Introduction to Social Research

Quantitative and Qualitative Approaches

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Section 2.3 showed the continuum of possibilities regarding the timing of structure in a research project. That continuum applies to design and data, and to research questions, the subject of this chapter. The contrast was shown between fully prespecified research questions, and those which unfold as the study develops. It was pointed out that quantitative research was more likely to use prespecified questions, whereas qualitative research is more diverse. It was also pointed out that there are advantages to prespecified research questions. The next two chapters focus on developing research questions.

As a finished product, a piece of empirical research needs to demonstrate both conceptual clarity, and a good fit between its different component parts, especially between its questions and its methods. For the report of the completed research, it does not matter about the order in which things were done, only that there is this conceptual clarity and good fit. They can be achieved in different ways: the questions can be developed first, and the methods aligned; or the research might begin with only a general approach to its topic, and then develop focus in the questions and methods as things proceed; or there might be a mixture of these two, where the researcher cycles backwards and forwards between questions, methods and some initial data.
The next two chapters describe a model of research with well developed research questions 'up front'. This is a good model for learning about research, making it easy to see the connections between questions, concepts and data, and thereby promoting fit and conceptual clarity. As a model, it is worth aiming at; if it is rejected as not appropriate, the reasons for that rejection are helpful in understanding the area, and in tailoring a more suitable approach. This helps clarify where on the continuum of structure the researcher wants to be. That position can then be articulated, to ensure that the approach to design and data fits with it.

Miles and Huberman (1994: 55) point out that developing research questions is a valuable defence against the confusion and overload that is possible in the early stages of research. Often, also, the researcher can make considerable progress towards identifying specific research questions, particularly when considerable knowledge is brought to the research. However, when research questions emerge during the research project rather than in advance of it, the need for conceptual clarity and for fit still arises, and therefore the issues in these next two chapters remain relevant. They do not disappear, they simply come up later.

3.1 GENERAL AND SPECIFIC RESEARCH QUESTIONS

Empirical research is driven by research questions. One way to get to research questions is to identify a research area, and then develop questions within that area, working deductively from general to specific questions. Another is more inductive: to begin with some specific questions, and to work from these back to more general questions.

The distinction between general and specific research questions, or questions and subquestions, is important. General research questions guide the thinking, and are of great value in organizing the project, but they are not themselves specific enough to be answered. Specific research questions ideally follow from the general questions. They direct the empirical procedures, and they are the questions which are actually answered in the research. It is useful in planning to identify and separate the general and the specific research questions.

It is also important to distinguish both of these from the research area or topic. This difference can be a source of confusion. When asked the key question 'What are you trying to find out?', students will often respond by identifying an area. A research area and research questions are different things. An area identifies a general field of inquiry, within which we can identify many research questions, general and specific. Thus there is a hierarchy of concepts – the research area, the general research questions and the specific research questions.

The first general way of proceeding identified above is to begin with a research area, and then identify questions within that area. Sources and types of research areas and topics are discussed by Marshall and Rossman (1989), by Campbell et al. (1982) and by Zuckerman (1978), and seven ways of selecting topics are suggested by Neuman (1994: 110): personal experience, curiosity based on something in the media, the state of knowledge in a field, solving a problem (often associated with professional experience), 'social premiums', personal values and everyday life. Whatever the method of selection, identifying an area enables the researcher to connect the work to the research literature. But identifying the area is only the first step, and the researcher needs to go further, by developing both general and specific research questions.

The second way of proceeding is to begin with some specific question of interest. Again, this may come from any source at all: personal or professional experience, 'real-world observations, dilemmas and questions' (Marshall and Rossmann, 1989: 28), the literature, or somewhere else. When beginning with a specific question, it will be necessary to move upwards in generality and abstraction, to more general research questions and then to the research area. It does not matter which of these ways is used, or if they are mixed up. What matters is the development of the questions, their location in a research area, and their ordering into general and specific research questions.

Example 3.1 points to quantitative and qualitative examples of general and specific research questions. It is often easier to develop this sort of question structure in quantitative projects, because of established patterns of thinking in quantitative design, as is described in Chapter 5. But it is helpful in planning qualitative research too, as the Miles and Huberman example shows. In ethnographic research also, Spradley (1980) suggests a taxonomy of specific research questions which can follow general questions. The general questions are sometimes called 'grand tour' questions; the specific questions would then be 'mini tour' questions, though the taxonomy has other sorts of specific questions as well. In addition to these examples, the monograph by Campbell et al. (1982) contains many general and specific research questions generated by applied psychological research into organizations and their behaviour, and Miles and Huberman (1994: 24) cite Smith's example of different types of specific research questions and their general form, used in a study evaluating reading instruction.

3.2 DEVELOPING RESEARCH QUESTIONS

This stage is sometimes called conceptual analysis, but since that term has philosophical overtones, the more general term 'question development' is used. However, there is usually an important role in this stage for the sort of conceptual analysis done in philosophy, which involves clarifying meanings, defining terms and distinguishing between different types of questions.

Sometimes the more abstract concepts of research area and general and specific research questions are not enough to get the process of identifying
EXAMPLE 3.1: GENERAL AND SPECIFIC RESEARCH QUESTIONS

Quantitative
• De Vaus (1991: 27–44) starts with the broad topic of divorce, and then (guided by a conceptual model) frames general and specific research questions to guide a survey to study increases in divorce rates.
• Jaeger (1988: 308–11) shows the hierarchical approach to defining research questions in designing a survey of teacher ‘burnout’. Three broad areas are identified, and general and specific research questions are developed within each area.

Qualitative
• Miles and Huberman (1994: 24) show the use of general and specific research questions in their case study of the adoption of innovations. Four general questions are shown, and several specific questions are derived from each.

and developing research questions started. When that happens, it is good to focus on the question ‘What are we trying to find out?’ The focus on this question almost always shows that there is ‘much more here than meets the eye’. The topic expands, and many questions are generated. What perhaps seemed simple and straightforward becomes complicated, many-sided and full of possibilities. This can happen whether the researcher proceeds deductively or inductively. On the one hand, any research area, when fully analysed, will yield many research questions, general and specific. On the other, any research question, when carefully considered, will generate others, and they in turn will generate more.

What sort of work is this, in the question development stage? First, it is generating possibilities. Answers to the question ‘What are we trying to find out?’ are provisional at this stage. We do not want to get to a final set of questions too quickly, because we might overlook possibilities. Generating possibilities is not endless, but we do want to allow enough time to see what the possibilities are. Second, it is a mixture of question subdivision, where we split a general question into its component parts, and of disentangling the different questions from each other. Third, it is ordering these questions, and progressively developing focus.

It is usually an iterative process to get a stable view of what one is trying to find out. There are benefits to doing some of this work with others – another student, or a small group, which might include supervisor(s), colleagues or other researchers. Others will often see possible questions that the individual researcher might miss, and discussion with others can also be a stimulus to think more deeply, and perhaps differently, about the topic.

What generally happens after a period of question development is that the whole thing has expanded, sometimes greatly. This can cause anxiety, but for most projects it should happen. In fact, if it doesn’t, we should probably be concerned, since it may be a sign of insufficient question development work. Therefore, it is to be encouraged, within reason, as an important stage. Probing, exploring, and seeing other possibilities with a topic can be valuable before reaching closure on the specific directions for a project.

When a small set of starting questions has multiplied into a larger set, disentangling and ordering are required. Disentangling is necessary because one question will often have other questions within it. Ordering involves categorizing, and the grouping of questions together. This will soon become hierarchical, and general and specific research questions begin to be distinguishable from each other.

The final stage then involves bringing the project down to size, since it has usually become too big. In fact, it probably suggests a research programme with several research projects by now. How is this trimming done? It is important to decide which questions are manageable within the practical constraints of this project, and which seem the most important. There are of course limits around any project – even if that project involves a grant and a team of researchers. The principle here is that it is better to do a smaller project thoroughly than a larger project superficially. Trimming a project down to size is a matter of judgement, and experience in research has a big role to play here. Once again, therefore, this stage is best done in collaboration with others. This stage is sometimes called delimiting the project. This means drawing the boundaries around it, and showing what is not in the project, as well as what is.

How many research questions should there be? There are practical limitations on any one project, and, as noted, it is better to have a small job done thoroughly than a large job done only superficially. More than about three or four general research questions, assuming that each is subdivided into (say) two or three specific questions, is testing the upper limit of what can be done in one study.

This question development stage is pre-empirical, in the sense that we are not yet really focusing on questions of method, which come later. As far as possible, we are following the rule that we put substantive or content issues before methodological issues. Method questions will always intrude to some extent, but there is value during this stage in keeping them at arm’s length. The questions ‘How will I do this?’ or ‘Should I use this method or that one?’ are important, but the point here is that they can come too early.

As well as delaying considerations of method during this stage, it is wise also to assume that the issues of method can be solved later. There is
(almost) always a way to do what we want to do, even if we cannot at this point see what that way is. It may be that we will need help on some issues of method or technical details, but such help is usually available. It is therefore a wise strategy, through this important question development stage, to assume that a method exists or can be developed for what we want to do, even if we do not at this stage know the details of that method.1

During this planning stage, there is benefit to ‘hastening slowly’. Since research questions do not usually come out right the first time, several iterations are often required, and we only reach an answer to the question ‘What are we trying to find out?’ after careful thought. This question development stage needs time – time to see the possible questions buried in an area and to see related questions which follow from an analysis of particular questions. The set-up stage of the research is important, for the decisions taken here will influence what is done in later stages. This does not mean that the decisions cannot be varied, as when iteration towards the final research questions goes on during the early empirical stages of the project. But varying them should not be done lightly if considerable effort has been invested in reaching them during the set-up stage.

Focusing on what we are trying to find out is useful not only at this stage, but at all stages of research, including research which unfolds. It helps to keep things focused during planning, design and execution of the project – especially during data analysis – and it helps in the writing up of the research report.

3.3 THE ROLE OF RESEARCH QUESTIONS

Research questions are central, whether they are presupposed or whether they unfold during the project. They do five main things:

• They organize the project, and give it direction and coherence.
• They delimit the project, showing its boundaries.
• They keep the researcher focused during the project.
• They provide a framework for writing up the project.
• They point to the data that will be needed.

The third point (keeping focused during the project) requires a comment. Research can get complicated, and it is therefore easy for any of us to get lost on the way through a project. Clearly stated research questions have great value in bringing the research back on track in those situations when complications or side issues threaten to take it off course. Being able to step back from the complications and details, and to refer again to research questions, can be of great assistance.

The last point above, indicating what data will be necessary in the project, is the important empirical criterion, to be discussed in Chapter 4. The idea is that a well asked question indicates what data will be necessary to answer it. This brings up the distinction between a research question and a data collection question. A research question is a question which guides the project, and which the research is designed to answer. A data collection question is more specific again, and is a question which is asked (perhaps in a survey questionnaire or interview) to provide data relevant to a research question.

Chapter 4 also discusses conceptual frameworks. The conceptual framework shows the conceptual status of the factors, variables or phenomena we are working with, usually in diagram form. Developing the research questions often involves developing a conceptual framework for the research as well. These two things do not have to go together, but it can be very useful when they do. This is because developing the questions often brings into focus the (implicit) conceptual framework we are using in our thinking about the topic. When that is the case, it is a good idea to make that framework explicit. The research questions then operationalize that conceptual framework, pointing ahead to the data. In quantitative research, that is usually very clear, and it is taken for granted that the conceptual framework for the study will be shown. Developing a conceptual framework can be very useful in qualitative research as well, focusing and delimiting the study, and giving direction to the sampling decisions which will be required. Examples of conceptual frameworks are given in Chapter 4.

To this point, this chapter has dealt with identifying and developing the research questions, and with their central role in a project. It is time now to consider the hypothesis, and what role it has in research. This is needed because there is some confusion about its role, especially when quantitative and qualitative research are considered together. We will consider what it is, where it fits, and under what circumstances it can help us in research. This is the appropriate point to deal with this topic because of the relationship between hypotheses and research questions.

3.4 HYPOTHESES

Complicated definitions of the hypothesis can be found in some of the older research methodology literature (Nagel, 1961; Brodbeck, 1968; Kerlinger, 1973), but these will not be used here. Instead, I will use the simple definition of the hypothesis as a predicted answer to a research question. To say we have a hypothesis is to say we can predict (at least to some extent and with at least some confidence) what we will find in answer to a question. We make this prediction before we carry out the research – a priori. A specific research question states what we are trying to find out. The hypothesis predicts, a priori, the answer to that question.

This now raises a question about the basis for that prediction. On what basis can we make such a prediction? Why do we expect to find this (what we predict) rather than something else? In general, there are only two answers to this last question. One is: because another researcher did some...
research and this is what was found. While this answers the question, it does not explain the prediction. The other answer to 'Why predict this?' involves an explanation. Propositions are put forward which explain why the predicted answer (the hypothesis) can be expected. We can call this set of propositions a 'theory'.

In this case, we have a theory, which explains the hypothesis, and from which the hypothesis follows, by deduction, as an if-then proposition. If the theory is true then the hypothesis follows. So, in executing the research and testing the hypothesis, we are actually testing the theory behind the hypothesis. This is the classical hypothetico-deductive model of research. In passing, we should note that it shows why theories cannot be proved, only disproved. We cannot prove the if-part (the theory) by validating the then-part (the hypothesis). This is why it is often pointed out that scientific knowledge develops by disproving its theories (Popper, 1959).

Two important points follow from viewing the structure of inquiry in this way. The first concerns the role of hypotheses in empirical research. When is it important to have hypotheses? When we do have an explanation (a theory) in mind behind the hypotheses, even if implicitly. If this is the case, we should by all means formulate hypotheses as predicted answers to research questions, and test them. If not, we can proceed simply with research questions. After all, there is no logical difference between research questions and research hypotheses, when it comes to their implications for design, data collection and data analysis.

Therefore there is a simple procedure for determining whether it is appropriate to have hypotheses. Once we have specific research questions, we can routinely ask, of each one, and before carrying out the research: 'What answer do we expect to this question?' If we cannot predict with any confidence, we need go no further into the matter of hypotheses, and we can proceed instead with research questions. If we can predict, we next ask: 'Why do we predict this (and not something else)'? If the only answer to that question is 'Because some other researcher found it to be true', again we do not need to propose hypotheses. If, however, we really do have some explanation in mind, from which the predicted answer(s) follow, then there is value in proposing hypotheses, and exposing and analysing the theory behind them. In testing the hypotheses, we are then testing the theory. Since the hypothesis has a central role in the testing of theories, there seems little point in putting forward hypotheses for testing unless we can also put forward the theory behind them.

The second implication of this way of seeing the hypothesis concerns the overall structure of scientific knowledge, and takes us back to Figure 2.1. The structure shown in that diagram illustrates the point made above, that a hypothesis is derived from, and explained by, the higher-order theory above it. It also shows the hierarchical structure of knowledge, with increasing levels of power, abstraction and generality towards the top of the diagram, and the central role of empirical generalizations. This view of the hypothesis, its relationship to research questions and to the theory behind it, shows, in microcosm, the same structure. This underlines the point that there are concepts and propositions at different levels of abstraction in a research project, and therefore that there need to be logical links between these different levels of abstraction. We return to this point in Chapter 4, and especially again in Chapters 10 and 11.

Hypotheses are given a very prominent place in some research methods books, especially quantitative ones (for example, see Burns, 1995), but that is not the case here. The sequence of ideas shown above helps in understanding the role and place of the hypothesis, and enables judgements about the appropriateness of hypotheses in a study. If appropriate, we can use them. If not, the study is just as well kept at the level of research questions. In my opinion, there is no point in simply having hypotheses for their own sake. What is useful in all cases is to go through the above questioning sequence, once the research questions are settled, asking whether the answer to each research question can be predicted, and, if so, on what basis.

In this discussion, hypotheses are seen as predictions or answers to specific research questions. When they are appropriate, hypotheses parallel the specific research questions. The same point can apply at a more general level as well. On that level, and again when appropriate, a general hypothesis can parallel a general research question. However, the more general the question, the less likely is a hypothesis. This explains why the hypothesis has been more of a preoccupation in quantitative research, but the reasoning described above can apply equally well in both approaches.

3.5 A SIMPLIFIED MODEL OF RESEARCH

Whether or not hypotheses are appropriate, organizing research around research questions, and insisting that each question conforms to the empirical criterion to be described in the next chapter, leads to a simple but effective model of the research process. It can be diagrammed as shown in Figure 3.1.

This simplified model of research stresses:

- framing the research in terms of research questions,
- determining what data are necessary to answer those questions,
- designing research to collect and analyse those data,
- using the data to answer the questions.

The model in the upper part of the diagram shows research questions without hypotheses. The model in the lower part shows research questions with hypotheses.

Based on this model of research, we can see that two overall questions guide the research planning process. They are also the questions around which the research proposal can be written, and, later and with some additions, the research report. The questions are the straightforward ones of...
3.6 THE ROLE OF THE LITERATURE

The appropriate point in the planning process at which to concentrate on the literature is something which may well vary in different styles of research. In the traditional model of research, the literature is reviewed (often comprehensively) as part of the research planning and question development stage. The literature itself becomes an input to the analysis and planning during this stage. This is the recommended way to proceed in many research situations, and it has been the model typically followed in quantitative research and in some types of qualitative research. At the other end of the scale, we might have, for example, a grounded theory study, where the literature coverage is deliberately delayed until directions emerge from the early analysis of data. The literature is brought in later, and treated as further data for analysis. The reason for this, as explained in Chapter 8, is that the researcher wants categories and concepts to emerge from the data – to be fully grounded in the data – rather than to be brought to the data from the literature. If these two examples are the two ends of a scale, there are obviously points in between, which combine elements of the two approaches.

It is a matter of judgement at what point to concentrate on the literature. Factors involved in this judgement include the style of the research, the overall research strategy, what the objectives of the proposed study are, the nature of the substantive problem and how much is known about it, how well developed the literature in the area is, and how closely this study wants to follow the directions established by that literature. A further important factor is the knowledge that the researcher already has, especially when the research topic comes from practice or experience.

In applied social research, many topics and questions come from the world of professional practice, and are often set in organizational, institutional, community or public contexts. Often, also, the researcher is a professional practitioner, or closely connected with professional practice, in that setting. In such a situation, the researcher has considerable knowledge about the topic, before commencing the research. This knowledge can be used as a starting point for the question development work described earlier. This involves exploring and articulating that knowledge, an activity which will often be valuable to the individual in encouraging reflection about the issue. For the research itself, this knowledge is a valuable input to the research planning process. Maxwell (1996: 27–9) offers some specific suggestions for dealing with ‘experiential knowledge’, and gives the example (1996: 30–1) of an ‘experience memo’ on the subject of diversity.

In such cases, there is often some benefit in delaying for a while the use of the literature, during the question development stage. There is benefit, in other words, in doing a certain amount of work on developing the questions (and perhaps the conceptual framework) before consulting the literature. This is because the literature will influence that process, and we may want to minimize or delay that influence. Of course, the literature can
be a fruitful source of concepts, theories and evidence about a topic, but it can also influence how we look at a topic, perhaps thereby precluding the development of some new way.

This topic of the use of the literature is taken up again in Chapter 8, when grounded theory is described. As noted there, when research problems and questions are embedded in professional practice and experience, the logic of the grounded theory approach, with its typically delayed approach to the literature, is appealing. Whatever the style of research, however, the point at which and the way in which the literature is dealt with are matters for judgement in the light of the overall logic of the study.

NOTES

1 This term means that some topics are 'hot', and offer an opportunity: 'for example, you read that there is a lot of money available to conduct research on nursing homes, but few people are interested in doing so.' Your need of a job suggests nursing homes as a topic' (Neuman, 1994: 110).
2 A fruitful source of research topics is also a university (or departmental) dissertation or project library. It is common practice in these projects for the writer to suggest avenues and areas for further research, making a collection of such projects a valuable resource of research topics and questions.
3 Sometimes the literature will talk about research problems rather than research questions. The distinction is academic, and has no practical significance in planning research. A problem requires a solution, and can always be phrased as a question. A question requires an answer, and can always be phrased as a problem.
4 Miles and Huberman are more ambitious: they suggest that more than a dozen or so general questions is 'looking for trouble' (1994: 25).
5 This point applies especially to measurement and statistical analysis issues in quantitative research, and to data analysis issues in qualitative research.
6 To claim to do so is to commit the logical fallacy of 'affirming the consequent'.

Further Reading

RESEARCH QUESTIONS