the reading that will be most useful to them. Even within a well-defined topic, the vast range of literature available can seem overwhelming. Many undergraduate students have reading lists for all their classes and are not used to finding all the literature they need on their own.

One of the best ways to navigate the literature is to seek some expert advice. If you have been allocated a research supervisor they will usually be familiar with the area you are studying and should be able to point you in the direction of some key publications. If your research is unsupervised you may find it helpful to approach a knowledgeable member of your department or organisation for advice. Don’t be afraid to venture beyond your own backyard, though; researchers working in other departments or even other institutions will often respond helpfully to a simple email request for information on suggested reading. Seek expert advice early on if possible, as this will reduce the time it takes you to find out what are the most important texts in the area. But make sure you have done some initial searching before you ask for help. Each year I have students come to see me saying things like: ‘I can’t find anything about the educational achievement of different ethnic groups.’ It’s slightly embarrassing for them when I type a few keywords into a search engine and find a long list of relevant books, articles and reports in a matter of seconds.

If your research is unsupervised and you can’t find an expert to guide you, there are many textbooks dedicated to conducting literature searches that you can use. These can be useful even if you have had guidance from experts or teachers, as doing a research project is as much about learning new skills as it is about carrying out a successful study. You’ll be expected to develop your independence as a researcher over the course of your study, rather than just follow your supervisor’s directions, and at each stage of your project you will increase your knowledge and skills.

Different Types of Literature and How They Can Be Used

Particular types of publication can be very useful starting points for a literature review. If you have a fairly clear idea of what topic you are interested in, specialist, peer-reviewed journals can be a good place to start. Journals vary in the degree to which they specialise, however, and particular titles will be more or less useful depending on their coverage of your research topic. Even within a
particular area, there are large numbers of publications, and these range from the very general to the very niche. In the area in which I conduct most of my research—education—there are literally hundreds of different journals serving different purposes and aimed at different audiences. While those such as the *American Educational Research Journal*, the *Australian Educational Researcher*, the *Asia Pacific Journal of Education* and the *British Educational Research Journal* are widely read and very prestigious, their breadth of coverage means that only a very small proportion of articles they publish coincides with my own interests. Other journals such as *Gender and Education* and *Race, Ethnicity and Education* focus on particular issues relating to education but still cover a wide range of substantive areas. At the other end of the scale are journals dedicated to very specialist areas of inquiry, such as the *Journal of Music Teacher Education* and *Children’s Literature in Education*.

The type of journal that is of most use will depend on the extent to which you have developed and refined your research questions. If your questions are relatively well defined you are more likely to be able to find specialist journals focusing on relevant topics. You should bear in mind, though, that a good literature review will cover material from a wide range of sources and should not be restricted to articles in a few specialist publications. At some point you will need to broaden your search beyond specialist outlets and link your study to wider debates. I look at how specialist and more general literature fits into your literature review later in this chapter, where I introduce the idea of the ‘literature funnel’.

Another way of finding relevant literature that might be more useful if your ideas are less well developed is to look for introductory texts. These can range from the very general such as *Sport, Culture and Society* (Jarvie 2012) to more specialised texts such as *Sport and Social Exclusion in Global Society* (Spaaij et al. 2014). These texts are designed to familiarise readers with key issues in a particular area and can be a good starting place if you need to narrow down your initial area of interest. Also useful are edited collections or ‘readers’. These are books that focus on a particular theme or substantive area, with each chapter written by different authors. They collect important work in a particular field and can offer a useful overview of research in that area. An example of such an edited collection would be *Fighting Fans: Football Hooliganism as a World Phenomenon* (Dunning et al. 2002). Larger edited collections, often called ‘handbooks’ are also available in some areas, the *Handbook of Sport and New Media* (Billings & Hardin 2016) being one example. These sometimes span several volumes but can be useful for assessing the ‘state of the art’ in a particular field relatively quickly. Like journals, however, readers and handbooks vary in the degree to which they specialise. Find out what is available in the area you are planning to
research and try to identify publications appropriate to the stage your ideas have reached.

**Different Uses for the Literature**

How you engage with the literature can vary depending on where your research questions come from. If your questions have arisen from professional, organisational or institutional contexts (see below) your study is likely to be more ‘applied’ in focus and concentrate on particular problems with immediate practical relevance. Your research questions may already be well defined and have been formulated before you even begin reviewing the literature. If this is the case, the literature will provide a wider context for your study, and fill you in on what is already known, rather than generating ideas for research questions. Don’t assume that just because your questions are practical or applied they won’t be relevant to important academic debates. To maximise what can be learned from your findings, it’s important to think about how your study can be linked to other work in similar (and sometimes even very different) subject areas.

Most writers agree that you should conduct a literature review regardless of where your questions came from. While the way you use the literature may vary depending on the type of research questions you want to answer, it’s important not to exaggerate the differences between ‘applied’ and ‘academic’ or ‘theoretical’ research. There is no firm dividing line between these categories (Hakim 2000) and most studies do not fit into a single one (Graziano & Raulin 2004). Solving practical problems sometimes requires background knowledge and theoretical understanding, and theoretical issues often arise when practical problems are explored (Hammersley & Atkinson 1995).

Reviewing the literature can help you make links between ‘applied’ and ‘theoretical’ issues. Questions that originate in practical concerns should, where possible, be linked to relevant theories in order for the possibility of the findings being transferred to other problems to be explored (Sellitz et al. 1965). In this way it is possible to move ‘upwards in generality and abstraction’ and connect specific research questions to more general issues (Punch 1998, p. 35). I look in more detail at how theory can be connected to your research questions later in this chapter.

Punch (1998) recommends two different methods of arriving at research questions and describes the ways in which these can be connected to the wider literature. If you have already identified specific research questions (as is common in applied research) it is helpful to first link these with more general questions,
which can then be connected to a wider area of study. For example, research aiming to discover the reasons young people give for drinking alcohol could be linked to research on young people’s lifestyle choices and the literature in the area of drug use more generally. Evidence from such a study could also inform theoretical debates about decision-making, risk-taking and the nature of ‘youth’ in modern societies.

If you haven’t identified any research questions but you know you want to research a particular topic, the literature can help you move towards more specific ideas about your study. You can do this either by narrowing the focus of your topic, identifying some research objectives or by formulating some very general questions to start working with.

You might, for example, be interested in the areas of crime and gender and want to combine these topics in your research. This is a very broad area and, unless any particular questions immediately spring to mind, exploring the literature might help focus your interest on a particular issue. While this is a topic I am completely unfamiliar with, a quick search of my library’s electronic catalogue revealed three relatively recent textbooks in this area (Silvestri & Crowther-Dowey 2008, Davies 2010, Hodge 2011), as well as more than ten similar, older texts. I also found a government report on the topic (Ministry of Justice 2014). Browsing these publications will not only help you narrow down your area of interest but can also point you in the direction of more specialist literature. Looking at the titles of the chapters in any of these textbooks could lead you to decide that you are most interested in women offenders. You could follow up the references provided at the end of the chapters you have read and then start to conduct a more focused search of the literature. A quick search of my library revealed at least one recent text specialising in this area (Sheehan et al. 2013). As you continue to narrow your topic you will find that your literature search becomes both easier and more efficient.

Are All Types of Literature Useful or Necessary?

While there is considerable debate surrounding ‘grounded theory’, an approach to research originally proposed by Glaser and Strauss (1967), many researchers advocating this method suggest deliberately delaying the literature review until after themes and concepts have emerged from a preliminary analysis of the data you have collected. Instead, literature is introduced later in the research process and treated as additional data (Punch 1998). This approach is intended to reduce the influence of existing ideas and explanations on the researcher’s interpretation of their data and, in particular, their own theory-building.
Other commentators suggest that those conducting applied research don’t need to be too concerned with theory. Tymms and Taylor Fitz-Gibbon (2002) argue that applied research is more likely to be hindered than helped by a preoccupation with theory, and that research into practical problems can be conducted perfectly well (or perhaps even better) with minimal theoretical knowledge (see also Scriven 1998).

These approaches, however, are risky for inexperienced researchers and may not even be possible for students conducting research as part of their undergraduate or master’s degree. Detailed proposals for research projects, including summaries of the theoretical background and relevant substantive literature, are often required as part of the assessment process. And funding bodies that sponsor postgraduate students, such as the National Science Foundation, the Australian Research Council and the Economic and Social Research Council, require similar documents as part of their studentship funding application processes. Undergraduate or master’s students are probably best sticking to a more conventional approach, especially if there are strict guidelines on how your work will be assessed. Doctoral students, though, might want to read more about these less conventional approaches to reviewing the literature before deciding whether they are suitable for their own research.

There are very good practical reasons for not delaying your literature review until relatively late in the research, whatever your level of experience. It may be the case that the questions that you have raised have already been answered by previous research, in either the same context or one that is very similar. If you’re conducting an undergraduate research project this might not be too much of an issue, but for master’s level or doctoral research it could be a problem. Deliberately avoiding reading around your topic when planning your research risks ‘reinventing the wheel’ by unknowingly repeating previous work (and perhaps making the same mistakes!).

The degree to which your research should be ‘new’ or ‘original’ is not a simple matter. Deciding whether a set of research questions covers a sufficient amount of new territory will depend on the level you are working at, what you are expected to achieve and the resources you have available. I discuss these issues in more detail later in this chapter, alongside the idea of ‘gaps’ in existing knowledge and the importance of ‘originality’ in different types of research.

**Stop Reading and Move On!**

One of the most difficult decisions during the course of a research project is when to stop reading. Stopping reading can be stressful because, as I have already
Where Do Research Questions Come From?

mentioned, reading other people’s work is something you’re probably used to and feel comfortable doing. If you haven’t conducted your own research before, moving on from reviewing the literature to designing and conducting your study can be quite intimidating. As always, supervisors or experienced colleagues will be able to offer you advice in this area but you should still be prepared to make some difficult decisions on your own.

An important point to remember is that moving on from the literature is not an ‘all or nothing’ final decision. You’ll probably read more intensively at the beginning of your project but you should carry on reading throughout the research process, whenever you have time. Developing research questions is an ‘iterative’ task, where you move back and forth between the literature, your evolving questions and sometimes even the data you have collected (Marshall & Rossman 1999). But the literature review is never really ‘finished’ and you certainly shouldn’t worry about moving on to other parts of the research process once you have finalised your research questions. You will never be able to read everything written on your topic and you can always go back and do a little more reading later on.

One final point about reviewing the literature is that it can be a frustrating process, especially in the early stages. Literature searches are inefficient and time-consuming by nature. It’s possible to move quite quickly from a situation where you can’t seem to find many relevant publications to one where you have so much literature you don’t know what to read first. You might spend a long time locating and reading a particular article only to find it isn’t really useful to you. This doesn’t necessarily mean that you’re doing something wrong. Using expert advice and the latest search tools can help you to be more efficient but if you haven’t conducted a literature review before be warned that it isn’t always straightforward and can take a lot of time. Don’t underestimate how much time and energy this vital stage of the research process can take.

**Key Points**

- Try to get expert help on where to begin reading but do some initial searches yourself.
- If you already have some research questions, specialist journals and edited collections are good places to start your literature search.
- If you only have vague ideas about topics or questions, introductory texts or ‘readers’ can be better places to start.
- It’s best to start to explore the literature at the beginning of your project, rather than leave it until later on.
- Try not to worry about moving on from the literature once your research questions are finalised; you can return to the literature later.
Not Reinventing the Wheel: What Counts as a ‘Gap’?

Reviewing the existing literature in an area serves several purposes. One of the most important is to help you identify what is already known about a topic so that you don’t ask a very specific question that has already been answered (Cozby et al. 1989). This doesn’t mean that you can’t replicate a particular study — this can be a very valuable exercise — but you first need to know about the study you want to replicate! The key point here is that if you want to find out something new it is pointless to conduct a study to answer research questions that have already been dealt with thoroughly.

One of the most commonly suggested ways of generating ideas for research questions is by identifying a ‘gap’ in the existing literature (Mason 1996). Alvesson and Sandberg (2013) are critical of this approach to developing research questions, arguing that it avoids questioning the assumptions on which existing research is based. They may have a point in terms of particular areas of professional research but ‘gap-spotting’ still has a place and is a much more manageable goal for students and new researchers.

To find a ‘gap’ you obviously need some knowledge of the relevant literature. Identifying gaps can be tricky, as even experienced researchers are unlikely to have a comprehensive knowledge of all the literature in a particular area. It’s difficult to be sure that something hasn’t already been done when there is always another article you could read.

Finding a gap in the literature can also be quite a subtle activity. If you are relatively inexperienced, you might find it difficult to look at research in an area and be able to immediately see what is ‘missing’. It’s a skill that you learn over time and some of the most interesting studies are produced when someone spots a gap that no one else had seen, but which seems obvious once it is revealed. Despite what Alvesson and Sandberg (2013) argue, gap-spotting can be creative, requires considerable imagination and also questions the assumptions underlying existing research.

A good example of creative gap-spotting is Bartram’s (2013) research on economic migration and happiness. A growing interest in measuring ‘happiness’ and ‘well-being’ had led to the discovery that the relationship between economic status and happiness was an interesting one. People who were rich were usually happier than those who were poor but happiness also depended on relative differences in income and wealth. Regardless of your absolute level of income or wealth, being better off than other people in your society or country generally made you happier. This meant that two people with the same income or wealth but living in two different countries could have very different levels of happiness or well-being.
Bartram was interested in migration and this finding on economic position and happiness prompted him to ask questions about whether people who went to work in countries to earn more money – economic migrants – actually became happier. Did they become happier because they earned more money? Or were they actually less happy because, although they earned more money in absolute terms, they would often be earning less relative to those in the country in which they now worked?

The process needed to come to these research questions required considerable imagination and creativity, as well as in-depth knowledge of research findings in two different areas. On the face of it this might be considered ‘gap spotting’ but it shows that this process isn’t as conservative as some commentators suggest and can lead to ground-breaking new research.

Experienced researchers often start by identifying questions that may have been addressed in previous research but have not been answered completely or correctly. Contradictions, inconsistencies and incomplete explanations can all provide good starting points for research (Booth et al. 2003). If you’re a student or a new researcher, this strategy might seem too ambitious or risky but you shouldn’t rule it out as a possible source of ideas. You need to make sure that such questions do actually address a genuine deficiency, absence or omission, though. Booth et al. (2003) warn that a lot of time and energy has been wasted trying to challenge a claim that no one has actually made. It is possible, however, for you to test the ideas of other researchers or develop existing research in new ways.

How Big Should a ‘Gap’ Be?

My students are sometimes concerned that the questions they want to address are too minor. Their fears are almost always unfounded. My experience is that they are much more likely to be over-ambitious in the initial stages of planning a study and often have to be persuaded to be more modest in their aspirations. The ‘gap’ they have spotted or the questions they want to answer are usually too large or broad to be researched with the time and resources they have available.

It is important to remember that the incremental and cumulative nature of research means that many researchers spend most of their professional lives investigating very specific issues. With others working in the same field, their research will be contributing to the process of answering much larger questions that have more obvious implications and importance. But while new researchers may wish to contribute to these ‘big’ questions, their own research questions need to be relatively narrow in scope (Black 1993).
It’s also important to remember that not all research has to be highly original, especially if it’s being conducted as a relatively small part of a degree or other course. The extent to which you need to spot a gap or your study needs to be original will depend on why you’re carrying out your research project and what the results are intended to be used for. This issue of ‘originality’ is discussed in more detail in the next section.

**Key Points**

- It’s important to know the literature in your area so that you don’t ‘reinvent the wheel’.
- Finding a ‘gap’ in the literature can be one way of generating ideas for research.
- Don’t worry about the focus of your study being too minor. It is more likely that your initial ideas will be over-ambitious.
- Not all research projects need to be ‘original’ or fill gaps in the literature; this will depend on your level of study.

**Originality**

Before discussing originality in any detail it’s important to repeat the fact that some research projects don’t have to be original at all. Most undergraduate dissertations are tests of students’ competence in research, rather than opportunities to make an original contribution to knowledge. If you’re an undergraduate student, you shouldn’t worry too much about the extent to which your research is original or fills a gap in the literature. Your priority should be to formulate research questions that can be addressed using the limited time and resources available to you. Although I’ve seen many very good pieces of research produced by undergraduates, you won’t be expected to make any ground-breaking discoveries. But as you’ll see from the discussion below, whatever you choose to study, it’s actually very unlikely that your research won’t be original in some ways.

If you’re a postgraduate student studying for a master’s or doctoral degree you may be required to incorporate some element of originality into your research. The extent to which master’s theses need to be original will vary, but it’s usually assumed that doctoral research will be ‘original’ in some respect. It’s best to be clear about this requirement before starting your research. Your course handbook or supervisor/advisor should provide this information. This shouldn’t be a cause for too much concern, because as I’ve already suggested, doing research that is
completely unoriginal is actually quite difficult, and is virtually impossible if you
collect new data. However, you may need to satisfy your supervisor or examiner
that your research does make a sufficiently original contribution and the following
discussion should help you in this task.

‘Originality’ is a problematic term in relation to social research. It’s not clear
that any study is entirely ‘original’, in the common-sense understanding of the
term, or that even replication studies are unoriginal. Denscombe (2002) suggests
that a study can be considered original if it makes a new contribution in one or
more of the following four areas: topic, method, data or analysis. If a study focuses
on a completely new topic, the research will obviously be original as it would be
the first to explore this particular area. This would also be the case if new questions
were asked in an area that was already well researched.

Research can also be considered original if an innovative research design or new
method of data collection or analysis is used, regardless of the topic of investigation
or questions posed. Sometimes established methods can be used for the first time in
an established area. One of my doctoral students, for example, is using agent-based
modelling (ABM) to explore patterns of female genital mutilation (FGM). While
ABM has been developed and used for some time in other contexts, and there is
considerable existing research on FGM, ABM has allowed new questions to be
asked about the development of patterns of FGM in different geographic locations.

Denscombe (2002) argues that any research with new data is original. As the
majority of studies include at least some data that could be considered ‘new’,
completely unoriginal research is something of a ‘straw target’ in social research.
Although it is possible to imagine exceptions, almost all data that are collected in
fieldwork or laboratory settings are ‘new’ data, in the sense of not existing before
that study took place. Even data collected as part of a replication study are new, as
they relate to different participants, in a different context and at a different point
in time.

The only data that do not meet the criterion of being ‘new’ would be what social
scientists traditionally call ‘secondary data’. Secondary data have been collected by
someone other than the researcher, sometimes to answer very different questions.
As these data already exist before the researcher analyses them I prefer the term
‘existing data’ but ‘secondary data’ is the term usually used by social researchers.

Secondary data can be a very useful resource for researchers, regardless of
their experience. These data sets are currently under-used in many areas of social
research but, thanks to a number of recent initiatives and publications, the potential
they offer is now more widely known (see Smith 2008, MacInnes 2017).

Even if your study relies exclusively on secondary data, there is still considerable
scope for it to make an original contribution. The range and availability of data sets
mean that the opportunities for conducting new analyses are enormous. As long as you’ve reviewed the literature to check that no one has conducted exactly the same analysis as the one you are planning, originality shouldn’t be a problem when using secondary data.

As long as you are aware of what has already been done, there is considerable scope to introduce originality into your project by asking new questions, using new data or applying new methods. But whatever your level of study, you need to be clear about the extent to which your study will be expected to be original. In my experience, most students overestimate the originality required in their work. There is no substitute for talking to your supervisor or advisor, and reading your course requirements carefully. Listening to the advice of others can be very valuable but you should balance this with your own judgement when making decisions about the direction of your study, especially if you are aiming to pursue a career in research. In the next section, I discuss the benefits and pitfalls of being influenced by those in your field.

**Key Points**

- If you are an undergraduate student, you shouldn’t worry about the extent to which your research is ‘original’. Instead concentrate on demonstrating your competence in conducting research.
- Originality can take many forms, including innovation in relation to methods of data collection and analysis as well as asking new questions.
- Using secondary data doesn’t mean that your research will be unoriginal. It can be a useful resource for very original work.

**Influence**

An issue closely related to originality is how much you should be influenced by existing work in your area of interest. It’s very easy to follow current fashions when choosing topics for research but there are also dangers to this (Medawar 1979, Campbell et al. 1982, Alvesson & Sandberg 2013). Don’t automatically judge the importance of questions or topics by how often they occur in the literature, as some of the more popular questions may be studied simply because they are straightforward to research rather than because it’s important that they are answered. Other more significant questions may have been avoided because addressing them would be very challenging. Take care to recognise, however, that particular questions may be left unanswered simply because they are unresearchable (see Chapters 2 and 3).
Relying too heavily on existing studies for inspiration for your own research questions can lead to research that is mundane or derivative. This shouldn’t be too much of a concern if you’re an undergraduate, as it is your ability as a researcher rather than your contribution to knowledge that is usually being assessed. But if you’re a doctoral student wanting to make a new and interesting contribution to your field, the literature can hinder as well as help the creation of new research questions. There are some very well established ‘habits of thought’ (Sellitz et al. 1965, p. 31) among researchers in particular areas and this can lead to conservatism about what is researched (Punch 1998).

Conservative thinking in particular areas of study, or even in whole disciplines, doesn’t only restrict what is studied but also how the research is conducted. Don’t be afraid to use new and innovative research designs or methods of data collection and analysis, as long as you are confident they are appropriate and that you have the ability and resources needed to use them. The fact that certain kinds of question logically lead to the use of particular research designs (see Chapter 4) doesn’t necessarily mean that the most commonly used designs or methods are the most appropriate to investigate a particular topic. Unfortunately, it’s very common for researchers to use the methods of data collection and analysis that they are most comfortable with rather than those that are best suited to the objectives of their research. Just because there is a tradition of using certain research methods in a particular area doesn’t mean that there is no room for innovation or creative thinking in research design, data collection or data analysis.

**Key Points**

- Don’t always judge the importance of questions or topics by how often they arise in the literature.
- Paying too much attention to previous studies can lead to derivative and conservative research. Just because something has always been done in a particular way doesn’t necessarily mean that this is the best way to do it.
- You need to be aware of relevant studies but shouldn’t be afraid to ask new questions or use innovative research methods.
- If you want to pursue questions that seem to have been overlooked, you need to make sure they are ‘researchable’.

In this section I’ve tried to answer some common questions about the relationship between research questions and the wider literature. Before looking at research questions themselves in more detail in Chapter 2, in the rest of this chapter I examine two particular sources of questions in greater detail: theoretical ideas and practical contexts.
The Role of Theory

Theories are nets to catch what we call ‘the world’: to rationalise, explain and master it. (Popper 1959/2002, pp. 37–38)

Theoretical ideas often influence the kind of research questions we ask. In fact, some academics spend their entire careers engaging with theoretical questions, either happy to leave the testing of their theories to others or unconvinced that theories need to be tested at all. Students are often told that they must link their research projects to wider theoretical debates and may be encouraged to think about the ways in which their research contributes to theoretical understanding in a particular area. But what exactly do we mean by theory? And how can it help you generate your research questions? In this section, the idea of theory and its relationship to research questions is explored. I examine two different ways in which the term theory is used and look at ways in which ‘theory’ can be used to generate ideas for research questions.

It’s important to make it clear that ‘theory’ is a contested term. While many people write about ‘theory’, they are not always referring to exactly the same thing and the meaning of the term varies between the natural sciences, the humanities and the social sciences (Abrahamson 1983). In this section I argue that some uses of the term are more helpful than others, and I present two ways in which ‘theoretical’ ideas can be used in your research. Using theory in either of these two ways could help you generate or develop your research questions and link your project to wider ideas in your field of study.

Theories as Explanations

Researchers often use theories to explain something that has happened or is happening (or even is about to happen). But theories are more than simply ideas. Not every explanation counts as a useful theory, because theories have to have certain characteristics if they are to be useful. Theories that are intended to explain the social world should be:

- abstract;
- explanatory; and
- testable.

An idea is ‘abstract’ if it applies to a variety of different situations, people or groups, rather than only a single event or person. A theory of social stratification,
for example, would aim to explain why lots of different people find themselves in a number of socio-economic situations, at different times, rather than simply how one individual has ended up where they are at a particular moment. There are different levels of abstraction, and the ‘higher’ the level of abstraction, the more ambitious the theory and the more it is intended to explain. Rational choice theory (RCT) (Allingham 2002), for example, is seen as a ‘middle range’ theory, as it focuses on one particular aspect of social life – decision-making – but aims to explain how people make choices in a wide range of social and economic situations. Basil Bernstein’s (2009) theory of the transmission of educational advantage is slightly less ambitious, as it aims to explain outcomes in a particular setting at a particular stage of people’s lives. It is, however, linked to a higher-level theory he developed about language codes. Bulmer (1979, p. 21) provides a very clear diagram showing what is meant by ‘abstraction’ if you need a more visual illustration of this idea.

The second characteristic of theories is that they should not just be descriptive. Continuing with the example above, a theory of stratification should be able to explain why some people move between social or occupational classes and others do not. It should seek to go beyond describing how society is currently stratified, to explain exactly why it is like this and even to predict how it might change. One test of a theory is how well it is able to make predictions using data that are different from those that were used to formulate it. This links directly to the last quality of theories – that they should be empirically testable.

We should be able to see how well a theory works in practice by collecting and analysing data. This is perhaps the most important characteristic of a theory. While some commentators have questioned the extent to which theories should be either explanatory or abstracted (e.g. Shaw & Costanzo 1970, Bulmer 1979) the idea that a theory should be able to be tested empirically is central to most definitions (Cozby et al. 1989). A theory that cannot be tested is of little use, as it is impossible to assess the extent to which it matches up with reality. It is unusual for whole theories to be tested by one piece of research, however. It is much more likely that a series of ‘hypotheses’ are derived from a theory and then tested individually. I look at hypotheses in more detail in Chapter 2.

**Concepts**

Some of the ideas we use in social science are ‘theoretical’ but are not necessarily explanatory or testable. A better term than ‘theoretical’ might be ‘conceptual’, as these ideas often relate to concepts or models. Concepts are different from the type
of theories we have already discussed, because they are descriptive rather than explanatory. But they do rely on particular assumptions about how the world works and also involve a certain amount of abstraction.

Social class is a good example. While social class is a widely used concept in social research, there is a great deal of debate about how it should be measured and even how it is defined in the first place. Regardless of how it is defined and measured, however, we find time and time again that people’s social class is related to their life chances. The chances of being a victim of a crime, suffering from long-term ill-health, studying at university and many other important aspects of our lives are strongly related to our social class background.

Dividing people into different social classes when analysing data does not, on its own, explain anything – it simply describes patterns in the data. Neither does it always test a particular hypothesis that has been derived directly from a higher-level theory. But the idea of social class, as with many of the concepts we use in social research, relies on particular assumptions about how the social world is, if not about how it works. There are some theories of class that incorporate an explanatory element but the concept of social class is often used separately from any explanatory theory.

In Chapter 4 we will look at some of the issues surrounding defining and operationalising concepts that you might use in your research and how these relate to formulating and developing research questions. Before moving on from the topic of theory, however, the difference between theory testing and theory generation is discussed below.

**Theory Development and Theory Testing**

Many researchers have argued that, to be meaningful, research questions should be linked to theoretical issues (e.g. Campbell et al. 1982, Popper 1972, Bradley 2001). Research that addresses wider theoretical concerns is much less likely to be dismissed as trivial and will also appeal to a wider audience (Hammersley & Atkinson 1995, Mason 1996). But what are the best ways of doing this? And how can you use theory to help formulate research questions or link your existing research questions to theoretical issues?

Theories can be related to research questions in two main ways. As I mentioned above, they can be tested, often by applying them in a new area or with new data. Alternatively, gaps in existing theories can be identified and research can aim to generate and develop theory in order to make up for this absence. These two approaches are often described as ‘theory testing’ and ‘theory generation’.
In ‘theory testing’ (‘theory-first’) studies, hypotheses or research questions are derived from a theory and are then subjected to empirical testing. Creating hypotheses from research questions is discussed in Chapter 2.

In contrast, the aim of ‘theory generation’ (or ‘theory-after’) research is to produce theories as the result of the investigation (Punch 1998). Alvesson and Sandberg (2013) view producing ‘interesting theories’ as the goal of all research. But if all research focused on developing theories none of these ideas would actually be tested, so theory generation must be seen as only one type of research, with a particular type of value. Any theory, however ‘interesting’, ultimately needs to be tested and re-tested against as wide a range of data as possible in order to see how well it actually represents the social world.

Whether you decide to adopt either of these approaches, and which one is appropriate if you do, will depend on many factors. The most important consideration is likely to be the extent to which theories have already been developed in your area of interest (Punch 1998). This is something you need to find out while doing your literature review. It may be the case that the area you have chosen is under-theorised, in which case a theory-generating project may be useful. Alternatively, there may be a great deal of theory but few empirical studies testing these ideas. This could provide an opportunity to derive some hypotheses or research questions from these theories and design a study to test out some of their claims.

Two common misconceptions are that you must conduct ‘qualitative’ research in order to generate theory and that theories can only be tested in ‘quantitative’ studies (Hammersley & Atkinson 1995). This is certainly not the case. Whether you generate or test theories (or even do both) will depend on the topic, context and practical circumstances of your research (Punch 1998). The deciding factor is the extent to which developing theory in a particular area is useful, rather than the methods used to collect or analyse your data.

One of my recent undergraduate students, for example, decided to use a theory-testing approach in his research project. He was interested in finding out the reasons why some students take ‘academic’ A levels and others take ‘vocational’ A levels. After doing some reading in the area, he found that many studies of educational choice referred to Pierre Bourdieu’s concept of ‘cultural capital’ (Bourdieu 1986). He decided to see if this theoretical concept could help explain why students opted for different types of qualification in the same subject area. As is discussed in Chapter 4, this concept is not without its problems but in this case it provided the stimulus for this student to formulate some interesting research questions and attempt to test an element of a wider theory in a particular context.

My doctoral research was also in the area of educational choice (see White 2007). I was unhappy with the limitations of the theories of choice that were
popular at the time (including those using Bourdieu’s concepts) and developed a new theoretical model of decision-making using the data collected in my study. While it is easier to make a theoretical contribution in areas where conceptual work is sparse, the quality of existing theories can be as important as their quantity when making decisions about whether new theory needs to be developed. If you’re not happy with the theories that are currently used in a particular area, you might want to consider developing your own.

The theoretical literature can be a useful source of research questions. However, if you are interested in testing or generating theory, you need to make sure that your ambitions in this area match the level you are working at. But it is important to remember that whether you are testing or generating theory or not, you will probably be expected to link your research to theoretical ideas in your field. Because of this, it is worth getting to know some of the theories that are used in the area in which you are working, even if you don’t plan to test them or generate any new ones.

**Key Points**

- Theory can provide a good starting point for research.
- Some research aims to test existing theories and other studies aim to generate new theoretical ideas.
- Testing theories is not restricted to ‘quantitative’ research and ‘qualitative’ approaches are not always necessary to generate theory.
- Both generating and testing theory can be challenging and you should be cautious about trying either if you are an inexperienced researcher.
- It may be a requirement of your assessment that you link your research to theory even if you don’t intend to generate or test theory.

**Practical Stimuli for Research Questions**

As mentioned earlier in this chapter, ideas for research can emerge from practical rather than theoretical concerns. This is often the case for practitioner-researchers who want to research issues directly related to their work. Students and researchers who are interested in public policy may also be curious about how particular pieces of legislation work in practice. Both policy and practice can stimulate interest in particular areas or topics, and can in turn generate interesting research questions. In this section we look at ‘practical’ stimuli for research questions in more detail.
Using Policy to Generate Research Ideas

Policy can provide a useful starting point for generating ideas for research. Libraries have traditionally held important policy documents and nowadays it is relatively simple to find recent and historical records online. Policy research is often focused on the extent to which legislation has helped tackle particular ‘social problems’, and so its relevance to wider issues is usually clear (Greer 1978, Bulmer 1979, Robson 1993). Because it addresses issues perceived as important by politicians, officials and/or the public, research of this kind is relatively easy to justify, which is an important consideration if you are a student applying for postgraduate scholarships, a practitioner seeking funding or study time from your employer, or a professional academic bidding for a research grant.

While policy research is often concerned with legislation enacted at a national level by government departments, it is important to remember that policy is also made at regional, local and institutional levels. Policy formulated and implemented at any level can provide ideas for interesting research questions, and if you have limited time and resources you may find that policies applied on a relatively small scale provide the best opportunities to conduct a manageably sized study.

For example, I was involved in a research project researching school admissions policies operating at the national, local authority and institutional levels. It was possible to investigate all three of these contexts because the research was externally funded, well resourced and conducted over a number of years. However, any one of these contexts would have provided enough scope for an undergraduate or master’s level study. In fact, the policy documents themselves contained sufficient data for a very interesting small-scale study and an early paper from this project was based on just such an analysis (see White et al. 2001).

Social Problems as Sources of Research Questions

As policies are often concerned with addressing particular aspects of social problems, it’s not surprising that the two are frequently discussed together. Social problems can provide a rich source of ideas for research questions, whether they are related to particular policy initiatives or not. Marshall and Rossman (1999) suggest that investigating the ‘incidence and persistence’ of a problem can be particularly valuable and Robson (1993) points out that social problems are ideal for generating research questions because, unfortunately, they are never in short supply. The outcomes of research into social problems almost always have implications for future policy-making at many levels, can add to our understanding of a topic and even contribute to the development of theories.
A useful indicator of how topical policies and social problems are is the amount of coverage they receive in the media. Browsing broadsheet newspapers can be a good strategy for students looking for ideas for a research project, as the most topical policies and problems will usually be discussed in some detail. There are also journals such as *Health Policy*, *Criminology and Public Policy* and the *Journal of Education Policy* that deal specifically with policy-oriented research. These journals cover a wide range of policy-related issues in a particular field whereas more specialist publications such as *Substance Abuse Prevention Treatment and Policy*, *Telecommunications Policy* and *Sexuality Research and Social Policy* focus on specific issues. These journals can give you an idea of what policies are currently being discussed by researchers in a particular area.

Another type of journal concentrates on social problems and related issues. Again, these range from publications that are wide in their scope such as *Social Problems*, *Journal of Social Issues*, *Australian Journal of Social Issues* and *Social Issues and Policy Review* to those focusing on more specific areas: *Women’s Health Issues*, *Current Issues in Tourism*, *Journal of Drug Issues*, *Journal of Family Issues* and *Journal of Sport and Social Issues*.

Simply because of the enormous amount of information available in the area, policy can be a good starting point for research topics. Social problems and issues can be just as useful for generating research ideas, and the research they stimulate can be easily defended in terms of its relevance and importance. Do bear in mind, however, that the process of academic publication is relatively slow and articles are usually published many months after being submitted, even in journals that provide pre-publication online access. This doesn’t mean they won’t be useful for stimulating research questions, only that they may not represent the current ‘cutting edge’ of debate in an area. Media coverage and policy documents themselves can often provide a much more current picture of recent developments.

**Linking Research to Policy and Social Issues**

Whatever the starting point for your research, you should not ignore the potential implications of your findings for social problems and related policies. Research is never conducted in a ‘policy vacuum’ and most research can be linked in some way to social problems and issues (Pole & Lampard 2002). In the same way as theory can be relevant to even the most practically focused study, there can be unexpected implications for policy or social problems arising from all kinds of research (Hammersley & Atkinson 1995). Linking your research project to contemporary policy and topical issues will usually increase outside interest in your study, even if your research questions originated elsewhere.
Another area that can generate research questions, and one closely linked to policy and social problems, focuses on the practice of professionals working in various settings. These types of question, and the research that attempts to answer them, are often characterised as ‘applied’ in nature. I discuss these in more detail in the next section.

**Key Points**

- Research questions can originate in practical concerns as well as in the academic literature.
- Policy formulated at all levels can provide ideas for research questions, and policy documents themselves can be used as data.
- ‘Social problems’ are a very good source of ideas for research projects.
- Media coverage and the content of specialist journals can both be used as indicators of current concerns about policy and social problems.
- Academic journals dedicated to policy or social issues can also be useful but be aware that articles appear many months (or even years) after they are written.

**Applied Research**

‘Applied’ is a term used to describe several different approaches to research, including action research, evaluation research and practitioner research. Applied research questions often originate in the world of professional practice, rather than in academic settings, and tend to be more concerned with addressing immediate, practical problems than contributing to wider social scientific debates. These types of question often relate to particular organisational or institutional contexts and can be concerned primarily with specific sites or populations. ‘Applied research’ is common in areas such as health, education, social work and criminal justice, although it can be carried out anywhere practical issues arise.

Personal experience often plays a central role in generating research ideas and this is particularly the case in applied settings. Ideas for applied research often come from practitioners themselves, even when they are not directly involved in conducting the research. If you are a practitioner interested in conducting research, then examining current practice or beliefs about ‘good practice’ or ‘what works’ in your profession or institution can spark ideas for research questions that will be both interesting to you and relevant to your work.

If you are interested in conducting applied research but are unfamiliar with the problems encountered in a practice setting, it may be useful to seek advice
from those with practical experience in the field. It’s important to have input from the relevant professionals, who should be involved in the process of formulating research questions and preferably consulted regularly throughout the research process (Soydan 2002). If you were conducting research into the careers of persistent offenders, for example, it may be necessary to consult with police officers, legal professionals and staff from the prison and probation services so you can find out what they think are the most important problems to be investigated. Reading the academic research literature does not always prepare you for the realities of the setting you want to research.

There is a tendency for applied research to be viewed as a less prestigious activity than ‘pure’ or ‘theoretical’ research, especially in academic circles. This is not just the case in social science but also in the natural sciences. The experienced academics interviewed as part of Campbell et al.’s (1982) study certainly believed this to be a common opinion, but few took this view themselves, with most arguing that too little applied research was actually being conducted at that time. Try not to be put off by negative views about the status of applied research; using research to address practical problems and issues is a very valuable activity. There will never be a shortage of problems to address and there is often more funding available, to both students and professional researchers, to conduct applied research.

Key Points

- Applied research can be very valuable and should not be seen as inferior to more theoretically focused work.
- If you are a practitioner, you can use your own professional experience as a source of relevant and important research questions.
- If you don’t have practical experience of a particular setting, discussing your research with professionals in the area will be useful.

The Relationship between Research Questions and the Literature Review: The ‘Literature Funnel’

In the first part of this chapter I explained the relationship between research questions and the various forms of literature that can help generate, develop and inform them. One of the things my students often find difficult, however, is showing the relationship between their questions and the wider literature when they write up their research. Providing detailed advice on how to conduct a literature search and write up a literature review is beyond the scope of this text and there are many
Where Do Research Questions Come From?

books available that are dedicated to this subject. However, thinking and writing about research questions, combined with the experience of supervising students’ research projects, led me to develop the following model of the relationship between the literature review and research questions.

Figure 1.1 shows what I call the ‘literature funnel’. This is a diagram I drew on the whiteboard in my office when explaining to students how their research questions related to their literature review and how their literature review should be structured. I have changed it slightly over the years but the essential ideas underlying it have stayed the same.

At the bottom of the literature funnel are your research questions. I advise my students to always present their research questions at the end of the literature review. In a well-written literature review they should appear as if they naturally arise from the discussions that precede them. In other words, the literature review should lead to the research questions being asked. Structuring your literature review in this way leads the reader through a ‘narrative’ that explains why your questions are important and how they fit into wider debates and relevant research findings, before presenting the research questions themselves at the end. Having clearly established the research questions, you have then prepared the reader to evaluate your methods, findings and conclusions presented in the rest of the research report.

Looking at the ‘funnel’ – the grey shaded area in the centre of the diagram – you will see that there are three elements, linked by two arrows that point downwards. We have already seen that the ‘specific research questions’ are at the end of this chain. Above these are ‘key issues, debates and findings in the area’. This represents the part of the literature review that deals with studies that are directly
relevant to the research questions you want to address. By reviewing the literature you can find out what is already known in the area you are studying and either identify particular questions that have yet to be fully addressed or explain how your research will fit with what is already known.

Above this middle section, at the top of the funnel, is a section called ‘wider social and research context’. This is where you link your research topic to relevant theoretical, policy and practical issues. This part of your literature review places your study in a broader context and shows that it is important not only because it addresses particular questions but also because the answers to those questions have implications for wider issues. This is also where you would link your study to recent or important policy legislation, practical issues, and theoretical ideas and debates.

The arrows linking these three elements show the direction in which your literature review should proceed. You start by introducing the context of your research and showing its importance by linking it to a discussion of wider debates and issues. Next you concentrate on the literature that directly relates to your topic and then show how answering your research questions could contribute to this literature.

To the sides of the diagram there are two other arrows, running downwards from the beginning of the literature review to the end. These indicate an increase in focus and detail as the literature review progresses. As the funnel in the centre of the diagram suggests, your literature review should narrow in focus as it progresses, going from wider contextual issues, through the research on the topic you have chosen to focus on, to your specific research questions. As your review progresses, the detail should also increase. This means that you will spend less time discussing the more general issues at the beginning of the review but that the detail and depth of discussion will increase the closer you get to your specific research questions.

This is very unlikely to provide a representation of how you actually conducted your literature search and the order in which you reviewed the literature. As I mentioned earlier, this is likely to have been a much messier and more iterative process. Rather, this is a way of structuring the presentation of your literature review so that it is led by, and leads to, your research questions. It moves from a general, but brief, discussion of the wider issues and context, through what we already know in an area, to a consideration of the very specific questions that are still unanswered and will be addressed in the project.

The literature funnel is meant to act as a guide to help you present your literature review in a way that places your research questions at the end of a discussion that leads to them in a structured and logical way. It tells the reader what we do and don’t know about a particular subject, and explains why it is important to answer particular questions. It hopefully also helps you understand more clearly why we
conduct a literature review in the first place – something that is not always obvious to all new researchers.

**Examples of Generating Research Questions**

Having already discussed the generation of research questions in general terms, in the last section in this chapter I illustrate this process by showing some examples. I start by suggesting some resources that can be used to help you identify a topic or issue that could form the basis of a research project. Following this, I show you how to move from a topic or area of interest to a set of research questions that will guide your research design and data analysis.

**Using the Media as a Source of Ideas**

There are several ways you can generate ideas for research topics if you can’t think of an area to study straightaway. Looking through newspapers and magazines can be helpful, as it will give you an idea of the kinds of issue that are currently topical. Most serious broadsheet newspapers (such as *The New York Times*, *The Globe and Mail*, *The Guardian*, *Le Monde*, *The Australian* or *Die Welt*) provide coverage of social policy and social problems, and also have sections dedicated to particular topics such as education, media, society, health, politics and business. These sections provide in-depth coverage of issues in these areas and can be very helpful in generating research ideas. Magazines covering social issues, such as *Time*, *Maclean’s*, *Der Spiegel*, *New Statesman*, *The Economist* and *Le Nouvel Observateur* can also serve the same purpose. As many newspapers and magazines are free to access online, the best place to start is on their websites, which allow you to browse and search for not only recent stories but also older coverage. Many of these websites also offer content that is exclusively available online and you can follow up any interesting issues via suggested links or by using the search tools. Any relevant articles can also be used in your literature review to demonstrate that your research relates directly to contemporary concerns.

Watching the television news, current affairs programmes or documentaries can also alert you to topical issues that could raise interesting research questions. Stations such as *Aljazeera*, *BBC News*, *CBSN* and *CBC News* cover both national and international news and have accompanying websites. Radio broadcasts on stations such as the *BBC World Service* or *NPR* provide audio reports but their websites also have multimedia resources. Your library should have guidance on how to cite media sources appropriately for the referencing system required by your institution.
Before moving on to look at some examples of how media news reports can generate ideas for research questions, it’s important to point out that although these reports can be very useful for generating ideas and demonstrating that an issue is topical, they are a different kind of literature from research reports and articles published in peer-reviewed academic journals. Media reports of statistics and other research findings are not always reliable (see Paulos 1997, Best 2001 for examples) and, once you have identified a topic you are interested in, it’s essential that you thoroughly investigate any claims by reviewing the relevant research literature.

So how exactly can media reports help you generate ideas for research questions? In this section we will look at two news stories that I found on two different newspaper websites: The Guardian (UK) and The New York Times (US). I will show how thinking about the issues raised in the articles can lead to ideas for research topics and how these research topics can be developed into suitable research questions. Links to the original articles are provided at the end of the chapter if you wish to read them in full.

Example 1: ‘Automatic’ Voter Registration

The first news story I selected relates to the issue of voter registration and was published on The Guardian website on 17 March 2015 (Pilkington 2015). It reported that in the state of Oregon, in the United States, information held by the Department of Motor Vehicles (DMV) would be used to automatically register citizens who were not already on the electoral role. As a result, up to 40,000 new voters will be registered in that state. This legislation also raises the possibility that such initiatives could be extended to other states and that other sources of information could be used to automatically register citizens who are eligible to vote.

There are many interesting issues relating to voting behaviour and it’s a topic that is researched extensively by political scientists. As I explain later, however, this doesn’t mean that there aren’t any opportunities for new and innovative research. When starting to look for ideas for research topics, try not to worry too much about how much research has already been conducted in the area. You’ll be able to work out your own ‘angle’ on a particular topic once you have a clearer idea of the existing literature.

The most obvious issue raised by this news story is voter turnout. In some countries – such as Australia, Brazil and Singapore – voting is compulsory but in most countries it is voluntary and turnout can have an important influence on election results. Some questions raised by this news story are:
Where Do Research Questions Come From?

1. To what extent does turnout at elections vary:
   (a) over time?
   (b) between types of election?
   (c) between countries or regions?
   (d) according to the social background of voters?

Other questions might be:

2. What reasons do people give for:
   (a) not registering to vote?
   (b) not voting even when registered?

Another question might take a slightly different angle:

3. What impact do increasing levels of voter registration have on voter turnout?

These questions are a few of many that could have been generated by thinking about the issue of voter turnout, and they would all be good starting points for a research project. They would provide sufficient direction for you to start your literature review, during which you could work out what research has already been conducted and where the opportunities for new research lie. They would almost certainly not be your final research questions but could be developed and refined (or even abandoned) depending on what you discover when reading around the topic. If you have found a topic covered in a newspaper report interesting then it is likely that you will also be interested in the wider issues and debates in this area. Finding a news report on a topic that grabs your attention may seem like a small step but starting with little idea about what to research and getting to this point represents considerable progress. Discovering an initial area of interest means that you can start to engage with the literature and think about the next stage of your study.

News stories often cover topics that raise more than one issue and it’s important to be aware of all the possible questions raised by a report. The article on voter registration also relates to the use of administrative data by governments, private companies and other organisations. As many governments and inter-governmental institutions have legislation on both data protection and freedom of information, this is an area of interest for political scientists, sociologists, criminologists and lawyers. Possible research questions in this area could include:

1. To what extent are members of the public aware of:
   (a) the personal information about them held by:
      i. governmental organisations and private sector institutions?
      ii. businesses and commercial enterprises?
Examples of Generating Research Questions

(b) the uses to which this information can be put?
(c) their rights to access details of any personal information held by these organisations?

2. To what extent does their awareness of:
   (a) the type of information,
   (b) the uses it can be put to and
   (c) their rights to access details of it vary according to their social, educational and economic backgrounds?

Again, these questions just serve as a starting point but they would be sufficient to direct you to literature in the area and enable you to start reviewing existing research and commentaries. As before, the research questions will doubtless require some development but they will ‘kick-start’ a project and move it towards the next stage.

Example 2: Attendance at Women’s Sporting Events

The second example was taken from *The New York Times* website on 23 March 2015 (Harms 2015) and focuses on the low attendance at women’s college basketball games in the United States. Although no direct comparisons were made with attendance at men’s games, figures from the National Collegiate Athletic Association (NCAA) website show that in the 2014 season men’s games attracted more than three times the number of attendees than women’s games (NCAA 2015a, 2015b). Differential participation in sport – whether as a participant or as a spectator – is an area of concern for international sporting bodies, government departments and other interest groups, and has attracted a great deal of attention from researchers.

Because of the breadth of this area, there are a huge number of possibilities for research. Although the newspaper article focused on spectators, and this would be an interesting area of research, it could also lead to questions about playing sport. Or a study could focus on physical activity more widely, either at a general level, or in relation to particular pursuits. The focus could be on activity levels and relate to concerns about health or it might concentrate on the social aspects of participation in sport and exercise.

The article looked at sport played by women and one angle could be comparing men’s and women’s sport. But social researchers are interested in differences between other groups and your research might instead focus on ethnic groups, age groups or any combination of background characteristics.
This single article has raised many different possibilities for research into participation in sport or physical activities, sports spectators or leisure pursuits more generally. Potential research projects might focus on debates on health, equal access to resources or social aspects of leisure. Or a study could focus on participation by sex, social class, ethnicity, age or another variable relating to either watching or taking part in sport or leisure activities. As you can see, one quite specific article can easily generate lots of ideas for research if you spend a little time thinking about the issues it raises.

Going back to the original article, some possible research questions are:

1. What are the differences in spectator attendance between women’s and men’s basketball matches?
2. To what extent have these differences changed over time?
3. To what extent do these differences vary according to the level of competition?

As is explained further in Chapter 2, these are descriptive questions. They ask about what is happening rather than why something is happening. It is usually necessary to ask descriptive questions first – we need to know what is happening before we can ask why it is happening – but these are often followed by explanatory questions such as those below:

4. What reasons do basketball spectators give for:
   (a) attending men’s matches?
   (b) attending women’s matches?
   (c) not attending men’s matches?
   (d) not attending women’s matches?

These questions follow from the first three, as once we have found out what is happening the next step is to try to explain it. It only makes sense to try to explain differences in men’s and women’s attendance if there actually is a difference. In Chapter 4 I explain the implications of different types of question for the kinds of data needed to answer them and the research designs needed to produce these data.

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**Using Your Studies to Generate Ideas for Research**

Other sources of ideas for research are the courses, modules and classes you have previously taken or are currently studying. Going through your course handbook, lecture notes or recommended texts could spark your interest in a particular topic.
 Asking your lecturers or seminar tutors may also be helpful. They will be knowledgeable about current debates in an area and should also have a clear idea about what you’ll be able to achieve given the time you have available.

 I teach on an undergraduate module called ‘Education and Social Justice’. On this module we examine the role of education through the life course and focus on participation in education and training, and educational attainment and outcomes. Students taking this module have often chosen to conduct research in the area of education for their undergraduate research projects and use the topics covered in the module as starting points. One of my supervisees was interested in the difference in career trajectories between people who had been educated at state-funded schools and those who had attended fee-paying schools. Her initial research question was:

 How do the careers of privately educated students compare with those educated in state-funded schools?

 The advantage for this student of drawing on topics covered in a module she was already studying was that she was already aware of many of the issues and some of the literature in the area, and could use both her teachers and the module reading list for guidance on further reading. Although you may be keen to conduct research on a topic that is new and unfamiliar to you, it’s worth bearing in mind that using your existing knowledge to generate ideas for your study can give you a useful head start.

 Using Personal Experience to Generate Research Questions

 As I discussed earlier, professionals and practitioners often draw on their own experience in the workplace when thinking about ideas for research. Through working in a particular career or profession, they may have become very familiar with the important issues and questions in their field.

 Other personal experiences – perhaps relating to your family, health, education or leisure – can also provide ideas for research. It doesn’t really matter where your initial idea comes from, as long as you can turn it into research questions that are relevant to the discipline you study. Not all of the ideas arising from reflecting on your own experiences will lead to good research questions but the two examples below show how two students I supervised came up with their ideas for research.

 One of my former PhD students was a nurse educator who had noticed that some of his students’ attitudes to the abuse of legal and illegal drugs appeared to change
Where Do Research Questions Come From?

over the course of their training. He was curious about whether these changes were related to their specific experiences as trainee nurses or whether they were due to other factors, such their experience of going to university or just getting older. This led to him formulating the following initial research questions:

1. What attitudes do student nurses hold towards illicit drug use at the start of their training?
2. How do these attitudes compare with other health and social care students?
3. To what extent do student nurses’ attitudes towards illicit drug use change over the course of their training?
4. Are similar changes evident among other health and social care students?
5. To what extent are changes in the attitudes of student nurses linked to specific experiences of nurse education and training?

These questions not only provided this student with some focus for his initial literature search but also suggested that he would need a particular kind of research design for his study (see Chapter 4). But the original idea for the study came from his personal experience rather than from anything he had read or been taught.

Another PhD student had previously worked in a further education college, preparing mature learners for entry to university. As a result of her experiences, she wanted to find out whether older learners’ experiences of, and outcomes from, higher education differed from those aged between 18 and 21 years. In her proposal she included the following questions:

1. To what extent does the attainment of undergraduate mature learners differ from that of 18 to 21-year-old students?
2. To what extent do barriers to learning in higher education differ depending on the age of learners?
3. To what extent does the age at which students graduate affect their opportunities for employment?

These questions are the ones this student developed writing her research proposal. They changed slightly over the course of her study and she ended up focusing more on students’ experiences than their outcomes. The original questions, however, did provide her with a good starting point to review the literature in the area and think about the kind of research design she would need to answer them.

It’s important that any questions that arise from your own experiences be seen as starting points, rather than finished products. Regardless of your experience and knowledge in a particular area, you’re unlikely to be aware of all the relevant literature and, even if you’re already familiar with some of the important issues,
you will still need to think about what data you need to answer your questions (or even whether your questions can be answered with research). Being clear about the focus of an investigation is certainly a good thing but there is still a lot of work to do before you can start to collect your data. Research questions often need to be refined, and your study needs to be located in relation to the existing theoretical ideas, substantive findings and debates about policy and practice. As I explained earlier in this chapter, such links can only usually be made by thoroughly reviewing the literature in these areas.

Key Points

- Media reports, course notes, discussions with academic staff and personal experience can all help you generate initial ideas for a research topic.
- Your literature search will be much more difficult if you start it without some idea of the topic you want to investigate.
- Even if you have very clear ideas about the direction of your research, you will still need to link your study to existing ideas, debates and findings.

Summary

If you’ve read this whole chapter, you’ll hopefully have learned something about where research questions come from and the roles different sources can play in generating research ideas. I started the chapter by highlighting the importance of both ‘disciplined curiosity’ and the capacity to be surprised by what you find out. As the quotation at the very beginning of the chapter suggests, if you are genuinely curious you should be able to get ideas for research simply by looking in the right places. Throughout this chapter I have suggested ways in which you can generate and refine research questions by using the existing literature and other sources of information, and I’ve used examples to show what kinds of question can arise from these sources. Even if these examples have not been in areas you are interested in, I hope they have given you some idea of how questions can be generated.

This chapter has also tried to show you how the wider literature relates to research questions: in generating ideas for questions; developing these questions; and also linking your questions to the literature when you write up your research. Depending on where you are with your research questions, you should now be able to find the best place to start reading.

In the next chapter I look in detail at the form and content of research questions. It is best to have at least some idea about a topic or area of interest before you start
reading Chapter 2. If you have got to this stage you’ll be able to follow some of the advice I provide to turn your initial ideas into research questions, or to refine the questions you already have.

**Exercises for Readers**

Each of the following three sets of exercises is aimed at readers who are at different stages of developing ideas for research. Each subsequent set presumes a slightly clearer and more focused topic or question. Start at the level that suits you best and progress to the next one when you have finished.

If you have no idea about what you want to research:

1. Look through the news reports and opinion pieces published on three or four news media websites. Try to identify two or three areas or topics that interest you.
2. Think about what questions or issues these reports raise. Try to list as many questions or issues as you can fit onto one page.
3. Try to pick one topic, issue or broad question from this list to focus on in your study.

If you have already identified a topic, issue or broad question:

1. Start to search for literature by using a search engine or online bibliographic database. Develop a list of literature in the area.
2. Try to identify and locate journals, textbooks, readers or handbooks that cover the topic you are interested in.
3. Use the titles, abstracts and introductions to these publications to identify the key themes and ideas in the area you are interested in.

If you already have specific research questions:

1. Look for specialist publications that focus on the topic you are interested in. These are most likely to be journals or edited collections.
2. Try to establish what is already known in your area of interest. Think about where your questions fit into the wider literature.
3. If necessary, modify your questions based on what you have learned from your reading.
Further Reading

Locke et al. provide detailed advice on reading and reviewing research reports. Although there are several texts available in this area, in my view this is by far the most useful:


Paulos and Best provide examples of how media reports can be misleading and advice on how to critically evaluate their claims:


Marx provides a useful list of potential sources of research questions:


Media Reports Used as Examples in This Chapter


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