

# DEVELOP MENT FIELD WORK



**A PRACTICAL GUIDE**

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## DESIGNING DEVELOPMENT RESEARCH

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Good research requires good design, yet the process of research design can be fraught with difficulties and frustration. Fortunately, there is a wide literature on the principles of research design in social and economic studies that offers both theoretical and practical guidance (Blaxter et al., 2006; Denscombe, 2010; Kitchin and Tate, 2000; O’Leary, 2010; Robinson, 1998). For research design advice specific to development research, see Desai and Potter (2006) or Sumner and Tribe (2008a). Research design is an enormous theme. It covers three broad overlapping areas that are crucial to the genesis and initiation of a viable and relevant research topic. First, research *philosophy* covers issues of ontology (theories of what the world is) and epistemology (theories of what it is possible to know about the world and how we might come to know it). Second, research philosophy flows into *methodologies* (theories of how the world can be interpreted) and methods (sets of techniques for interpreting the world). Third, and crucially, ‘design’ also incorporates issues of research *logistics and practice* which include site selection, proposal writing, research timing, budgetary issues, and planning for ethical research.

For new researchers the design phase is often daunting, coming at the beginning of the investigation period with the expectation that it should be made watertight, within a neat timeframe, before flying off to ‘do’ the real research. This conception of the process can be unhelpful. Design is a fundamental and integral part of actually *doing* research, and in most social research at least, design is likely to evolve as the subsequent phases of the project unfold and the perspectives of the researcher almost invariably shift. This does not imply that design should not be thoroughly worked-out before ‘fieldwork’ begins – it should. Using a ‘building’ analogy, Kitchin and Tate echo this point:

We are moving from the choice of what type of building we want to construct to decisions regarding the process of construction. To take the analogy further, if we miss out this stage and

progress straight to constructing the building without adequate planning then there is a good chance we will run into problems at a later date. (2000: 34)

It is important to reiterate, however, that design should be seen as an essential part of the *on-going* research process requiring, as does every other component of research activity, flexibility and reflexivity. To extend the analogy, this means that you may have to be prepared for some walls to falter and buckle as the ground shifts during construction. Ultimately, it is the balance between rigidity and flexibility that is likely to determine the success or otherwise of the project. Those who live in earthquake zones know well that architecture that is too rigid can be disastrous.

Given the wide literature that already exists, why include a chapter on design in a book such as this? We argue that a range of issues particular to development research design often sets it apart from design in other areas of social science. Although all research is built from fundamentally similar (albeit highly contested) foundations, it is important to understand something of the *difference* of development field research.

## WHAT MAKES DEVELOPMENT RESEARCH DIFFERENT?

A number of points can be offered in order to support our claim that development research is somehow different (Laws, 2003; Sumner and Tribe, 2008a). First, research often takes place in localities and cultures that are relatively unfamiliar to the researcher. This is not always the case, of course, and ‘foreignness’ lies on a continuum which is influenced by cultural, life-cycle, gender and geographical factors. Consider a female Londoner from high-income Hampstead Heath conducting research into male rural labour markets in low-income Herefordshire in the United Kingdom – is this any less ‘foreign’ than an urban New Zealander doing urban research in the Philippines? In practical terms, however, it is often the case that both the territorial geographies and cultural traits of the research ‘site’ are relatively unknown to the development researcher. Related to the first point is a second, which concerns language. Despite the rapid globalisation of English, it is likely that the researcher will undertake his or her work in a foreign or second language. Particularly if the research is socio-cultural in nature, it can be argued that without a high level of proficiency, or excellent assistance, whole worlds will remain unexplored, misinterpreted and, ultimately, poorly conveyed. Third, development research by researchers from Western institutions often necessarily involves a discrete period of research activity in the field with little chance of returning to ‘fill the gaps’. Fourth, as Sidaway (1992) and Abbott (2006) remind us, development researchers from the ‘first world’ will often enter local society further up the hierarchy than their respective position at ‘home’ – it has been increasingly argued that the consideration of this should not only influence the practice of doing research, but should also be explicitly fed into design (see Chapter 9 and 10).

In the increasingly common case of foreign students from developing countries doing home-based development research (see, e.g., Chacko, 2004; Mandiyanike, 2009; Ng, 2011; Subedi, 2006; Sultana, 2007), some of the above discussion remains pertinent.

Doing development research away from home is distinct to doing research on one's own society and culture while based there indefinitely. For example, the researcher is likely to have a discrete fieldwork period available and will thus face all the same problems of limited piloting, distance from supervisors whilst in the field, and the perils of the return phase. Also, if the student is working in a Western university, this implies that they are relatively privileged. Even if this is not the case, it is likely to be perceived as such during fieldwork and will undoubtedly influence data collection and the outcome of research. Finally, the foreign student doing home-based research is likely – or may be bound – to take something of the philosophical and methodological baggage picked up from the foreign institutions and supervisors with whom they are working. Just as the distinction between 'foreign' and 'home-based' research is far from watertight, research can never be free of 'external' influences however 'local' it may appear.

This chapter makes the case that successful development research, whilst not inherently different from social research in general, does require a special set of skills and sensitivities. The development researcher needs to be more eclectic than is the case with research in more familiar terrains, more sensitive to cultural and ethical issues, and more willing to re-design research strategy as the research project evolves. Nevertheless, there are a range of generic issues in social science research design, which we deal with first.

## DESIGN: THE IDEAL FIELD RESEARCH PROJECT

The business of research design is about putting philosophy into practice and operationalising ways of exploring theoretical ideas. As the 'bridge' between the conceptual and the logistical, it involves both abstract and practical issues and the lines between them are not always clearly demarcated. The following section touches on both of these areas, moving in general from philosophy to practice. We are mindful that the reader may well be embarking on his or her first research project, and thus we have attempted to keep the following as jargon-free as possible. For those who wish to pursue these generic themes in greater depth, please refer to the reading list at the end of the chapter and follow up the references used in the text.

## IS PHILOSOPHY IMPORTANT TO RESEARCH?

Before beginning your design it is worth considering the philosophy and nature of research in general and the various types of research that can be undertaken. You may wonder, understandably, what on earth philosophy has to do with the practical business of undertaking a sound field research project. Indeed some people do ignore such questions – but always at their peril. There is a common misconception outside of academia that research is a value-free, objective process, often undertaken by men and women with white coats, thick-rimmed glasses and untidy hair. Whilst this may be true of some types of research, and perhaps some researchers, such

generalisations are less relevant in the postmodern world. In the social sciences and Development Studies in particular, there has been a flowering in the range of alternative philosophies and methodologies resulting partly from the ‘cultural turn’ of the 1980s (see Harriss, 2005; Sumner and Tribe, 2008b). Lamentably, research and the term ‘science’ have long been colonised by one approach which has assumed the mantle of ‘scientific-method’: the empirical-analytical perspective. This approach, generally built on positivistic epistemological assumptions, is more often associated with the natural sciences. However, an on-going debate concerning whether or not social science can be approached using similar philosophies and methods (the naturalist/anti-naturalist debate) continues to rage. As with all examples of contesting paradigms (Kuhn, 1970; Lakatos and Musgrave, 1970), proponents of opposing factions are active in research in Development Studies, often in the same departments! It has to be said that there is now widespread recognition that social science is somehow different and requires differentiated foundations and tools from the natural sciences (see Sayer, 1988 for the classic argument in this regard).

In fact, there are a number of different types of science. It is important to have a basic grasp of these essentially different ‘worldviews’ as no research can take place in a philosophical vacuum. It is important to know something about where you fit in as this makes design, practice and the defence of your arguments far easier. In this context Graham argues:

Philosophy is to research as grammar is to language, whether we immediately recognise it or not. Just as we cannot speak a language successfully without following certain grammatical rules, so we cannot conduct a successful piece of research without making certain philosophical choices. (2005: 10)

## DIFFERENT TYPES OF SCIENCE AND DEVELOPMENT STUDIES

Habermas (1978) divides science into three types; *empirical-analytical*, *historical-hermeneutic* and *critical* (see Box 2.1). Each of these branches is host to a range of approaches, and it is perhaps best to see the three as lying on a continuum, given the considerable overlap that exists. The first is composed of approaches where it is largely believed that facts speak for themselves, that science should concern itself with observable entities and that there is no room for ‘normative’ or value judgement based research.<sup>1</sup> The most influential branch of this approach is ‘positivism’, which seeks to verify or falsify propositions through the collection of empirical data and argues for the construction of laws based on its findings. It is this branch of science that has come to dominate the public imagination and, to some erroneously, define intellectual endeavour or progress.

Historical-hermeneutic science lies at the other end of the spectrum and rejects the empirical view of the world. Facts do not exist independently of experience and individual perception is paramount. As such, outcomes are not predictable, laws are not derivable and the objective becomes the *interpretation* of patterns and processes. Examples of such approaches include humanism, phenomenology and, arguably,

postmodernism and post-structuralism (see Kitchin and Tate, 2000 for a useful summary of these approaches).

In between these two views lie critical sciences, of which Marxism, realism and (some types of) feminism are three very different examples (see Johnston and Sidaway, 2010). What these approaches have in common – although some would argue that the differences are greater than the similarities – is that they have a moral dimension. The purpose of critical research is to uncover non-explicit processes and relations (including the nature of previous research findings) and communicate these to people so that they may act upon them in order to improve society, a process referred to as ‘emancipation’.

## Box 2.1 Different types of science

### Empirical-analytical

*Essential elements:* Facts speak for themselves; science should seek facts about observable objects; normative and moral questions are avoided as they cannot be measured scientifically; proposes that processes and patterns can be predicted.

*Most common methods:* Surveys, closed questionnaires, some cartographic analysis, and secondary data can be important, although primary data is central also.

*Development Studies example:* The availability of public services and the types of building materials used in urban squatter settlements in Fiji.

### Historical-hermeneutic

*Essential elements:* Rejects the empirical view of the world; facts do not exist independently of experience; interpretation of process and pattern rather than prediction.

*Common methods:* Interviews, open questionnaires, visual texts, participatory methods including participant observation and ethnography; primary data generally more important.

*Development Studies example:* Perception of land tenure security by residents of squatter settlements in Fiji.

### Critical

*Essential elements:* The uncovering of non-explicit processes and relations; the communication of these findings to promote progressive social change; the explicit incorporation of moral questions.

*Main approaches:* A broad range of methods are used depending on the nature of the critical science being employed. Mixed methods are often appropriate for such studies.

*Development Studies example:* Civil society advocacy of the legal rights of the residents of squatter settlements in Fiji.

Changes in Development Studies over the 50 or so years of its disciplinary history illustrate the shifts and tides of changing paradigms in the social sciences in general. Initially, Development Studies was generally more empirical-analytical, especially given its preoccupation with economic growth and modernisation which it was felt could be ‘measured’ in objective ways (Lewis, 1954; McClelland, 1970; Rostow, 1960; Soja, 1968). The 1960s, and in particular the rise of dependency analysis, saw a flourishing of critical approaches in Development Studies – some of which were explicitly action-oriented and policy based (dos Santos, 1970; Frank, 1967; Prebisch, 1962). More recently, development has been contested by more reflexive, explicitly subjective philosophies. The postmodern and post-colonial critiques have been particularly influential in academic (if not policy) circles as totalising strategies and the research used to generate them has been heavily criticised. In particular the ‘value-free’ nature of modernisation research was labelled a façade (Brohman, 1995; Chambers, 1983; Esteva, 1992; Rahnema and Bawtree, 1997). There are, of course, major exceptions to the neat chronology described above: modernisation was often researched from a hermeneutic viewpoint (as in some forms of Anthropology and Sociology); some of the dependency theory of the 1960s and 1970s was based on an empirical-analytical worldview (especially structuralist influenced versions); some of the more recent post-turns are heavily critical, while others are highly conservative given their almost complete relativism (see Corbridge, 2000 for an introductory critique). This illustrates the danger of stereotyping different types of research and different worldviews.

What should you do about all the above philosophical debates? Is it essential that you have your theoretical colours nailed on to the mast before you begin? We would sound a note of warning here. It is possible to get too deeply, sometimes painfully, involved in such considerations. Unless your project is specifically about the application of philosophy, try not to become too tied up in it. The nature of your training to date and your intellectual character will partly pre-determine which of the ‘worldviews’ you most closely identify with. While in the field it will quickly become quite apparent where you best ‘fit’. It is quite possible that you may hold two worldviews at the same time (especially if you believe that opposing worldviews are incommensurable as some postmodernists do), or you may find that the research process changes how you feel about competing philosophies. Very few academics have fully resolved where they sit in this respect and are constantly evolving their theoretical lenses to interpret and understand what they observe. If you think you know the answer in its entirety, then clearly you do not fully understand the question!

## CONSIDERING YOUR POSITION – WHAT KIND OF RESEARCH?

Flowing from the idea of different types of science comes the recognition that there are therefore many different ‘types’ of research in which one can become engaged. There are a number of continuums which are likely to apply to all projects, shown in Box 2.2.



## Box 2.2 Research types

Applied	–	Pure
Descriptive	–	Explanatory
Market	–	Academic
Exploratory	–	Problem-solving
Covert	–	Collaborative
Value-free	–	Action-based
Subjective	–	Objective

Source: Blaxter et al. (1996)

Every individual project will be located somewhere along each of these continuums, making each unique. You may decide that you wish to locate yourself at specific points along these planes. In this case it will be fruitful to consider *positionality* early on. Where are you coming from? Whose side are you on? Are you a pragmatist or an idealist? It is impossible not to have a position and for your individuality to not influence the research process in some way. How does the avowed ‘positivist’ who undertakes a regression analysis of causal variables in economic growth decide which variables to include, and how does he or she arrive at this research topic in the first place? All research, however positivist in appearance, has value-judgements at its root. Edward Soja (1979), for example, has written a stinging self-critique on the application of positivism in his research on modernisation in Kenya.

Considerable academic debate surrounds the issue of positionality and other linked themes, and this has been greatly amplified since it was first considered by feminist researchers in the late 1970s and 1980s (Dear, 1988; Moser, 2008; Nagar and Geiger, 2007; Shope, 2006). Your position, however, may ‘fall-out’ naturally as the research project and the questions you will address become clearer. Despite the myriad possibilities, based to a large extent on the differential philosophical foundations,

the basic characteristics shared by all of these different kinds or views of research is that they are, or aim to be, planned, cautious, systematic and reliable ways of finding out or deepening understanding. (Blaxter et al., 2006)

It should be made very clear that non empirical-analytical science is equally as rigorous as its counterparts. Indeed, it could be argued that in order to convince, other types of science have to be even more rigorous in their analysis as they swim against the popular tide of what is considered really ‘scientific’. This has certainly been the case in Development Studies where modernisation approaches continue to dominate in the policy sphere. Those who adopt ‘alternative’ approaches, beware, as in some ways you have an extra responsibility to be diligent and systematic. There are some excellent examples of research in this vein to which you can turn for guidance,



however; consult your colleagues or supervisor on which examples would be relevant to your particular topic.

## HOW CAN I THINK OF A PROJECT OR TOPIC?

Robson (2011) likens research design to crossing a river, whereby with each step you move between the stones which represent focus, questions, strategy, methods. Before questions or hypotheses can be set up, it is necessary then to cross the first stone and come up with a focus for the study. This period can be both stressful and enjoyable, involving a fair bit of dreaming and drifting through the literature. Some may already have the general area of their project decided for them if they choose to study with a supervisor that only offers postgraduate study in areas of his or her expertise. Although this might seem like an easy option at the time and the student may be flattered that the academic wants to have him or her working on a pet theory or topic, in the long run (and the research period feels like a *very* long run at times) it is not always the best idea. The student should choose a topic that rings bells and sets off fireworks in the mind. Being *very* interested in the matter you are going to invest a significant part of your life in is a bare minimum. How then do you decide on a topic that has these qualities? The suggestions in Box 2.3 may be useful (see also Barrett and Cason, 2010; Desai et al., 2008).

### Box 2.3 Suggestions for thinking of a research topic

- Pick up some of the current development journals such as *Third World Quarterly*, *Development and Change* or *World Development* and see what published researchers are doing. You will be amazed how much of the material that is published comes from postgraduate or post-doctoral work.
- Think of a country you are interested in, and have perhaps studied or travelled in to some extent before. Find a journal for that country or the region in which it is located, like *The Contemporary Pacific*, *Asia Pacific Viewpoint*, *Journal of Modern African Studies* or *Bulletin of Latin American Studies*. Consider what is being studied in the region of your choice and whether it would excite you to do more, or take another angle.
- Talk to people about development issues in the department where you are working or plan to work. Find out what the strengths of the department are if you don't already know them. Talk to both staff and previous students. Try also to talk to people outside the department about what are perceived as the strengths from their vantage point.
- Look at previous postgraduate work that has been deposited in the library. Most departments will store their theses in some form. Build from these ideas, while taking care to distinguish the better quality theses.
- You could consider developing some of your previous research. You may have done an undergraduate research essay or an honours dissertation on a development topic of interest to you.

- You might like to relate it to other interests you have. You may have done charity work, advocacy, or you may have travelled. If you are from a developing country you may have worked for a government department focused on a particular aspect of development.
- Drawing a diagram may also be useful. Place the very general area of interest to you in the centre (e.g. urbanisation) and draw linking topics from it, creating a spider-like effect.
- Consider what puzzles and/or 'bugs' you in terms of development issues. Is there anything that you feel very strongly about? You may take every opportunity to educate friends and family about the moral outrage associated with impacts of European and US protectionism on agricultural trade from developing countries – do a project on some aspect of it then!
- Enjoy the freedom of not knowing exactly what you are doing at this point. This won't always be the case. Don't be scared to follow wild ideas – your supervisor will help you make them manageable. Just dream a little bit.

*Source:* Adapted from Blaxter et al. (2006)

Don't be scared about being confused at the early stages of research: it can even be useful. Reading and thinking widely helps push out our intellectual boundaries and, though it might threaten to confuse and overwhelm you at first, out of that broadness of view, with all its contradictions, blind alleys and unresolved issues, comes a good appreciation of the breadth of your topic and its possibilities.

When deciding what to focus upon, a number of things should be borne in mind. As previously mentioned, your motivation is very important. The project will have to sustain your interest for one or two and possibly many more years, so you need something you are passionate about. It is not hard to find such issues in Development Studies – although we would like to point out that the most unlikely of topics (such as the social and economic impacts of pumpkin exports from Tonga) will often turn out to be fascinating. Simultaneously, there are some development-related topics which become popularised and therefore get a lot of research attention for a time, for example, climate change, eco-tourism or micro-finance. Be wary about jumping onto a 'research bandwagon'.

You will also need to consider the regulations of the department you are working in; the size and manageability of the project in the time period that you have to complete it; the cost and any sources of funding you may need to find; the resources available in and around the department; and your project's demand for support. Finally, and this may be especially relevant for development research, you will need to consider access issues – it may not be automatic that research permits are granted (see Chapter 6). This may be the case if the research is around sensitive issues (see Chapter 10). This is not to say that such issues should be avoided, however, as with all aspects of research a fine balance between pragmatism and idealism needs to be struck.

Robson (2011) offers a useful categorisation of the roots of successful and unsuccessful research – although these ideas will not apply in every case (see Box 2.4). Arguably, research does not have to have 'real world value' to be successful. How is real world value measured? What of pure research which pushes back academic and theoretical boundaries? Look particularly at the roots of unsuccessful research. You

should avoid taking the ‘easy options’ – such as relying on secondary data – as they often involve hidden pitfalls. Resist attempting to build a research project around a method you believe you are particularly adept at; methods are means, not ends.

### Box 2.4 Successful and unsuccessful research

Successful research begins from:

- a) **Activity and involvement** – good and frequent contacts in the field and with colleagues.
- b) **Convergence** – coming together of two or more activities or interest.
- c) **Intuition** – feeling the work is important, timely, right.
- d) **Theory** – concern for theoretical understanding.
- e) **Real world value** – work leading to tangible and useful ideas.

Unsuccessful research begins from:

- a) **Expedience** – undertaken because it is easy, cheap, quick or convenient.
- b) **Method or technique** – using it to try out a specific method or technique.
- c) **Motivation by publication, money or funding** – research done for publication interest rather than interest in the issue.
- d) **Lack of theory** – without this research is easier, but will be of less value.

*Source:* Robson (2011)

## HOW CAN I NARROW IT DOWN?

Thinking widely helps to inspire, extend and contextualise your research topic, but it can also overwhelm you and distract you from conducting well-focused fieldwork. In any research, there comes a time when the exploring of the boundaries has to finish and commitment must be made to a specific topic and design. Such a process is also mirrored in the structure of many research reports.

We can think of a piece of research (and a thesis or academic paper) as being shaped like an hour glass. It starts wide at the top. Here we have a broad scope to explore and encompass existing knowledge: knowledge about the philosophy and methodology of study, the themes for the study, and the region being studied. Out of this, as the hour glass begins to narrow, we should identify gaps or debates in existing knowledge or aspects that you think are inadequate or wrong. These define our key general research questions, which we then refine to develop specific questions that we seek to answer in our research. This takes us to the start of the narrow section of the hour glass, analogous to the focused fieldwork we undertake. This done, and our data collected, we then begin our analysis, at first narrow and specific but gradually widening out to re-address more general issues and debates as we see our contribution to knowledge across a broad base.

There are some ways in which we can try to narrow our ideas down from the general to the specific:

- *Talk*: Talking to others (rather than being buried in our own thoughts) helps to articulate ideas more clearly. Issues that seemed muddy in your mind often become clearer when you have to express them to others.
- *The auntie/uncle sentence*: This is a refinement of the talking strategy. Imagine you are at home with your relatives on holiday and you are asked by Auntie Flo or Uncle Fred ‘What are you studying at university?’. You have one sentence to offer a reply about your research topic. Because Flo/Fred are not academics, you must not use any jargon or language that cannot be understood by a non-expert (‘It is an examination of the socio-psychological parameters underpinning the construction of meta-narratives relating to the incremental impoverishment of a selected sub-section of a marginalised population’) but, because you don’t want to patronise them by offering a glib response (‘I’m going to free the world from poverty’), you must give a sense of what the research is about. It is a tough exercise but well worth doing. If you can construct such a sentence that you are happy with, write it down in large letters, pin it to your wall, show others and keep it in mind throughout your work.
- *Draw a picture*: Just as we suggested above using a diagram to begin to explore a possible project, a drawing can help you identify the key elements of a research project and their linkages. See what the central issue is, what the main components are to support this central issue, and identify linkages amongst them. Your drawing could be a neat box-and-line type or a more free-form doodle that evolves as you add or emphasise different components. Again, if you are happy with the end result, store it away, discuss it and keep it in mind. Such a drawing might even pass as a ‘conceptual framework’ for your project and it can help later to inform the structure of what you write.
- *Ask questions*: Research usually involves finding answers to a series of questions. Sometimes these are big, earth-shattering questions (‘What are the causes of poverty?’); other times they are more simple and specific (‘Why do children in this region suffer from malnutrition more than nearby?’). Think of research as having one central focusing question (the question you really want to answer and the one that will define your contribution to knowledge – this is rather like your auntie/uncle sentence) and a series of secondary questions that you need to answer first if you are to address the main one. See Box 2.5 for some advice in this regard.

### Box 2.5 How to devise clear research questions

1 Once you have a broad idea for a research topic you need to clearly define it. O’Leary provides some useful questions that can help you work out what aspect of your research topic interests you.

- What is your topic?
- What is the context (e.g. geographical location, institution)?
- What do you want to achieve (to explore, to change, to discover, to understand)?

*(Continued)*

(Continued)

- Are there relationships you want to explore (impacts, increases, decreases, relationships, correlations, causes)?
  - What is the nature of your question (what, why, when, where, how or who)?
- 2 When you have answers to some or all of these questions, experiment with putting them together in various configurations. This will give you draft questions to consider.
  - 3 As you come up with research questions, try to answer the 'so what' question: 'Why might your question interest others?' Try using the phrase: *'I am studying \_\_\_ because \_\_\_ in order to help people understand \_\_\_.'* This helps you to see beyond your own interests and to clarify the potential contribution your research could make.
  - 4 You may find that as you work through this process your question becomes increasingly complex. If this happens, break the question into parts. Try to identify a key or main question and list sub-questions.
  - 5 Once you have a research question (or questions) you are happy with, discuss it with peers or a supervisor/mentor and ensure that it:
    - is clear and intelligible;
    - is researchable – overly abstract topics are difficult to turn into research terms;
    - has connections to existing theory and literature; and
    - is not too broad to be practicably researchable, nor too narrow to be significant.

Sources: Booth et al., 2009: 47; O'Leary, 2010: 53; Walliman, 2006: 90

## WHICH METHODS SHOULD I USE?

Having defined your question and the approach you wish to take you will have a range of methods at your disposal (see Chapters 3 and 4, as well as Mayoux, 2006; Moses and Knutsen, 2007 for examples). You need to decide on methods for generating data and methods for analysing the data you produce. Certain methods are often associated with particular approaches; however, there is greater flexibility than some may think. Crang and Cook (2007) argue for a more realistic exploration of these links and question whether particular philosophies necessitate certain methods. It is true that some methods are better suited to some approaches (e.g. textual analysis in hermeneutic research or chi-squared analysis in empirical-analytical studies). This need not be the case at all times, however. In this context Giddens' argues:

However statistical a given study might be, however abstract and remote it is, it presumes some kind of ethnography of individuals involved in the context of what is being described. (1984, quoted in Wolfe, 1989: 71)

There is something of an artificial distinction which has evolved concerning the use of qualitative and quantitative methods – the former for hermeneutic and the latter for empirical-analytical science. There is no reason why methods cannot be mixed; one shouldn't fall into the trap of being qualitative or quantitative and thinking that they are mutually exclusive. This idea of the applicability of mixed methods is taken up in greater detail in Chapter 4.

Chapters 3, 4 and 5 review some of the methods open to researchers, but before making a choice about methods, it is important to remember the place of methods in research design. Methods are a means to an end, not an end in themselves. Your methods must be appropriate to what you seek to discover or answer and they must be appropriate to you as an individual: your abilities, values and preferences. Again, it is important to be flexible in research design. We suggest moving from philosophy to positionality to choice of topic, and then to method. However, in practice, this may be more reflexive. If you are an expert in multi-variate statistical analysis, there may be no point in locking yourself into a path that takes you only to qualitative participant observation as a method. Draw on your strengths, but do not be a prisoner to them.

In considering which methods to adopt, think of research as being like preparing a meal. You may start with an idea, you then explore the cookbooks to see if others have done this before, you eventually settle on a menu and, later, specific recipes. Your menu is like your research design and your recipes are like your methods (bearing in mind that you might favour recipes you have used before but which suit the overall menu). Your recipes then should specify a list of ingredients (your data needs). This shopping list is important: remember that you have defined what you need to get the job done and you do not need to buy up the whole market or cook the same dish six different ways. Yet, when you go to the market, you might find that not all the ingredients are available. This requires some quick thinking: you either have to find acceptable substitutes or, in the worst case, you have to revise your menu.

The market might also reveal some exciting ingredients that you had not thought of but which you can acquire and accommodate within your menu. In cooking and serving your meal, be prepared to make changes: some seasoning added near the end can make a big difference to the taste and good presentation of the meal will make its consumption more pleasurable! In the above, we have outlined in a roughly chronological way the first steps of the research process, flowing from general philosophy to particular methods. In reality, research very rarely follows a linear path and is usually circular, if not spaghetti-like.

## LOGISTICS – PROPOSING AND PLANNING

We have considered overall research design but this broad planning process is not sufficient. There are some critical practical issues that flow from the general design and these, again, require both careful planning as well as a flexible attitude that allows modification (see O'Leary, 2010 Ch. 5; Rossman and Rallis, 2012: 118–128).

One of the most critical aspects of research design is the research proposal. Proposals may be drawn up at different stages and for different purposes (e.g., requesting funds, immigration clearance, ethics approval, PhD programme application or confirmation), and the purpose and timing of the proposal affects the particular shape and length of what is written. Nonetheless, a good proposal must have certain elements (see Box 2.6).

### Box 2.6 Essential components of a good research proposal

- A research title or statement of intent: this should be concise and jargon-free (the auntie/uncle sentence helps here).
- Key research question(s) – see Box 2.5.
- An acknowledgement of the wider literature and issues as they pertain to the topic: what do we know or not know already? Only the key references should be listed.
- The context of the research: the particular region or locality for the research and the way this shapes the topic.
- The methods to be used, including data needs, location, methods of collection and analysis.
- A discussion of ethical issues, and ethics procedures/permissions which may need to be obtained.
- A timetable: when will the main phases of the work be conducted?
- A budget: what are the anticipated costs and sources of income?

Keep the proposal as concise as possible: you are writing a statement of intent, not draft chapters. Unless you are required to produce something more substantial, a proposal should be able to cover the above aspects in 3,000 words or less. However, more specific proposals, for example for an ethical approval process, may require a longer paper or a more specific format. In line with our overall theme about flexibility, treat the research proposal as a working document. It may meet a particular need and summarise your intent at one point in time but it should not put your project in a straightjacket. Review and, if necessary, revise your proposal as you go.

Within the proposal, several logistical issues may need to be covered. Furthermore, such issues may take on particular importance or shape in a piece of Development Studies research. Examples of such issues include:

- *Site selection*: What is the intended location for field research? Choose a site or sites that, from what you can find out, are likely to give you appropriate data. This can be difficult if you do not have first-hand knowledge of the place and you may have to modify your selection once in the field. Bear in mind practical issues of accessibility, health, safety and sustenance (can I find a place to stay?) as well as suitability for the topic of study. Do try to find out as much as possible about health and safety hazards beforehand and either avoid overly hazardous places or take reasonable steps to mitigate the hazards (such as anti-malarial measures or personal safety plans). Always have an emergency plan so you can get help and get to safety from the places you are in.



- *Pre-testing*: Ideally, it is desirable to test and refine your methods in the field before embarking on the full field project. Given the difficulties and expense of travelling overseas to a field site, this is not always possible. In some circumstances, some sort of virtual pre-testing might be tried, for example using friends or, if possible, expatriates from your intended country/region.
- *Language and cultural issues*: Are you going to be able to communicate effectively in the field and behave appropriately? If you have doubts, language courses should be looked at and/or enquiries made to arrange a field assistant and translator. A 'cultural mentor' – someone from the cultural group you intend to work in – can also help pave the way and educate you both before you enter the field and during your work. In addition, prior contact with key informants and gatekeepers (if possible) should be considered to ease the path to the field. See Chapter 8 for more on language and field assistance.
- *Ethics and immigration clearance*: Most research activity requires some form of official clearance to proceed. Many universities now have human ethics approval procedures that require prior application before a project can be approved. You need to find out about such principles and procedures at an early stage and plan for this in your timetable (see Chapter 9). Similarly, if you are working in a country that you are not a citizen of, you will almost always need a special research visa. Investigate this early, for some countries have a very lengthy and difficult application process (see Chapter 6).
- *Budget*: Estimating the costs of research is not straightforward, especially if it is an unfamiliar location. Apart from the obvious costs of international travel and field equipment (voice recorders, etc.), you may need to add items such as visa and insurance costs, photocopying of documents, local travel, pay for translator/research assistant, gifts (if appropriate), and personal accommodation and sustenance. One of the best ways to estimate these latter costs is to consult an up-to-date backpacker guide (such as the *Lonely Planet* series) or online forums and blogs which cover cheaper accommodation and travel better than official tourist guides. You often succeed in finding accommodation with local households but do not underestimate the costs of this, and you should never exploit local hospitality. Be prepared to contribute to household expenses and give gifts above what may be asked for as 'rent'. See Chapter 6 for more information on funding and budgeting.
- *Timetable*: A good timetable is critical for guiding not just your time in the field but also your whole research project. It is often best to start at the end! Set a completion date for your work (this may be determined by funding, etc.) and keep to it. To assist you in this, making a completion date public to friends, family and supervisors creates a disincentive to drag your research on too long. Within this outer timeframe, set intermediate signposts: for example, finish literature review, leave for the field, finish fieldwork, complete data analysis, submit first draft and so on. You may have to revise these as you go and, of course, disasters or mishaps may justify extensions, but try to keep to your end date. Meeting intermediate targets might create pressures and panics along the way but they are better than leaving them all to the end. Also, when you meet those targets, reward yourself – have a (short) break – so you can maintain your energy and enthusiasm throughout the process. Some people, in preparing a timetable, also draw up a 'Plan B timetable': if things don't go according to Plan A, have a fall-back option that you can manage. Although you need to remain flexible, you can always find a reason to stay longer in the field. Use your data 'shopping list' to define the priorities of the data you see as essential (as opposed to that which is in the 'might be useful or interesting' category) and use the completion date to sharpen your decision when to stop.

Thus, research design is a critical process. You can plan what you have to do, you can anticipate what lies ahead and you can develop contingencies if things don't go quite as intended. Some degree of rigidity is necessary: keep a focus on your topic and don't be distracted by too many interesting cul-de-sacs; develop a budget and keep to it as much as possible; and set a timetable that allows for some latitude and down-time but sets an achievable end-date. But balance this rigidity with flexibility: be prepared to modify the plan as you go, for no piece of research ever goes exactly as anticipated. Expect shocks and disappointments. Be prepared to accommodate exciting and serendipitous opportunities. And always review throughout your research what you want to achieve, how you are going to achieve it and when you will have it done by. This is reflected upon in the boxed case study 2.7 below (see also Drybread, 2006; Gros, 2010; Haer and Becher, 2012).

### Box 2.7 Rigid methodology melting in the Chilean sun

*In April 1994, feeling scared and elated at the same time, I (Warwick) arrived in Chile to begin a year's PhD fieldwork. My only real comfort on arrival was the knowledge that my research objectives were clearly laid out with a neat methodology to suit. A lot of hard work had gone into developing the topic. By June of the same year, I had the feeling that these efforts had been of little worth as the rigid methodology had all but melted in the Chilean sun. In truth, this was far from the case. The research methodology and the nature of the project had 'evolved' considerably but, in retrospect, this was not only inevitable – it was desirable.*

*The main aim of my research was to assess the implications of neo-liberalism for small-scale fruit growers, and the relationship existing between such growers and multinational export companies. This would require a lot of qualitative and quantitative primary and secondary material, for which a detailed and timetabled plan had been devised. In the case of the small-scale growers, a questionnaire/semi-structured interview schedule had been developed which would be used with at least one hundred growers in two localities. Two sites were pre-selected for study, El Palqui (in the 'Norte Chico' region) and West Curicó (Maule region). Both localities (reputedly) had large populations of small-scale growers, and were important fruit export sources. In order to investigate the multinational exporters a postal questionnaire had been written. Further to this it was intended that a range of other informants – including agronomists, packing-house managers, local agricultural input suppliers, labour agencies etc. – would be interviewed. I planned to spend at least two months engaged in intensive research in each locality and return to Santiago to pick-up secondary data at the conclusion of the project. I even knew what bus company I was going to use to get about!*

*Hopes of sticking to this methodology soon evaporated. The first major problem lay in the selection of the study localities. In the case of West Curicó, the secondary information I used was unexpectedly outdated and the census from Santiago was wrong – virtually no*

*apple growers existed in the area! Those that did were invariably of a different size to that quoted. Eventually, a suitable area – East Curicó – was identified for study with the help of the local agricultural extension agency and a pilot study organised. By the time a specific focus area had been identified, two weeks had passed.*

*I also encountered considerable problems getting to as many small-scale growers as I had hoped and getting the type of information anticipated. A major shortcoming was linguistic. A three-month, once-a-week, course in Spanish was not sufficient preparation for the nuances of countryside Chilean. It was necessary to take an intensive course back in the capital and hang around a lot of cafés and bars talking, but mainly listening, to people. It is extremely educational to just 'hang-out' at times.*

*There were further logistical impediments. First, there were problems with obtaining a 'random' sample. A number of 'gatekeepers' (see Chapter 9) had to be relied on and this led to a bias, as the individuals would select those who they felt would be 'most interested' and 'interesting'. Second, the postal questionnaire was a failure (three responses from thirty, two of which were to inform me that they couldn't help!). Personal, pre-appointed visits were the only way that the export companies could be successfully approached. Third, it was time consuming to track down individuals. The growers' work took place from sun-up until sun-down. More often than not I would attempt to locate them at their plots (parcelas) often located very far apart. This was particularly the case in Curicó. Riding up to 50 km in the Chilean sun on mainly rough stone surface roads and with pockets full of stones to scare off mad dogs, was not envisaged during the research design phase.*

*A number of positive 'chance' discoveries also partly altered the direction of research. Whilst in a legal office in Ovalle near El Palqui for another purpose, a large set of fruit sale contracts drawn up between export firms and farmers was stumbled upon. The analysis of these contracts formed a major section in the final thesis.*

*Surprise meetings also became increasingly important. In a restaurant I ended up chatting with one of Ovalle's lawyers; somebody who had worked in the defence of small farmers in disputes concerning the re-possession of their property by companies. You should not become excessively concerned if the research timetable is altered.*

*The first few months in the field led to a large re-definition of the aims and methods of my project. Crucially, not as much time as originally hoped for could be spent in the field. I really wished to avoid the worst excesses of researching as a 'visiting outsider' (Chambers, 1983) but spending more time outside probably helped create a 'bigger picture'. Ambitions as to the number of farmers to be interviewed were cut and language difficulties meant that the information was not always as rich as hoped for. This, however, forced me to think about and focus on the really important issues. Chance findings convinced me that there was a much greater role for qualitative elements in the research. Overall, it became obvious that it was important to be flexible and eclectic. Designing by doing often leads to development research projects that better capture the essential elements of locality.*

Source: Murray (1997)

## CONCLUSION – EXPECT THE UNEXPECTED

Each field experience, like the places in which they unfold, will be totally different. However, a number of broad points can be offered, some of which may be of use to those about to start their fieldwork period. The study in Box 2.7 seems to suggest that a fine balance between rigidity and flexibility is required in fieldwork. It is important that the researcher has a clear idea of the purpose of his or her research aims and objectives. It is also advisable to have a clear idea of what methods will be employed in order to achieve these things. One must also be prepared to refine and, in some cases, let go of these plans once in the field – often at very short notice. In the same context, it may also be important to be prepared to think on one's feet. But the most important thing, perhaps, is not to give up: the authors know of very few researchers who have not experienced the above types of problems – almost invariably they have managed to sort them out.

Research is not easy, but it is remarkably rewarding. Expecting the unexpected and undertaking contingency planning can help researchers cope with unforeseen outcomes, raising the quality of the final output and the undoubted joys associated with arriving at that point. Good researchers are those who can design their work well and organise their time and resources accordingly. But there are also those who can react, adapt and revise their plans so that they can retain an eye on their objectives yet, if necessary, re-draw the map in order to get there.

### Summary

- Philosophical issues are important in research design, particularly matters relating to world-views and epistemology. Interrogate your own starting points and reflect on these issues throughout your research.
- Deciding on a research topic involves weighing up matters of inspiration, passion and practicalities. Choose a topic that will sustain your interest throughout the process but also consider its relevance, its feasibility and links to existing knowledge and theory.
- The choice of methods for a piece of research should flow logically from the methodology of the researcher and the key research questions. Methods are a means to an end.
- A good research proposal is a critical element in designing research. It should spell out key elements of the proposed research, but remember that is a statement of intent rather than a straightjacket for the research that follows.
- Good research design helps put in place important fixed elements for research, mainly a clear focus, direction and research question. However, in practice this clear vision and rigid framework need to be balanced by flexibility during the research process to respond to unforeseen obstacles and new opportunities.

## Questions for reflection

- What is your starting point in doing development research (insider/outsider, language proficiency, social status, etc.?)
- What are your views on epistemology (how knowledge is created and reproduced), and how does this help shape the approach to your research?
- Can you articulate your research topic in a single jargon-free sentence (or some other device to simplify and clarify what you intend to do)?
- Can you identify a clear central research question and a small number of secondary questions that allow you to answer the central one?
- What data will you require to address these questions and what methods are most appropriate to gather such data?
- To what extent can you foresee and plan for important practical issues such as logistics, ethics, finances and milestones?

## RECOMMENDED READINGS

Crang, M., and Cook, I. (2007). *Doing Ethnographies*. Los Angeles, CA: Sage.

For those engaged in intensive primary fieldwork this is an excellent, relatively jargon-free introduction covering theoretical and practical issues.

Creswell, J. W. (2008). *Research Design: Qualitative, Quantitative, and Mixed Methods Approaches*. London: Sage.

This book provides a broad-ranging introduction aimed at postgraduates and covering both qualitative and quantitative research design. In particular it deals with the possibility of combining approaches.

Desai, V., Elmhirst, B., Lemanski, C., Mawdsley, E., Meth, P., Oldfield, J., Page, B., Souch, C., Williams, G., and Willis, K. (2008). *Doing Development/Global South Dissertations: A Guide for Undergraduates*. Retrieved 14 July 2013 from [www.gg.rhul.ac.uk/DARG/DARG%20dissertation%20booklet.pdf](http://www.gg.rhul.ac.uk/DARG/DARG%20dissertation%20booklet.pdf).

An e-book covering practical, ethical and research considerations of development research, written for undergraduates (but also useful as a basic guide for postgraduates).

Graham, E. (2005). Philosophies underlying human geography research. In R. Flowerdew and D. J. Martin (eds), *Methods in Human Geography: A Guide for Students Doing a Research Project* (2nd edn, pp. 8–34). Harlow: Pearson Education.

This is a comprehensive and jargon-free introduction to the philosophies underlying human geography research and is applicable across the social sciences.

Robson, C. (2011). *Real World Research: A Resource for Social Scientists and Practitioner-Researchers* (3rd edn). Chichester: Wiley.

Robson provides a non-specialist introduction to the practical and philosophical issues surrounding research in what he terms the 'real world'. Excellent for those who are doing 'applied' research.

Sumner, A., and Tribe, M. (2008a). What can we 'know' in development studies? (pp. 53–80). *International Development Studies: Theories and Methods in Research and Practice*. London: Sage.

This chapter from a useful development studies text examines matters of ontology and epistemology: that is, what we can know and how we can know it in development studies.

## NOTE

1. Kitchen and Tate (2000: 7) remind us that the term 'empirical' should not be confused with the term 'empiricism'. The latter refers to the research philosophy described in this chapter, whilst the former refers to the collection of data for testing, which can take place within many different philosophical frameworks.