Choosing a research project

CHAPTER 9

This chapter provides key decision points of reference on which researchers can reflect and plan, including:

- how to choose a research project
- the importance of the research
- the purposes of the research
- ensuring that the research can be conducted
- research questions
- the scope of the literature review
- a summary of key issues in choosing a research topic or project

This chapter concerns the selection of the research and initial, practical matters that researchers can address when choosing and deciding the project on which to work. It is the first of six consecutive chapters that concern the planning of research. This chapter concerns the selection of the research and the initial matters to address, whilst the subsequent chapters unpack several of these in greater detail. We draw not only from relevant literature but from our own experiences of supervising several hundred research students. Research is a practical activity, and the advice that we give here is practical. This is not a simplistic recipe or low-level 'tips for researchers'; rather it is the distillation of key features of practicable research and issues on which to deliberate, and to help to ensure that the research provides relevant and useful findings.

9.1 Introduction

Choosing a research project is normally the decisive feature of successful research. Many novice students and researchers start with an over-ambitious project. The task of a mentor or supervisor is to help the novice researcher to narrow and hone down the research field in order to render the research practicable, useful and workable. Indeed part of the discipline of choosing and conducting a piece of research is fining it down to manageable/researchable proportions, to enable rigour (e.g. fitness for purposes and methodological soundness) to be inserted into the research. Rigour in planning and doing research lies in choosing a project that is sufficiently tightly framed. A research topic is only one small aspect of the field of the subject, and careful boundaries must be drawn around the topic: what it will and will not do.

For novice researchers, a piece of educational research often starts by wanting to be their life story or the opportunity to give their personal opinions some grounding in literature and empirical study that support their opinions or prejudices. This is not the task of research. The task of research is to find out, to investigate, to develop, to test out (e.g. a theory), to address questions such as: 'what if', 'how', 'why', 'how well', 'what' and 'where'.

9.2 What gives rise to the research project?

Several points can give rise to a research topic. For example, for many teachers it may be a problem that they encounter in their day-to-day work: they may want to find out the causes of the problem and how to solve it; they may want to plan an intervention to see how well it addresses or solves the problem. Examples of these might be: 'How can teachers improve students' learning of algebra in lower secondary schools?'; 'How to maximize the learning of students with Asperger's syndrome in mainstream schooling'; 'How to conduct a music lesson with many musical instruments, without the lesson descending into chaos and noise'; 'How to teach speaking a foreign language in large, mixed-ability classes'.

Some research projects may begin with an area of interest or personal experience that researchers may have been wanting to investigate, for example: 'What is the long-term effect on employment of early school dropout?'; 'How effective is early identification of behaviour disorders on educational provision for such students?'; 'How can teachers improve students' motivation to learn a second language?'; 'Why do young teachers leave teaching and older teachers stay?'

Some research topics may begin with a recognized area of importance or topical concern in the field, for example: 'How to maximize primary students' learning using ICT'; 'What is the effect of frequent testing on students' stress?'; 'How can developments in brain research and cognitive neuroscience impact on pedagogy?'; 'What is the predictive validity of personality tests or learning styles inventories on the success of first-time employees' applications for employment?'; 'Do interactive teaching methods produce higher test scores in university students than lecture-based teaching?' Such importance may arise from coverage of the topic in the press, articles, conference papers and journals.

Some research is conducted as part of a sponsored research project, in which the field and purposes of the research must be spelled out very clearly in order for the sponsorship to be obtained. For example in the UK the Economic and Social Research Council (www.esrc.ac.uk/research/research-topics), the Leverhulme Trust (www.leverhulme.ac.uk), Nuffield Foundation (www.nuffieldfoundation.org) and the Joseph Rowntree Foundation (www.jrf.org.uk) require detailed applications to be completed, and in the United States the Social Science Research Council (www.ssrc.org) requires similarly high levels of detail. Such funding might also need to fit the categories of research set out by the funding agencies.

A decision on what to research can arise from several wellsprings of the researcher's own motivation:

- a problem encountered in the researcher's everyday work or outside her/his everyday work (e.g. conceptual, theoretical, substantive, practical, methodological);
- an issue that the researcher has read about in a journal, book or other media;
- a problem that has arisen in the locality, perhaps in response to government policy or practices or to local developments;
- an area of the researcher's own interest;
- an area of the researcher's own experience;
- a perceived area of importance;
- an interesting question;
- a testable guess or hunch;
- a topical matter;
- disquiet with a particular research finding that one has met in the literature or a piece of policy (e.g. from the school, from a government), and a wish to explore it further;
- an awareness that a particular issue or area has been covered only partially or selectively in the literature, and a wish to plug the gap;
- a wish to apply a piece of conceptual research to actual practice, or to test a theory in practice;
- a wish to rework the conceptual or theoretical frameworks that are often used in a specific area;
- a wish to revise or replace the methodologies that are often used in researching a specific area;
- a desire to improve practice in a particular area;
- a desire to involve participants in research and development;
- a desire to test out a particular methodology in research;
- an interest in seeing if reported practice (e.g. in the literature) holds true for the researcher's own context (e.g. a comparative study);
- an interest in investigating the causes of a phenomenon or the effects of a particular intervention in the area of the phenomenon;
- a wish to address an issue or topic that has been under-researched in the literature;
- a priority identified by funding agencies;
- an issue identified by the researcher's supervisor or a project team of which the researcher is a member;
- a wish to explore further or to apply an issue or topic that one has encountered, for example, in the literature.

The long list above concerns the motivation that leads a researcher to consider doing a particular piece of research. Add to this a salutary point for researchers, which is that the study on which they might embark will probably take weeks, months and maybe years. Sustaining interest and momentum in the researcher(s) are important considerations. Researchers should ask themselves whether they really have the interest in studying the issue in question or in conducting the research for a long period of time. If the answer is 'no' then, if they have the luxury of not having to do this particular piece of research, they may wish to consider an alternative area that will enable them to sustain interest in, and motivation for, the research. A piece of research that is conducted by an unwilling or bored researcher could easily become unimpressive.

Beyond the *motivation* for the research are the *sources* of the research in question: where does research come from? For example, the research may derive from:

- a practical concern (e.g. 'why do females have higher scores than males in international tests of reading at age 14?') or a practical need (Leong *et al*., 2012);
- a literature review (though Andrews (2003) observes that if the research question derives from the literature review then there is a risk that there is no research question to initially drive the literature review (p. 18), i.e. the literature review could lack direction, purpose and boundaries). A literature

search (including specialist literature in the field, primary and secondary sources) helps the researcher to understand the existing field and the real-world implications of the research (Alvesson and Sandberg, 2013);

- the identification of a gap in the literature or field of study (gap filling) (Alvesson and Sandberg, 2011, 2013);
- the identification of where the research can build on existing literature (Alvesson and Sandberg, 2011);
- a theoretical concern, enabling theories to be generated and tested (e.g. 'how significant is performance-related pay in motivating senior managers of schools?', in which the 'theory' to be tested is that performance-related pay is a necessary but not sufficient motivator of senior staff (Pink, 2011));
- policy concerns (e.g. 'how effective is such-and-such in attracting females to take STEM subjects?');
- concerns in the media and blogs (including the Internet);
- society, empirical data (Alvesson and Sandberg, 2013, p. 16);
- personal experience, interest or observation (Leong *et al*., 2012);
- colleagues and contacts (ibid.);
- experts and practitioners in the field (ibid.);
- conferences and conventions (ibid.);
- faculty seminars, research groups, discussion groups and workshops (ibid.);
- students (ibid.);
- societies, associations, research bodies and special interest groups;
- spotting where areas are neglected, for example, overlooked/under-researched;
- existing studies and influential theories (Alvesson and Sandberg, 2013, p. 17);
- challenge to, or problematization of, an assumption, agenda or existing theory (Alvesson and Sandberg, 2013);
- a novel idea which challenges existing ideas or practices;
- funding bodies and/or project directors;
- spotting where applications may lie;
- spotting where confusions need to be clarified;
- spotting where new methodologies and research methods might be applied;
- other starting points – the list is endless.

It is essential that the research and the questions it asks should address something that is worth asking: asking the right question (Leong *et al*., 2012, p. 121). In turn this means that the research itself must be worth doing – it must make a significant contribution to the field. Behind the many features of effective research questions lies the need to ensure that the research itself, i.e. in principle, is interesting. In this respect there is an overlap in the literature between research areas and research questions, i.e. what some authors would place under the category of 'research questions' could just as easily be placed in the category of 'research areas' or 'fields of research', or 'research topics'. This harks back to the seminal work of Davis (1971) (see also Chapter 4), who provides a formidable list of twelve factors that make social science, and hence research and research questions, 'interesting'.

More recently, Alvesson and Sandberg (2011, 2013) argue that much research is 'gap filling', and that, whilst worthy, this risks being over-confined to the status quo, conservative, under-problematizing or over-problematizing matters, derivative and non-interesting because, since it builds on or around existing literature, it does not challenge assumptions in the literature, does not sufficiently problematize assumptions and agendas, and does not generate really new ideas or innovatory, creative thinking. It reinforces rather than challenges consensus (Alvesson and Sandberg, 2011, p. 250). Gap spotting, they observe, might be easy, uncontroversial and resonant with the idea of cumulative research, but it does not question received wisdoms and research perspectives.

Rather, Alvesson and Sandberg (2011, 2013) argue for the problematization of issues and the development of new ideas – *challenging* assumptions, agendas and theories – in order to create 'interesting' and 'influential' research and research questions (2013, p. 45). Problematization and questioning assumptions, they suggest, is a powerful methodology for generating interesting research questions and questioning of received truths, i.e. disruptive of existing theory, practices, paradigms and ideologies, and it is faithful to the uncertain nature of scientific 'truths' (p. 50). The aim of problematization, they argue, is to 'disrupt rather than build upon and extend an established body of literature' (2011, p. 248).

Of course, gap filling, building on existing research and problematization for the creation of new ideas are not mutually exclusive. All can generate 'interesting' research; as the authors remark (Alvesson and Sandberg, 2011, p. 266), there are good reasons for gap spotting as this can enable research to supplement and enrich existing studies, and clarify issues, for example, where there are disagreements among researchers. Innovative, high-impact research questions, they suggest (Alvesson and Sandberg, 2011, 2013), stem from the questioning of assumptions that underlie existing theories in significant ways. They set out a methodology for problematization to produce 'interesting' research and research questions which constitutes one of Davis's (1971) features of 'interesting' research: what appear to be matters or phenomena that can coexist actually cannot, and vice versa (p. 4). Alvesson's and Sandberg's (2011, p. 256) methodology for generating 'interesting' research through 'dialectical interrogation' of assumptions requires researchers to:

Step 1: Identify a domain of literature;
Step 2: Identify and articulate the assumptions that underlie that domain;
Step 3: Evaluate the assumptions that underlie that domain;
Step 4: Develop an alternative assumption ground;
Step 5: Consider this alternative assumption ground in relation to its audience;
Step 6: Evaluate the alternative assumption ground.

Essentially the task is to expose and evaluate existing 'in-house' assumptions (e.g. in the literature, in 'theories'), i.e. those assumptions which are regarded as unproblematic and which are accepted by their advocates (p. 254), thence to challenge those assumptions (e.g. problems with them, their shortcomings and oversights) (p. 267), and develop and evaluate an 'alternative assumption ground' that will generate 'interesting' theory, taking the latter into account in relation to the audience, i.e. the wider intellectual, social and political situation of the research community and their possible reactions to the challenges posed (p. 258), and check to see if the alternative assumption ground is obvious, interesting or, indeed, absurd (p. 259).

Alvesson and Sandberg argue, for example, that rather than trying to develop research and research questions solely from a literature review, it might be more 'interesting' (and they use Davis's (1971) word here) to ask how a particular field becomes the target of investigation, to evaluate and challenge the *assumptions* (unchallenged, accepted and shared schools of thought), *ideologies* (e.g. values, politics, interests, identifications, moral and ethical views), *paradigms* (ontological, epistemological and methodological assumptions, world views), *root metaphors* (images of a particular area) and *field assumptions* (broader sets of assumptions about specific subject matter which are shared by schools of thought within, across a paradigm or discipline) (2011, p. 255) that underlie a theory. From there, the researcher seeks to develop and evaluate the 'alternative assumption ground' which, thereby, is 'more disruptive' and 'less reproductive' (Alvesson and Sandberg, 2013, p. 122). Challenging in-house assumptions is regarded as a minor level of problematization (Alvesson and Sandberg, 2011, p. 255); questioning root metaphors constitutes a middle-ground challenge; and challenging ideology, paradigms and field assumptions constitutes a more fundamental form of problematization (p. 255).

Leong *et al*. (2012, pp. 128–9) suggest that research and its research questions can be framed which: (i) discover a new effect; (ii) extend an established effect (e.g. to new domains); (iii) demonstrate mediation of factors (interaction), i.e. the mechanisms that lead to an effect; and (iv) moderation of an established effect (modelling for which groups of people/situations the effects hold true or not true). Whilst discovering a new effect may be for seasoned researchers, they note that extending an established effect may be suitable for novice researchers. They comment that moving beyond 'gap filling' to novel research is uncomfortable because it takes us out of our familiar, sedimented, deeply ingrained ways of thinking. They suggest that making the opposite assumptions, exposing hidden assumptions, casting doubt on existing assumptions and scrutinizing meanings of key concepts is unsettling (pp. 126–7).

Alvesson and Sandberg (2011, 2013) are arguing that effective, high-impact research and research questions derive from high-impact research proposals that move beyond 'gap filling' to disrupting conventions, modes of thinking and examining a phenomenon. This echoes Leong *et al*. (2012) who argue that creative, innovative, worthwhile research may be unclear at the outset and that if it is too clear too early on then it may not be focusing on anything new or important (p. 122); as the authors say, if it is too predictable, why do it? Indeed they write that an innovative research question is one that generates ambiguity rather than certainty, and they suggest that effective research questions are those which: are unclear on their outcomes; can generate answers; and discriminate between theories, each of which leads to different predictions (p. 122).

9.3 The importance of the research

Whatever research area or topic is identified, it is important for it to be original, significant, non-trivial, relevant, topical, interesting to a wider audience and to advance the field. For example, I may want to investigate the use of such-and-such a textbook in Business Studies with sixteen-year-olds in Madagascar, but, really, is this actually a useful research topic or one that will actually help or benefit other teachers or educationists, even though it yields original data?

Or I might conduct research that finds that older primary children in a deprived area of Aberdeen, Scotland prefer to have their lunch between 12 noon and 1.00 p.m. rather than between 1.00 p.m. and 2.00 p.m., but, really, does anybody actually care? The topic is original and, indeed, the data are original, but both are insignificant and maybe not worth knowing.

In both of these examples, the research brings about original data, but that is all. Research needs to go beyond this, to choose a significant topic that will actually make an important contribution to our understanding and to practice. Originality alone is not enough. Rather, the research should move the field forward, perhaps in only a small-scale, piecemeal, incremental way, but nevertheless to advance it such that, without the research, the field would be poorer. Hence it is important to consider how the research takes the field forwards not only in terms of data, but also conceptually, theoretically, substantively and/or methodologically. At issue here is not only the contribution to knowledge that the research makes, but the *impact* of that knowledge; indeed funding agencies typically require an indication of the impact that the research will make on the research community and more widely, and how that impact will be assessed and known. What will be the impact, uptake and effects of the research, and on whom?

It is also useful for the researcher to identify what benefit the research will bring, and to whom, as this helps to focus the research and its audience. Fundamental questions are 'what is the use of this research?' 'What is the point of doing this research?' 'Who benefits?' 'Is this research worth doing?' If the answer to the last question is 'no', then the researcher should abandon it, otherwise it ceases to be useful research and becomes an indulgence of the dilettante.

Many novice researchers may not know whether the research is original, significant, important, complex, difficult, topical and so on. Here it is important for such a novice to read around the topic, to conduct a literature search, to conduct an online search, to attend conferences on the topic, to read newspaper reports on the topic; in short, to review the state of the field before coming to a firm decision on whether to pursue research in that field. In this respect, if the researcher is a student, it is vital to discuss the proposed topic with a possible supervisor, to receive expert feedback on the possible topic.

Before a researcher takes a final decision on whether to pursue a particular piece of research, it is useful to consider selecting a topic that interests the researcher, reading through background materials and information and compiling a list of keywords, clarifying the main concepts and writing the topic as a statement (or a hypothesis). Whilst incomplete, nevertheless this provides a useful starting point for novice researchers contemplating what to research.

9.4 The purposes of the research

Implicit in the previous section is the question 'why do the research?' This is ambiguous, as 'why' can refer to reasons/causes and purposes, though the two may overlap. Whereas the previous section concerned reasons, this section concerns purposes: what we want the research to achieve. It is vital that the researcher knows what she or he wants the research to 'deliver', i.e. to answer the question 'what are the "deliverables" in the research?' In other words, what do we want to know as a result of the research that we did not know before the research commenced? What do we want the research to do? What do we want the research to find out (which is not the same as what we want the results to be: we cannot predict the outcome, as this would be to 'fix' the research; rather, the kind of information or answers we want the research to provide)?

In this respect it is important for the researcher to be very clear on the purposes of the research, for example:

- to demonstrate that such-and-such works under a specified set of conditions or in a particular context (experiment; action research);
- to increase understanding and knowledge of learning theories (literature-based research);
- to identify common features of successful schools (research synthesis; descriptive research);
- to examine the effects of early musical tuition on general intelligence (meta-analysis; multilevel research);
- to develop and evaluate community education in rural and dispersed communities (participatory research; evaluative research; action research);
- to collect opinions on a particular educational proposal (survey);
- to examine teacher–student interactions in a language programme (ethnography; observational research);
- to investigate the organizational culture of the science faculty in a university (ethnography; survey);
- to identify the relative strengths of a range of specified factors on secondary school student motivations for learning (survey; observational study; multiple regression analysis; structural equation modelling);
- to see which of two approaches to teaching music results in the most effective learning (comparative study; experiment; causal research);
- to see what happens if a particular intervention in setting homework is introduced (experiment; action research; causal research);

- to investigate trends in social networking in foreign language teacher communities (network analysis);
- to identify key ways in which teachers in a large secondary school view the leadership of the senior staff of the school (personal constructs; accounts; survey);
- to interrogate government policy on promotion criteria in schools (ideology critique; feminist critique);
- to see the effects of assigning each student to a mentor in a university (survey; case study; causal research);
- to examine the long-term effects of early student dropout from school (survey; causal or correlational research);
- to see if repeating a year at school improves student performance (survey; generalization; causal or correlational research);
- to chart the effects of counselling disruptive students in a secondary class (case study; causal or correlational research);
- to see which catches richer survey data on student drug usage: questionnaires or face-to-face interviews (testing instrumentation; methodology-related research);
- to examine the cues that teachers give to students in question-and-answer classroom episodes (discourse analysis);
- to investigate vandalism in schools (covert research; informer-based research);
- to investigate whether case studies or surveys are more effective in investigating truancy in primary school (comparative methodology);
- to run a role-play exercise on communication between a school principal and senior teachers (role-play);
- to examine the effects of resource allocations to under-performing schools (ideology critique; case study; survey; causal research);
- to understand the dynamics of power in primary classrooms (ethnography; interpretive research);
- to investigate the demise of the private school system in such-and-such a town at the end of the nineteenth century (historical research);
- to understand the nature of trauma and its treatment on primary-aged children living in violent households (case study; action research; grounded theory; ex post facto research);
- to generate a theory of effective use of textbooks in secondary school physics teaching (grounded theory);
- to clarify the concept of 'the stereotype activation effect' for investigating the effect of sex stereotyping on reading in young teenagers (survey; case study; experiment; causal research);
- to test the hypothesis/theory that increasing rewards loses effect on students over time (experiment; survey; longitudinal research; causal or correlational research).

As can be seen in these examples, different purposes suggest different approaches, so 'fitness for purpose' takes on importance in planning research (see Chapter 10). One can also see that there is a range of purposes and types of research in education. The researcher cannot simply say that he or she likes questionnaires, or is afraid of numbers, or prefers to conduct interviews, or feels that it is wrong to undertake covert research so no covert research will be done. That is to have the tail wagging the dog. Rather, the research purposes determine what follow in respect of the kind of research, the research questions, the research design, the instruments for data collection, the sampling, whether the research is overt or covert (the ethics of research), the scope of the research, and so on.

9.5 Ensuring that the research can be conducted

Many novice researchers, with the innocence and optimism of ignorance, may believe that whatever they want to do can actually be done. This is very far from the case. There is often a significant gulf between what researchers want to do and what actually turns out to be what they can do.

A formidable issue to be faced here is one of *access*. Many new researchers fondly imagine that they will be granted access to schools, teachers, students, parents, difficult children, students receiving therapy, truants, dropouts, high performers, star teachers and so on. This is usually NOT the case: gaining access to people and institutions is one of the most difficult tasks for any researcher, particularly if the research is in any way sensitive (see Chapter 13). Access problems can kill the research, or can distort or change the original plans for the research.

It is difficult to overstate the importance of researchers doing their homework before planning the research in any detail, to see if it is actually feasible to gain access to the research sites or people they seek. If the answer is 'no' then the research plan either stops or has to be modified. It is not uncommon for the researcher to approach organizations (schools, colleges, universities, government departments) with some initial, outline plans of the research, to see if there is a possibility, likelihood or little or no chance of doing the research.

Nor is it enough to be clear on access; supplementary to this is 'access to what?'. It is of little use to be given access to a school by the school principal if the teachers have not been consulted about this, or if they are entirely uncooperative (see the discussion of informed consent in Chapter 7). One of the authors recalls an example of a Master's student who wanted to study truancy; the student had the permission of the school principal and turned up on the day to commence the research with the school truants, only to find that they had truanted, and were not present! The same is true for sensitive research. For example, let us suppose that one wished to research child abuse in primary school students. The last people to consent, or even to be identified and found, might be the child abusers or the abused children; even if they *were* identified and found, why should they agree to being interviewed by a stranger who is conducting research? Or, let us suppose that one wished to investigate the effects on teachers of working with HIV-positive children in hospital; those teachers might be so traumatized or emotionally exhausted at the end of a day's work that the last thing they want to do is to talk about it further with an outside researcher whom they have never met before; they simply want to go home and 'switch off'. These are real issues. The researcher has to check out the situation before embarking on a fully worked-out plan, because the plan might come to nothing if access is not possible.

It is not only the people with whom the researcher is working who have to be considered; it is the researcher herself/himself. For example, does the researcher have the right personality, dispositions, sympathies, interpersonal skills, empathy, emotional intelligence, perseverance and so on to conduct the research? For instance, it would likely be a disaster if a researcher were conducting a piece of research on student depression and tacitly believed that students were just lazy or work-shy and that they used 'feeling down' (as the researcher might put it) as an excuse, i.e. the researcher refused to recognize the seriousness of depression as a clinical condition or as a pathological disorder. Equally, it would be an unwise researcher who would choose to conduct a longitudinal study if she had limited perseverance or if she knew that she was going to move overseas in the near future.

Researchers themselves will also need to decide whether they have sufficient expertise in the field in which they want to do the research. It could be dangerous to the researcher and to the participants if the researcher were comparatively ignorant of the field of the proposed research, as this could mean that direction, relevance, prioritization or even safety might be jeopardized. This is a prime reason for the need for researchers to conduct a literature review, to demonstrate that they are sufficiently well-versed in the field to know what to do, what to look for, and where, when and how to proceed.

Researchers will also have a personal commitment to the research; it may help to further their specialist interest or expertise; it may help to establish their reputation; it may make for career advancement or professional development. These considerations, though secondary, perhaps in choosing a piece of research, nevertheless are important features, given the commitment of time and effort that the research will require.

In addition to access, there are issues of time to be considered. Part of the initial discipline of doing research is to choose a project that is manageable – can actually be done – within the time frames that the researcher has at her/his disposal. It would be ridiculous for a researcher to propose a longitudinal study if that researcher only has maybe six or nine months to plan, conduct and report the entire research project. The time frames may prevent certain types of research from being conducted.

Similarly, the time availability of the researcher has to be considered: many researchers are part-time students who may not have much time to conduct research, and often their research is a lonely, one-person affair rather than a group affair with a team of full-time researchers. This places a practical boundary around what can and cannot be done in the research. Again, these are real issues. The availability of the researcher features in ensuring that the research can be conducted, and this applies equally to the participants: are they willing and able to give up their time in participating in the research, for example, in being interviewed, in keeping diaries, attending follow-up debriefings, participating in focus groups and writing reports of their activities?

Whilst access and time are important factors, so are resources (e.g. human, material). For example, if one is conducting a postal survey there are costs for printing, distribution, mail-back returns and follow-up reminders. If one is conducting a questionnaire survey on a large, dispersed university campus then one will need the cooperation of academic and administrative staff to arrange for the distribution, collection and return of the questionnaires. If one is conducting an online survey of teachers' views of, for example, government assessment policy, can it be assured that all teachers will have access to the online facilities at times that are convenient for them, and that poor connectivity, slow speed and instability of the system will not end in them abandoning the survey before it is completed?

If one is conducting an analysis of trends in public education in early-twentieth-century Scotland, then one needs to have time to search and retrieve public records (and this may involve payment), maybe visit geographically dispersed archives, and sit in front of microfiche readers or computers in public record offices and libraries.

A further consideration in weighing up the practicalities of the research is whether, in fact, the research will make any difference. This is particularly true in participatory research. Researchers may wish to think twice before tackling issues about which they can do nothing or over which they may exert little or no influence, such as changing an education or schooling system, changing the timetabling or the catchment of a school, changing the uses made of textbooks by senior staff, changing a national or school-level assessment system. This is not to say that such research cannot or should not be done; rather it is to ask whether the researcher's own investigation can do this, and, if not, then what the purposes of the research really are or can be.

Many researchers who are contemplating empirical enquiries will be studying for a degree. It is important that they will be able to receive expert, informed supervision for their research topic. Indeed, in many universities a research proposal will be turned down if the university feels that it is unable to supervise the research sufficiently. This will require the student researcher to check out whether his/her topic can be supervised properly by a member of the staff with suitable expertise, and, indeed, many students find this out before even registering with a particular university. It is a sound principle.

A final feature of practicality is the scope of the research. This returns to the opening remarks of this chapter, concerning the need to narrow down the field of the study. We advise that a single piece of research be narrow and limited in scope in order to achieve manageability as well as rigour. As the saying goes, 'the best way to eat an elephant is one bite at a time'! Researchers must put clear, perceptible, realistic, fair and manageable boundaries round their research. If this cannot be done straightforwardly then maybe the researcher should reconsider whether to proceed with the planned enterprise, as uncontrolled research may wander everywhere and actually arrive nowhere. Part of the discipline of research is to set its boundaries clearly and unequivocally. In choosing a piece of research, the manageability of setting boundaries is important; if these cannot be set, then the question is raised of the utility of the proposed endeavour.

For example, if one were to investigate students' motivations for learning, say, biology, this would involve not only identifying a vast range of independent variables, but also handling likely data overload, and ensuring that all the theories of motivation were included in the research. This quickly goes out of control and becomes an impossible task. Rather, one or two theories of motivation might be addressed, within a restricted, given range of specified independent variables (unless, of course, the research was genuinely exploratory), and with students of a particular age range or kind of experience of biology.

Small samples, narrowly focused research, can yield remarkable results. For example, Axline's *Dibs in Search of Self* (1964) study of the restorative and therapeutic effects of play therapy focused on one child, and Piaget's (1932) seminal theory of moral development, in *The Moral Judgement of the Child*, focused on a handful of children. In both cases, the detailed carefully bounded research yielded great benefits for educationists.

Practical issues, such as those mentioned here, often attenuate what can be done in research. They are real issues. The researcher is advised to consider carefully the practicability of the research before embarking on a lost cause in trying to conduct a study that is doomed from the very start because insufficient attention has been paid to practical constraints and issues.

9.6 Considering research questions

The move from the aims and purposes of a piece of educational research to the framing of research questions – the process of operationalization of the research – is typically not straightforward, but an iterative process. The construction of careful research questions is crucial and we devote an entire chapter to this (Chapter 10). We refer the reader to that chapter and indicate in it that research questions typically drive and steer much research.

It is the answers to the research questions that can provide some of the 'deliverables' referred to earlier in the present chapter. A useful way of deciding whether to pursue a particular study is the clarity and ease in which research questions can be conceived and answered. As mentioned in more detail in Chapter 10, research questions turn a general purpose or aim into specific questions to which specific, data-driven, concrete answers can be given. Questions such as 'what is happening?', 'what has happened?', 'what might/will/should happen?' open up the field of research questions. Chapter 6 also mentioned causal questions; here 'what are the effects of such-and-such a cause?' and 'what are the causes of such-and-such an effect?' are two such questions, to which can be added the frequently used questions 'how?' and 'why?'. These questions ask for explanations as well as reasons.

As we mention in Chapter 10, the research may have one research question or several. Andrews (2003, p. 26) suggests that the research should have only one main research question and several supporting questions: 'subsidiary' questions which derive from and are necessary, contributory questions to the main research question (see Chapter 10 of the present volume). He notes that it is essential for the researcher to identify what is the main question and how the subsidiary questions relate to it. For example, he suggests that a straightforward method is to put each research question onto a separate strip of paper and then move the strips around until the researcher is happy with the relationship between them as indicated in the sequence of the strips (p. 39). This implies that the criteria for identifying the relationship have to be clear in the researcher's mind (e.g. logical/chronological/psychological, general to specific, which questions are subsumed by or subsidiary/subordinate/superordinate to others, which questions are definitional, descriptive, explanatory, causal, methodological etc., which question emerges as the main question). This process, he notes (p. 41), also enables the researcher to identify irrelevant questions and to refine down, to delimit the research; many novice researchers may have many research questions, each of which merits its own substantial research in itself, i.e. the research questions are unrealistically ambitious.

Chapters 1 and 2 drew attention to numerical, non-numerical and mixed methods research questions. Some research questions might need to be answered by gathering only numerical data, others by only qualitative data. However, we recommended in Chapter 2 that, for mixed methods research, attention should be paid to the research questions such that they can *only* be answered by mixed – combined – types of data, or by adopting mixed methodologies, or by having a set of purposes that can only be addressed by mixed methods, or by taking mixed samples, or by having more than one researcher on the project (mixed researchers), in short, by building a mixed methods format into the very heart of the research. So, a research question in this vein might combine 'how' and 'what' into the same research question, or 'why' and 'who' might be combined in the same question, or description and explanation might be combined, or prediction, explanation and causation might be combined, and so on. We provide examples of these in Chapter 2.

It has been suggested (e.g. Bryman, 2007b) that, in mixed methods research, the research question has considerable prominence in guiding the research design and sampling, yet it is often more difficult to frame research questions in mixed methods research than in single paradigm research (e.g. quantitative or qualitative) (Onwuegbuzie and Leech, 2006a, p. 477). This is because it requires quantitative and qualitative matters to be addressed within the same research questions. Onwuegbuzie and Leech provide examples of mixed methods research questions, such as 'What is the relationship between graduate students' levels of reading comprehension and their perceptions of barriers that prevent them from reading empirical research articles?' (pp. 483–4). Here both numerical and qualitative data are required in order to provide a complete answer to the research question (e.g. numerical data on levels of reading comprehension, and qualitative data on barriers to reading articles) (p. 484). They provide another example of mixed methods research questions thus: 'What is the difference in perceived classroom atmosphere between male and female graduate students enrolled in a statistics course?' (p. 494). This could involve combining measures with interviews.

Here is not the place to discuss the framing of research questions (Chapter 10 addresses this). Here we draw attention to research questions per se, in particular their clarity, ease of answering, comprehensiveness, comprehensibility, specificity, concreteness, complexity, difficulty, contents, focus, purposes, kinds of data required to answer them and utility of the answers provided, to enable researchers to decide whether the particular piece of research is worth pursuing. This will require researchers to pause, generate and reflect on the kinds of research question(s) required before they decide whether to pursue a particular investigation. This argues that researchers may wish to consider whether they really wish to embark on an inquiry whose research questions are too difficult or complex to answer within the scope or time frames of the study. Many of the most useful pieces of research stem from complex issues, complex research questions and 'difficult-to-answer' research questions. They move from Alvesson's and Sandberg's (2013) 'gap filling' to problematization, new ideas and areas, innovatory thinking and the elements that make for Davis's (1971) 'interesting' research, mentioned in Chapter 4.

9.7 The literature search and review

A distinction has to be drawn between a literature search and a literature review. The former identifies the relevant literature; the latter does what it says: reviews the literature selected. If the researcher knows in advance what are the research purposes, issues and research questions then this can make the literature