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.