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Designing a Qualitative Research Project

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10.1 Introduction

It should be clear from the very beginning of this chapter that this will not be a detailed step-by-step guide to presenting a research problem and designing a study. It would be impossible to create such a guide owing to the varied nature of qualitative research: the methods used, the accepted paradigms, and the possible areas of application. Designing and conducting research is a craft—it can be mastered through practice. It can be learned by observing the work of experienced researchers, or through a close reading of reports and papers based on research. However, the key way to achieving excellence is our own practice and, once the project is finished, reflecting on how the design and performance could have been improved; in other words, through a critical and reflexive approach to our own work.

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If designing qualitative research is a craft whose rules cannot be fully codified, then what can this section offer to the readers? Despite the diversity of legitimate approaches to the theory and methods available to qualitative researchers, we can identify questions which one must always, or virtually always, answer when formulating the problem and designing research. Steinar Kvale (1996) notes that at this stage the project may at times require a kind of an explorative interview with a researcher, which will help us better understand why, how, and what we want to do. Following this line of reasoning, we will suggest certain outlines for an interview we can carry out with ourselves at this stage of research design. We will try to identify the most important questions to ask ourselves while designing the study, as well as a few suggestions as to where the search for answers to these questions can begin. This is perhaps the most accurate type of a universal guide to qualitative research design that can be offered.

There are many more recognized procedures in qualitative research than in quantitative, and the rules for assessing its quality are not standardized (Flick 2008b). It should be noted, however, that the multitude of correct solutions should not lead anyone into disregarding the design stage of research; on the contrary—we should put effort into it all the more. This flexibility means greater responsibility for the final result, and a greater number of potential traps. In order to avoid them, we should demonstrate reflexivity and consistency.

By **reflexivity** we understand an attitude in which we consider what is investigated, how, why, and what for; we consider our role and professional responsibility, and we can indicate our assumptions as well as describe any involvement which may affect the findings. It is important here to understand the theoretical assumptions which we adopt when formulating the problem, and on which the methods are based. This attitude does not appear right away, we usually start from an interest in a wider area or phenomenon, and full clarity—the condition for creating a good research project—comes from work in the stage of formulating the problem: comprehensive discussions with yourself, with other researchers, and with experts outside of academia, as well as a thorough literature review.

By **consequence** we understand taking care that our research conduct results from how the research topic is shaped conceptually, so that the carefully thought out research question can drive all the other steps of our proceedings. In formulating the research problem and deciding on a specific theoretical approach, we consistently choose the right tools and techniques for data collection and analysis, as well as for presenting research findings. Properly understood consequence does not mean rigidly sticking to the original conceptualization and remaining oblivious to what is discovered in the data—depending on the selected theoretical approach, we have the ability to reformulate the original problem, put forward a new one, abandon some assumptions, or adopt additional ones. Consequence can therefore be maintained either by preparing a strict research project and aiming at checking the original hypothesis or gaining knowledge about a previously defined phenomenon or process from the very start, as well as by choosing the “funnel approach,” according to which the area of our interests is very broad to start with, but we assume that categories relevant to the project will emerge only during the data collection and analysis. The latter approach can be characterized by consequence too, as long as its choice comes from a good understanding of the problem and the accepted theoretical perspective, and if we are aware of the procedure for collecting and compiling data, which will help us get closer to the findings.

The next sections of this chapter will present the subsequent stages of the research design journey, starting from a vaguely defined area of interest, and ending with a detailed research project proposal. Let us first discuss the questions we face when formulating the research problem—those helping us to reflexively approach the research. We will then describe issues that need to be resolved when matching to the problem research methods which help us to be consistent. In the last part of the article we will discuss the decisions which need to be taken when planning the details of research project implementation, including ethical issues (although we should remember that they are relevant in each of the discussed stages).

10.2 The Theory and the Problem, or Formulating the Research Question

The list of issues which may occupy researchers undertaking qualitative research is practically infinite, and their diversity is huge. A few well-known examples, like the research of Thomas and Znaniecki on Polish peasant migrants, of Anselm Strauss on terminally ill patients, of Paul Lazarsfeld on communities affected by unemployment, or of Erving Goffman on “total institutions,” can help us realize the wealth of such research—and the list continues. However, each of these examples shows that a good qualitative study—that is to say one which develops our understanding of the social world beyond a reasonable doubt and more often than not has significant practical implications—is driven by the research question.

However, we usually set off not from an elegantly worded problem, which can be found in the introduction to a strong article by an experienced researcher or in an outstanding book, but a vague interest in a field of knowledge or a social phenomenon. Of course, when looking for a research topic, it is worth focusing on what fascinates us—commitment to a problem significantly increases the project’s chance for success, and the motivation necessary to go through the series of stages does not fade (Morse 1994). However, finding an interesting field is only the beginning. The topic then needs to be formulated so that others can also see its importance—to demonstrate that the question is not trivial and to make the issue “researchable”—to form the question in a way that allows finding an answer by using the methods at our disposal and convincing the reader that the answers we can deliver will be reliable. Achieving this state usually requires gradually narrowing down our interests: selecting such a slice of reality from our area of interest will show as much as possible the problem which intrigues us and at the same time will embed the research in a specific environment.

To come closer to this aim, we may want to find answers to the basic questions: (1) **what** is it that we are studying? (2) **for whom**, or **why** are we studying it? (3) **what** are we studying it for? (4) **how** are we going to study it? We do not usually ask ourselves these questions sequentially;

moving one step in any of the highlighted areas helps us to find more accurate answers to the other. For example, the answer to the question “what are we studying?” becomes more precise when we already know how and where we are going to study it (cf. Stake 2010, p. 74). Let us look more closely at the said questions (Table 10.1).

The question “what?” is the fundamental question about the research problem, formulated in the language of the specific, structured theory, using abstract theoretical concepts or existing within a defined theoretical approach, which (as with, for instance, the grounded theory strategy) provides the rules for a conceptual recognition of the world. There is no room in this chapter to discuss in detail the controversies associated with the concept of theory in qualitative research methodology (see, e.g. Flick et al. 2004 or Maxwell and Mittapalli 2008 on “paradigmatic conflicts”). Nevertheless, we need to be aware that there are several patterns of relation between qualitative research, its findings, and social theory. The pattern we choose will greatly affect all the subsequent steps of the proceedings, and so the question of the relation to the theory should be one of the first we answer. What possibilities are typically used?

It sometimes happens that the aim of a qualitative study is to confirm, refute, develop, or refine a systematic theory under certain conditions—however, it is not common. It is much more likely that, when designing the study, we choose assumptions and methods associated with a particular theoretical perspective. They allow us to describe the world and present the problem conceptually, but not necessarily to propose and test hypotheses. An example of such a theoretical perspective can be derived

Table 10.1 Questions aiding the design of qualitative research

What?	For whom?/why?	What for?	How?
Problem Theory	The world of academia	Instrumental knowledge	Unit of analysis
	The world outside academia	Reflexive knowledge	Case selection validity (generalizability)
	Theoretical significance		Accuracy
	Practical significance		Reliability
			Tools
			Data collection and analysis techniques
			Implementation resources

from linguistic studies on critical discourse analysis, which deals with the role of language in society or ethnomethodology, focusing on the study of routine ways to build a common world of meanings.

It may also happen that researchers use abstract theoretical concepts, such as power or memory, using the achievements of various theoretical traditions and treating them eclectically, and looking for significant contexts to interpret a particular phenomenon, rather than a systematic theory with assertions, hypotheses, and designated methods (Cheek 2008; Jasanoff 2004). Such use of concepts and relationships between them also helps in early problematizing of the research subject and going beyond a mere description of the phenomenon.

Yet another approach assumes that it is only at the stage of determining the research aim that a medium-range theory is created, based on the regularities emerging from the data, as in grounded theory and related approaches. Although in this case our approach is usually much more flexible, choosing this road does not exempt us from work in the research design stage. According to Uwe Flick (2008a), the days when you could go into the field and “just do it” are over also for qualitative researchers, owing both to the enormity of the research conducted to date and to the development of theories supporting the methods used. Also Valerie Janesick (1994) emphasizes that the research question is essential in directing research design, although she advocates flexible research design.

Even if theoretical issues are not the most important when addressing our research problem—for example, the fact that the research is clearly oriented to practical goals, as are many studies supporting public policies through providing diagnostics or evaluation—we have to keep in mind that there is no research without theory. Even work focusing on the practical significance of a problem uses theoretical assumptions, as the choice of the methods or legitimacy of the questions is only possible if we assume a certain way in which social reality exists. The less time we spend on considering these issues, the more we are forced to rely on colloquial, common sense social theories or research perspectives, to which we are accustomed and which we routinely accept. It is worth noting that this approach might reduce the quality of our research.

A useful overview of the various approaches to research is proposed by Flick (2008a), who calls them “research perspectives.” Examples of a

popular perspective in qualitative studies may be the approach aimed at reconstructing the point of view of the research subjects, rooted in the tradition of symbolic interactionism and phenomenology, or the approach whose objective is to describe the mechanisms of constructing social situations, related to constructivism and ethnomethodology. As an example, let us consider research into the institution of prison. A researcher conducting studies within the first perspective could reconstruct the ways prisoners experience and make sense of the situation of forced detention, on the basis of in-depth interviews. Within the context of the second perspective, they would describe the implicit hierarchy forming mechanisms, supplementing interviews with in-depth observations. How we formulate the problem and how we use the qualitative methods we have selected is usually linked to the context of one of these traditions. In the following chapters of this book we will also see clearly that certain theoretical traditions are associated with specific methods and means of formulating problems; similarly, presented qualitative research methods have their theoretical assumptions and types of problems which correspond to them the most.

Theory (understood here as broadly as possible) helps us to avoid answering the question: “what are we studying?” too superficially, to problematize the focus of the research, and to translate it into research questions. It is difficult to provide a universal recipe for a good research question, but it is possible to point to examples of recurrent research question types (Table 10.2), with the reservation that the list is by no means exhaustive.

Charles Tilly (2006) suggests accurately assessing the parameters of the research problem: its scope of conclusions and range of contribution, when formulating the problem. The larger the scope of conclusions and range of contribution, the greater the risk that the research will fail and the higher the expectations faced by researchers, but also the greater the potential cognitive benefit and scientific success. Tilly shows that by placing the research question in the context of existing arrangements, we can set ourselves various goals—from relatively simple ones, such as filling factual gaps in the existing knowledge, to challenging both the answers given and questions posed so far. Finding your own questions on the coordinate system proposed by Tilly requires gaining a good orientation

Table 10.2 Selected types of research problems

Research matter	The involvement of parents in the school education of their children	Commemorating the Second World War
Research questions		
Description/exploration of behavior	What are the different forms of parents' involvement in the school education of their children?	What different forms of commemorating the War exist at different levels (private, state, religious spheres)?
Factors affecting behavior	What factors affect the diversity of forms of parents' involvement in the school education of their children?	What factors affect the diversity of forms of commemorating the War?
Meanings assigned to behavior	What values or meanings do the parents attribute to the diverse forms of involvement in the school education of their children?	What values or meanings are attributed to the various forms of commemorating the War?
Description of behavior contexts	What external conditions (historical, environmental, institutional) help or hinder certain forms of parents' involvement in the school education of their children?	What external conditions (historical, environmental, institutional) help or hinder certain forms of commemorating the War?
Changes over time	Have the forms of parents' involvement in the school education of their children changed over time (and how)?	Have the forms of commemorating the War changed over time (and how)?

(continued)

Table 10.2 (continued)

Change over time due to intervention (the problem of <i>ex post</i> evaluation)	Have forms of parents' involvement in the school education of their children changed (and how) due to intervention (e.g. a parent support program)?	Have forms of commemorating the War changed (and how) due to intervention (e.g. a historical policy program or the construction of the particular museum)?
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Source: Authors' own adapted from Schensul (2008)

in the existing literature. This stage—particularly time-consuming when entering a new field—is necessary in the process of posing a good research question.

The question “why?” asked before conducting the research justifies the significance of the project. There are two basic ways to convince ourselves and others (including colleagues and members of grants committees) that the problem is worth their attention, their time, and the money required to conduct the research. We can argue either for its theoretical or practical significance. These two ways of justifying the importance of research are not, of course, fully separable; however, we usually lean toward one of them, which drives the research design as well as the way the study is implemented later on. While theoretical significance is an essential criterion of excellence in academia, the assessment of the practical importance of research in social sciences generally requires referring to problems defined externally (mostly by business, politics, or public administration). Hence the question “why?” should be considered together with the question “for whom?”, which asks about the recipient or purchaser of the project.

At the research design stage, the questions “why?” or “for whom?” should serve the thorough consideration of the involvement of the researcher, sponsors, and parties commissioning the research. This involvement also impacts the way the problem is formulated, as well as the specific criteria for assessing the project's success. Ethical or political involvement does not have to disadvantage the research—within certain paradigms, such as feminist theories, or certain contexts such as advocacy, it is indeed essential (see Chap. 5 by Davydd Greenwood in Volume 1 on

the *action research* movement). However, it should always be conscious and clearly declared, and we should also anticipate what deviations may be expected due to this involvement.

The question “what for?” asks about the expected results of the research and about its purpose. The answer to this question can be considered in terms of the concrete findings of a research project, such as a book, a scientific paper or a popular science article, expert evaluation, or a successful social intervention; however, we can look for more abstract answers, considering, for example, the question of the significance of research findings for social change or, conversely, for the maintenance of the existing social order. Finally, the answer to the question about the obligations of the researchers and the consequences of their work for the recipients of the research as well as the wider community opens the field to relevant ethical considerations.

The importance of the questions “who?” and “what for?” is convincingly argued by Michael Burawoy (2007), who calls on the representatives of the social sciences to constantly reflect on the type of knowledge they produce. The answers to the questions “who” and “what for” allow us to indicate the types of knowledge which may differ considerably in terms of truth criteria, validation methods, attitude to policy and politics, accountability criteria, as well as deformations and pathologies.

The recipients of research findings can be specialists either in a particular field of science (“scientists speaking to scientists”) or in environments outside of academia, mostly politicians, officials, or businessmen. Research, argues Burawoy (2007), can be used either to select appropriate measures to put into practice our a priori objectives, or to assume that the findings themselves can specify or change certain aims; in other words, knowledge can be instrumental or reflexive. Depending on the answers to the questions “for whom?” and “what for?”, we can distinguish four types of knowledge resulting from research: professional, expert, critical, and public knowledge. For example, an ex post evaluation study of a support program for the unemployed, commissioned by a state agency in a tender, will generate knowledge most similar to the sociology of expert knowledge, useful in public policy, and in solving problems defined by the client. Social studies, allowing the intervention of the person leading the researched social process, such as Alain Touraine’s well-known study

on the Solidarity movement (1983), are closest to the model example of involved public sociology. The work of researchers focusing on professional sociology, communicating their findings and achievements, primarily to colleagues from the same field through publications in journals and academic publishers, contributes to the development of the discipline and to a large degree remains the source of the standards of research and researcher excellence—this group includes most scientific papers, providing reliable knowledge on the researched phenomena and an example of efficiently applied techniques for research and analysis. Finally, critical knowledge, also addressed primarily to professionals, reveals and renews the political, ethical, and methodological foundations of sociology, pointing to new goals and new ways of practicing the discipline. This category includes works of Max Weber, Pierre Bourdieu, or Bruno Latour.

The last of the key questions at the research design stage, the question “how?”, asks in fact about the research methods and the conducting of qualitative research. We consider these issues in the next sections of this chapter.

10.3 The Problem and the Method, or Method Selection

The consistency of qualitative researchers manifests itself in how they match their research methods to the problem in hand. Detailed guidance on what methods are the most suitable for specific issues is provided also by the next chapters of this handbook. Here we will focus on methodological issues common to various research methods; issues toward which each method, and each person applying it, must take a stand at the design stage of a research project.

It is worth perceiving a research method as a whole: a systematic collection of theoretically rooted principles for the collection, analysis, and interpretation of data. These principles are largely determined at the design stage of a research project. Researchers make use of standard schemes, tested tools, and repetitive patterns, but at the same time create a unique procedure, tailored to the particular research questions and

data. In qualitative research the method is adapted to the problem—and to the researcher. We can therefore say that it is very difficult (if not impossible) to apply the same method to examine a different fragment of social reality. We then write that researchers “develop a method”—as usually they do not invent a new, innovative research method but also do not apply it in exactly the same way as it was used previously.

A situation in which we choose the method before we understand what question we want to answer is definitely not beneficial, although such a temptation appears especially when we have mastered a selected method or when we simply “like it” (Stake 2010). However, Uwe Flick (2008a) notes that non-arbitrariness matching the research method and scheme to the problem—in other words, the fact “we do not chose a qualitative perspective, a specific approach within it, or specific methods due to a general affection we have for them”—is one of the crucial aspects of evaluating the quality of a study.

Posing the problem does not only specify what the focus of the research is going to be, but usually refers also to the issue of what will constitute the **unit of research**. In social research, this may be a person, a social group or network, a formal organization, a statement as a discourse unit, a photograph, and so on. Formulating the problem usually limits also the collection of research units. At this stage of research design it is necessary to go one step further: to develop a case selection method for a qualitative study. It is not sampling in the same sense in which a random sample is selected for a survey—most of the time, it is referred to as **case selection** or **theoretical selection**, driven not by principles of probability but by criteria deduced from theoretical assumptions.

Following Michael Patton (1990), we can refer to the most frequently used case selection models in a qualitative study. We can select **extreme or deviant cases** (e.g. to study how illness is experienced, we can choose to interview patients who are terminally ill or suffer most acutely, and to study the social impact of unemployment—a village where almost the entire population have lost their jobs, like Marienthal in Lazarsfeld’s research); **intense cases**, where we select people with extensive experience in a specific situation, but we do not focus on extremes (e.g. when researching adoptive families, we choose the cases of families who have decided to adopt several times); or **maximum variation cases** (e.g. when

studying politicians we choose subjects who differ in age, gender, party affiliation, professional experience, or other characteristics important from the point of view of the problem). At times, we choose **critical cases** which allow us to generalize the claim we are proposing, or make it easier to decide which of the proposed explanations is the most likely. The choice of critical cases is based on similar principles as that of **disconfirming cases**, which could contradict our claims—in this way we can explore the limits of the generalizations we propose, or create more subtle classifications. More about case studies in Chap. 1 by Marta Strumińska-Kutra and Izabela Kołodkiewicz, in this volume.

It is also worth mentioning a case selection method related to the last two, advocated by Glaser and Strauss (1967). They propose that cases for research should be chosen through **constant comparison** during data collection and analysis, associated with a concept which is key to grounded theory: **theoretical saturation**, a stage of research where the analysis of subsequent cases does not enrich our understanding of analytical categories and the relationships between them—which is a signal that data collection can be concluded. The concept of theoretical saturation may provide answers to a difficult question of how many cases should be examined in order to achieve a convincing result; however Ingeborg Helling (1988) rightly points out that all too often researchers consider this concept as a skeleton key, limiting themselves to “mention of the theoretical saturation process,” which is “not sufficient for the reader to assess the outcome.” What is more, achieving this stage may be more due to the research area (and, once more, a well posed and adequately narrowed down problem) than the case selection method itself.

When considering the question of case selection, we must also keep in mind the practical limitations of access to research subjects. For categories of research units to which access is difficult, the ultimate criterion for selecting one or more of them may be only their availability; this can often reduce the value of research, especially if we want to formulate some kind of generalization, as it is very likely that the only accessible cases have some common characteristics, which makes them systematically different from the inaccessible ones. For example, if we aim to examine the life of prisoners in an authoritarian country and we obtain permission from the authorities to examine two prisons indicated by

those authorities, we can expect that the findings will not represent the life of other prisoners.

Many methods assume the use of **research tools**, such as observation sheets, interview scripts, code books for content analysis, and so on. At the research design stage it is often possible to develop the first versions of the research tools, such as guidelines for in-depth interviews. When developing the tools, it is important to check whether their content corresponds to the content of the research question. The omission of relevant aspects of the research question in the research tools will mean that giving a complete answer to them will not be possible. Preparing well thought out research tools is especially important when working in a group, or when data collection and analysis are the responsibility of people other than the person who has designed the study.

As mentioned earlier, the question of assessing the quality of qualitative research is subject to a lively debate (cf. Lincoln and Guba 1985; Tobin and Begley 2004; Flick 2008b). While quantitative research is governed by fairly explicit rigors of relevance, reliability, objectivity, and generalizability (which are often measurable), in qualitative research these criteria are not unambiguous. Once more, one of the key reasons for this state of affairs is the diversity of theoretical approaches and applied methods. Developing clear, universal criteria would have to be connected with standardization—with the loss of essential benefits stemming from the application possibilities of qualitative research. However, it is possible to indicate questions that relate to the suitability of the method for a given problem, and will help us to ensure a high quality of research.

The first is the question about how using a particular method can lead us to examine what we have declared; in other words—whether all the aspects of the problem we have posed will be described. It is also a question about whether the method directs us toward real—or at least reliable—data (e.g. group interviews usually will not yield reliable data on how to hide old age from others).

The second question is concerned with the impact of the person conducting research on the research findings. In qualitative research, the concept of reliability is not typically considered in terms of replicability of findings by other researchers. The relation between the researcher and the research is strong enough to measure reliability by revealing all the

relevant factors related to the research situation which may have contributed to affecting its findings. It is also important to reliably present the stages of the research process and the collected data, and so allow the reader to assess the conclusions drawn by the author.

The third crucial question refers to the scope of validity of the project's findings. Yvonna Lincoln and Egon Guba (1985) postulated that instead of asking about the generalizability of findings in qualitative research (i.e. the possibility of their generalization), we should ask about the possibility of their legitimate transfer to another cultural context. We do not need to ask, therefore, a nonsensical question about whether the results of unrepresentative (in a statistical sense) qualitative research relate to a smaller or larger population. It is replaced with the question of whether the results of qualitative research can bring anything new to the understanding of a fragment of social life in a different place or time. In a similar context, Robert Yin (1994) suggests that the findings of qualitative research should be subjected to analytical (theoretical) generalization, rather than the statistical generalization characteristic of quantitative research.

To sum up, since there are no universal guidelines that would facilitate the selection of a suitable method for each problem, we need to remember to thoroughly consider matching methods to the problem, to reliably present not just the findings, but also the data, and to deliberate on how the chosen method and case selection will allow us to determine the scope of validity of the findings, or whether they will allow us to transfer the conclusions to a different context.

10.4 The Resources Necessary to Conduct Research

The implementation of qualitative research should be carefully thought through as early as in the design stage. It is worth considering how to mobilize different resources: researchers, time, money, equipment, contacts, and so on. At the research design stage, we need to carefully assess what resources will be needed to carry out the study, taking into account the constraints, both external (applicable laws, the code of ethics, the

hierarchy of priorities in grant competitions, the grant amount), or which are set by researchers themselves (such as the budget records and project timetable) in order to effectively manage the project and bring it to a successful conclusion.

Flexibility of qualitative research, mentioned above many times, does not mean that qualitative research can be freely “redesigned” if ill-considered implementation constraints are discovered: a “redesign” of a study is a burden to the person leading it. The flexibility of qualitative research is worth using when in the course of research we gain unexpected, interesting data, which prompt us to describe new aspects of a given phenomenon; the “redesign” of the study is then proof of our craftsmanship and sensitivity.

When thinking about the research implementation stage, we should first of all take into account the following limitations: temporary, financial, legal, ethical, and competence (related to the limited skills of researchers). Let us look at them briefly. A research project, like any project, must have a designated end. The end of a project is normally defined in two ways: indicating the expected date of its completion, and listing the results which should be achieved before the deadline. When directing a project, we try to achieve all the promised results in the allotted time (or earlier). This is made easier by a **timetable**—in the simplest version, it is a breakdown of all the research activities into stages, and the assigning to each of them of the time needed to work on it. More complicated timetables (e.g. Gantt charts) gather information also about the links between stages, both of the division of responsibilities among the researchers and of the time when individual resources should be mobilized. The more complex the research process (e.g. requiring the involvement of dozens of researchers in various stages of the research, or specialized equipment), the more useful it is to have a complex schedule. When planning a study, researchers should consider what other time commitments they have (e.g. whether they are or will be engaged in other projects), and how the passage of time affects the studied problem. It is basically a question about what is the best time to carry out the study.

The time availability of the subjects is also important (e.g. some educational research is impossible to conduct during the holidays; researchers of administration staff know that recruiting busier respondents for

interviews takes longer, etc.), as are foreseeable events that can affect the results (e.g. when researching politicians, we need to take into consideration the dates of elections). It may happen that we decide on a research environment which for some reasons is difficult to access: whether in institutions reluctant to allow research, or in disadvantaged environments, where an attempt to acquire the necessary trust can be time-consuming and the effects uncertain, or when searching for subjects with specific experience, who may be difficult to identify on first contact. In this situation, we must make sure that the time required to gain access will not affect our schedule.

Also, a research project has its **budget**, into which financial constraints are written—more often than not it is the result of a difficult compromise between the researcher and the institution funding them. The more detailed the budget records, the less flexibility in spending money and the more important that the researcher is cautious and meticulous at the research design stage. This is when we should calculate the costs involved in the project, for example: what materials (books, reports, database access) and equipment (voice recorder, camera, computer software for qualitative data analysis) need to be bought? What salary for those employed in the project should be allocated for individual research activities (designing tools, database queries, interviews, transcripts, and creating reports)? Do we need to provide remuneration for the research subjects, and if so, how much? How much will it cost to support field research: travel, accommodation, and food allowance?

When designing a study, we must also consider legal restrictions—mostly those associated with access to research subjects, institutions, and to data. In some studies it is necessary to obtain formal permission to carry out research and meet internal conditions (e.g. in studies of the organizational culture of companies, the rules of a researcher's stay in the company are usually clearly defined; in studies of the religious experience of nuns, we should expect that our visits to the convent will be limited by its rules).

At the stage of planning research in detail, we should once more consider any **ethical dilemmas** and the special responsibility toward people and institutions subjected to the research. The most important rules to be observed are: the prohibition of causing any harm to the research subjects,

the voluntary nature of participation in research combined with full information about the purpose and course of the study (and so the prohibition of misleading research subjects as to the purpose of the study, and preventing situations in which the subjects do not know that they are the object of research and cannot refuse), the right of the participants in research to withdraw from research at any time, and the right to anonymity and confidentiality. Special care should be given to research into “sensitive” subjects: children, the sick, or those living in difficult conditions (more on this in Chap. 11, *Ethics in Qualitative Research*, Volume 1).

Equally important are the rules related to scientific integrity, which, although they derive from the requirements of academia, can be inferred from obligations to the research subjects. This includes, for example, a fair analysis, preventing conscious omissions, and avoiding the collection of non-significant data which will not be used in analysis, or is related to sufficiently well described phenomena (Flick 2008a, pp 70–75). In other words, we need to consider if “using the research subjects” makes sense and whether or not it is an unnecessary burden for them. This question takes on particular importance if we expect that the topic or course of the study will in any way be a psychological burden for the subjects, as in the case of research on experiencing loss or illness trajectory.

Many ethical dilemmas arising during the research design stage are solved using common sense. However, it is worth confronting common sense solutions with the existing qualitative research ethics guidelines (see Chap. 11, *Ethics in Qualitative Research*, Volume 1).

When writing about the competence constraints, we refer to situations in which the researcher lacks the knowledge or skills necessary for the proper execution of the study. In the research design stage, it is worth checking what knowledge and specialist skills the implementation of research will require, and whether we can make up for these deficiencies in the course of the project. We do not mean just an extensive reading of the subject literature. Taking up some research problems requires, for instance, knowledge of the foreign languages in which source documents are drawn up, or which are spoken by potential respondents. Other topics may require detailed knowledge of the law in a particular field of social life. Some technical skills (such as familiarity with software for encoding visual data) can significantly speed up the stage of analyzing the collected

data. If possible, we might want to set ourselves goals slightly exceeding our competence resources, so that each project will also be an opportunity to learn.

Once we have considered all of the implementation difficulties, we can begin the thorough preparation of a **project proposal**. The idea for the study usually needs to be written down according to the specific requirements of any/the grant competition organizers or scholarship sponsors. Although such documents exist in a wide variety of forms, we can attempt to create a model structure of a research project proposal using the most frequently repeated elements (see Example 10.1). At the stage of writing a research project proposal it is worth using the guidance of experienced researchers and submitting successive drafts of the document under critical assessment of colleagues dealing with similar themes.

Example 10.1 The Research Project Proposal: Based on Schmitter (2002)

1. **Introduction**—a brief description of the project, the project in a nutshell getting the reviewers' attention. It must contain information on what we are examining, using what methods (without details), and why (what inspired the research, what the findings can achieve, why this is an important subject).
2. **State of the knowledge**—incorporating the research idea into the existing achievements of scholars. This is the place to review the key literature, both theoretical work and empirical studies. It is worth writing here about deficiencies in the knowledge ("blank spots" which our research can fill). Schmitter points out that the literature review gives reviewers an idea of the candidates' erudition and of the research traditions they want to refer to (or which they are opposing).
3. **Description of the research problem**—determining the focus of the research, and on what grounds it becomes its focus. Schmitter advises us to separate, as clearly as possible, what we are examining (describing, questioning, explaining, etc.) from theoretical assumptions (which we accept and which will not be studied directly) in this part of the proposal. This is also the place to present possible explanations and to consider their validity.
4. **Methodology or operationalization**—a systematic description of the research tools and activities necessary to achieve the result. This is the place to describe the methods, namely: case selection, and data collection and analysis, as well as to justify the choice of these methods, and

indicate their limitations. It is also where we can refer to the problem of the reliability and accuracy of research methods.

5. **Project feasibility**—considering the possible problems with data availability. An optional part. Schmitter suggests that it can be included in a project whenever we expect problems with implementing the research.
6. **The importance of project findings**—a forecast of what the research findings may change, and what new paths of exploration they can open. There are two essential questions: what new knowledge will we gain? What will be the scope of validity of the project's findings? This part should refer to the deficiencies identified in the literature review.
7. **Bibliography**—the list of key works used to prepare the project. Schmitter emphasizes that for many reviewers this is a crucial part of the proposal—it is important to compare whether an ambitious, comprehensive research idea is accompanied by an equally ambitious, comprehensive subject literature review. He also points out that the reviewers may see it as valuable if the reading list is “unusual,” indicating the author's own explorations beyond the canon.

10.5 Conclusion

Posing a good research question—weighty and researchable—and developing an appropriate research method pave the way to success for the entire project. The later stages of data collection and analysis, as well as the presentation of the findings to a large extent depend on how early we have shown reflexivity and consistency. A well-prepared research design is a detailed map which will help us direct the study, but it is also a ticket to implementing its intentions—usually, the decision to grant funding for research is based on a project proposal formulated in writing.

At the end of this chapter, we present a questionnaire proposed by Charles Tilly (2006), used to assess the degree to which a research project has been considered. We hope that after reading this chapter those who are designing their projects will find it a little easier to answer all of Tilly's 10 tough questions:

- What main questions will your study address?
- Why, how, and to whom do those questions matter?
- What sorts of answers to those questions are worth considering?

- How will your study address the questions?
- What form will the evidence take?
- What are some possible conclusions from the evidence?
- What are the main technical problems you will have to solve?
- What are the main conceptual problems you will have to solve?
- What are the main theoretical problems you will have to solve?
- What are the main practical problems you will have to solve?
- Where will you start? Why there?
- What form will the final product(s) take?

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