The Two Parts of the Research Process or Inquiry in Social Science: The Prestudy and the Main Study

PHASE 1: THE PRESTUDY, OR EARLY THEORIZING

Observe—and focus in on something interesting or surprising to study.
Build out the theory (name the phenomenon; develop concepts, analogies, types, and so on to capture the process, pattern, and such involved).
Complete the tentative theory through an explanation.

PHASE 2: THE MAIN STUDY, OR THE PHASE OF MAJOR RESEARCH AND JUSTIFICATION

Draw up the research design based on the research question.
Execute the research design and theorize again.
Write up the results.

Comment: By including the prestudy as an organic part of the research process, a larger space is allotted to theory and theorizing. After having established what one wants to study through observation, one proceeds to give body and structure to the tentative theory in order to capture the process, pattern, and so on of the phenomenon in question. This is done through a process that has many elements to it, such as naming, constructing concepts, and coming up with an explanation. I refer to this process as *building out the theory*. The element of theorizing that goes into the main study is discussed in chapter 10.

The next few chapters contain a discussion of the key activities that make up the prestudy. Their purpose is to help the average student in social science to theorize in a creative way. The project of theorizing is not a utopian project that has as its goal to somehow turn every student into a pathbreaking social scientist. Its goal is instead to raise theory to the current level of methods. Just as students today can develop a competence in how to use modern research methods, so they should be able to develop a competence in creative theorizing.

CHAPTER 2

Social Observation

Sherlock Holmes: "I have no data yet. It is a capital mistake to theorize before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts."

—Sir Arthur Conan Doyle, "A Scandal in Bohemia"[1]

In order to theorize well, as Sherlock Holmes explained to Dr. Watson, it is absolutely necessary to have facts. This chapter will elaborate on this point and also discuss what kinds of data are helpful to theorize well in social science. It is not, however, a chapter in methodology.

At the early stage of the research process the emphasis is on getting a good empirical sense of the phenomenon you want to study, in order to theorize it in a preliminary way. What is at issue is *not* to study it in the rigorous way that is necessary once the research design has been constructed and the main study is carried out. But again, without any data at all, it is impossible to theorize well.

Whether the main study will be statistical in nature or consist of a few case studies is of less importance at the stage of the prestudy. Getting a good sense for what you want to study through observation is as indispensible for the one as for the other. But it is also natural for scholars with different orientations to end up with different mixes of qualitative and quantitative data.

It must be emphasized that what matters at this stage is not only to collect data but also to collect *social data*. This means data about social life, about what happens between people who live in groups, communities, and societies. It is not a trivial task to be able to distinguish between data in general and social data. How to prepare for this—how to develop a good eye for what is social—will be discussed in the second part of this book. And so will the need to know some social theory when you theorize.

The way you observe at the early stage of the research differs from the way you do it later. What matters at this point is to get a real sense for the topic so that you can develop some good ideas, which may later be used to construct a theory. This means, among other things, that you have to open yourself up to what is happening, with all your senses as well as with your subconscious.

Durkheim's basic rule for how to observe is as valid for the prestudy as for the main study. This is that the social scientist must begin the research process by acknowledging that he or she does not really know anything about the phenomenon that is to be studied.

The kind of knowledge that you might have picked up here or there about some topic is superficial and of little value, according to Durkheim. It is absolutely crucial for the student of social science, he says, to realize his or her "*complete ignorance*" of what is to be studied (Durkheim 1982: 246; emphasis added). Or to phrase it in other words: *It is always different from what you think!*

The term that Durkheim uses to describe the kind of knowledge we all have before having studied something seriously is *preconceptions* (*prénotions*). It is basically a kind of knowledge that comes from living and operating in society. The rule for the social scientist is "*all preconceptions must be eradicated*" (Durkheim 1964: 31).

Durkheim's ideas about preconceptions are, to repeat, as valid for the prestudy as for the main study. They are also useful in that

they make it easier for the social scientist to avoid the choice between empiricism and general theory. They do this by pushing the analysis beyond what exists on the surface, and this forces the social scientist to realize that a new conceptualization is often needed to account for the facts. It is precisely this that makes it easier to avoid falling into the trap of empiricism as well as general theory.

Back to the Use of Theory in US Sociology

The danger of ending up with either the empiricist kind of social science or with the abstract kind of social science was early recognized in US sociology. This dilemma was, for example, very clearly presented by C. Wright Mills in a book that originally was to be called *Autopsy of Social Science* but that we today know as *The Sociological Imagination* (Mills 1959).

Mills's attempt to solve the dilemma consisted of two moves: letting loose imagination in the sociological enterprise and going back to the classics' view of social science as an attempt to deal with important social issues.

A very different attempt to solve the dilemma was made in the 1960s and 1970s by a small number of sociologists who tried to develop a new approach to theory, known as *theory construction* (for example, Zhao 1996; Willer 1996). Their main idea was to look at theory as if it was a method. Just as you need to be systematic and clear about the ways in which you collect data and process these (methods), you need to explain these in a systematic and clear way (theory).

A theory is not something given or just an idea, it was argued, you *construct* a theory. It has a number of parts that need to be fitted together in a special way. The result of looking at theory in this way implies a rejection of both empiricism and abstract

theory. According to the advocates of theory construction, it leads to good, practical social science.

A few important works on theory construction were produced during these years, which are still very useful to study for those who are interested in theorizing. In sociology there is especially *Constructing Social Theories* (1968) by Arthur Stinchcombe. And if we broaden our view and go beyond sociology, there is *An Introduction to Models in the Social Sciences* (1975) by Charles Lave and James G. March.

From the perspective in this book, the idea of theory construction definitely represents a move in the right direction, and there clearly exists an affinity between the approach of creative theorizing and that of theory construction. Both argue that theory is not something that comes to you through intuition, nor is it something that should be restricted to a few star thinkers. It is instead something that needs to be carefully constructed, and you can learn as well as teach the skill to construct a theory.

But there also exist some weaknesses to the perspective of theory construction, and in my view these help to explain why it lasted only for about a decade in sociology and was then seen as a dead end (for example, Hage 1994; Zhao 1996).

One of these weaknesses has to do with theory contruction's preference for logical thought at the expense of most of what makes up ordinary thinking. This means among other things that the link to the subconscious was not understood and that the capacity to innovate could not be properly cultivated. One reason for this is probably that the theory construction movement developed its approach without any input from cognitive psychology.

As a result of this and some other reasons as well, which will soon be mentioned, there is something mechanical about the approach of theory construction. You essentially teach students to build a theory by fitting together prefabricated pieces, a bit like you put together something from IKEA. You need a certain skill to do this, but one that is quite different from the kind of theorizing that is advocated in this book.

One of these other weaknesses of the approach of theory construction has to do with the way it ignored the element of observation in theorizing and focused most of its attention on the stage of verification. From the perspective in this book, a lack of fresh empirical material makes it hard to go beyond the preconceptions that Durkheim speaks about.

When the process of research begins for the theory constructor, the first and the most important task is to produce interesting hypotheses of the type that can later be verified. Knowledge about the phenomenon you want to study is supposed to already exist. The key to successful research stands and falls with the number of interesting hypotheses that can be invented, and how well these are handled.

According to Stinchcombe in *Constructing Social Theories*, "a student who has difficulty thinking of at least three sensible explanations for any correlation that he is really interested in should probably choose another profession" (Stinchcombe 1968: 13).

In a similar vein Lave and March encourage the readers of their book to come up with as many hypotheses as they can for the phenomenon they are interested in, and to figure out how to discriminate between these through careful testing. But Lave and March do not tell the reader that it is necessary to observe something very carefully before you try to theorize it, and that you need to know quite a bit about a phenomenon in order to develop a theory about it. The reader is left with the impression that what fundamentally matters is to develop an agility in thinking and coming up with an explanation, rather than basing this on penetrating knowledge of what you are studying.

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Both Stinchcombe and Lave and March ignore, to repeat, the element of early and deep observation in theorizing, and this means that they do not challenge what Durkheim calls *preconceptions*. Robert K. Merton, in contrast, was well aware that observation had been pushed aside when verification became a major issue a few decades into the twentieth century in social science. When you "exaggerate the creative side of explicit theory," as he wrote in an article from 1948, you end up by underestimating the "creative role of observation" (Merton 1948: 506).

Merton argued that on the surface of things, it would appear that the emphasis on testing hypothesis was indeed the correct way to proceed:

> With a few conspicuous exceptions, recent sociological discussions have assigned but one major function to empirical research: "testing" or "verification" of hypotheses. The model for the proper way of performing this function is as familiar as it is clear. The investigator begins with a hunch or hypothesis, from which he draws various inferences and these, in turn, are subjected to empirical test which confirms or refutes the hypothesis. (Merton 1945: 505–6)

But there exist many problems with proceeding according to this "logical model," Merton also noted, and these make it difficult to produce really good research:

> But this is a logical model and so fails, of course, to describe much of what actually occurs in fruitful investigation. It presents a set of logical norms, not a description of the research experience. And, as logicians are well aware, in purifying the experience, the logical model may also distort it. Like other models, it abstracts from the temporal sequence of events. It exaggerates the creative role of explicit theory just as it minimizes the creative role of observation. . . . *It is my central thesis that empirical research goes far beyond the passive role of verifying and testing theory: it does more than confirm or refute hypotheses.* Research plays an active role: it performs at least four major functions which help shape the development of theory. It *initiates*, it *reformulates*, it *deflects* and it *clarifies* theory. (Merton 1948: 506; emphasis added)

Merton's diagnosis was right on target when he said that the element of verification—the testing of hypotheses against data—had become the central issue for many social scientists after World War II. But instead of drawing the consequences of his insight that social science research must start with observation, Merton wavered and cast his argument in terms of the contrast between how research is supposed to be carried out and how it is actually carried out. This made it hard for him to formulate a successful approach to theorizing.

The point that Merton did not make is that observation constitutes *the first stage of theorizing*; it is also an organic part of theorizing. The main purpose of observation at this early stage, he should also have added, is primarily *heuristic*—that is, its purpose is to help the social scientist to better understand what some phenomenon is really like.

It is important to emphasize that the notion of observation must be understood in a very wide sense at this stage of the research. There exist many different ways of making observations, not only in the social sciences but in society at large. To the extent that any of these can further the exploration and understanding of some phenomenon they should be used.

The main point of observation at this stage of the research is to get as much and as multifaceted information as possible about

some phenomenon so that you can get a new angle on it. At the later stage, when your ideas are to be tested in a systematic and methodical fashion, it is enough to work with the more narrow range of what can be called "'hypothesis-testing' facts" (Stinchcombe 1978: 5).

But at the beginning of the research the information should come from a very broad range of sources. It can come from interviews, archives, newspapers, bar codes, autobiographies, data sets, dreams, movies, poems, music—pretty much from any source that has something to say about the phenomenon you are interested in.

Again, it is not at all necessary that this information should be gathered in a reliable manner—meaning by this, with the help of the best available methods. This goes for quantitative as well as qualitative methods.

If rule number one for this stage of observation is that things are not what they seem to be, rule number two is that you can use any data or information that will help you to go beyond the existing preconceptions. *Anything goes!*

To acquire information at this early stage represents a distinct and relatively independent stage in the theorizing process. To get the data you need will take its time, and it is important not to be impatient. It is also helpful to *not* start analyzing the material until you have learned quite a bit about the phenomenon you are interested in.

In the beginning of the research process, there exists a distinct temptation to either draw on the ideas you already have or to begin to analyze whatever you are studying very soon after starting to observe things. Both of these lead to mistakes. Wittgenstein's dictum for how to philosophize is also useful for this stage in social science: "*Don't think but look!*" (Wittgenstein 1953: 66e; emphasis added).

Choosing a Topic to Observe

The first task when you want to do research is to pick a topic. The social science literature provides several suggestions for how to do this. You can, for example, choose something interesting, something that constitutes a problem, and so on. Since you can do good work and formulate a good research question only if the topic is well chosen, the issue of what to focus on is very important.

According to some scholars, you should choose a topic that is *interesting* and appeals to your curiosity. This makes intuitive sense, as does the idea that the kind of research we appreciate is research that we find interesting.

In an article by Murray Davis titled "That's Interesting!" it is argued that what makes us think that some studies are great is that they strike us as interesting (Davis 1971). The author also presents a theory why some ideas are seen as interesting and others not.

Something is seen as interesting, Davis suggests, because it breaks with what we expect to find. "All *interesting* theories, at least all *interesting* social theories, then constitute an attack on the taken-for-granted world of their audience. . . . If it does not challenge but merely confirms one of their taken-for-granted beliefs, [the audience] will respond to it by rejecting its value while affirming its truth" (Davis 1971: 311).

Some theories other than that of Davis try to explain why something strikes us as interesting. According to one of these, something is seen as interesting because it appeals to our sense of curiosity. Curiosity, in its turn, is often seen as a drive, like hunger or thirst. While we use food and drink to satisfy these two drives, we satisfy our curiosity by seeking knowledge (for example, Loewenstein 1994; Kang et al. 2009).

A second theory about what topic to pick holds that the scientist should choose to work on a *problem*. This view of things is

quite common and has, for example, been advocated by Thomas Kuhn and Herbert Simon.

Calling something a problem (or a puzzle) means that while it resists an immediate solution, a solution is nonetheless seen as possible. The quality that a problem can perhaps be solved is important. According to Chomsky,

> we might distinguish "problems" from "mysteries," the former being questions that we seem to be able to formulate in ways that allow us to proceed with serious inquiry and possibly to attain a degree of understanding, the latter including questions that seem to elude our grasp, perhaps because we are as ill-equipped to deal with them as a rat is with a prime number maze. (Chomsky 1991: 41)

What constitutes a problem for a scientist is defined through normal science, according to Thomas Kuhn (1970). If you follow the existing paradigm, certain topics will be seen as problems to be solved. These will be searched out by scientists and pursued by them with passion. Herbert Simon similarly conceives of "the scientist as problem solver," to cite the title of one of his writings (Simon 1991b). Those who advocate that scientists should choose to study problems sometimes add that before a problem can be solved, it has to be formulated in a special way. "It is a familiar and significant saying that a problem well put is half-solved" (Dewey 1938: 108).

To this can be added that before you start working with a problem, you have to find one. It is sometimes noted that problem-finding not only precedes the stage of problem-solving, it is also considerably harder. "The experience of scientists," according to Merton, "is summed up in the adage that it is often more difficult to find and to formulate a problem than to solve it" (Merton 1959: ix).

Last is a third view of how scientists should select a topic to study. According to Charles S. Peirce, for example, the scientist starts out by observing things but is at some point *surprised* by something that he or she finds. The reason for the surprise is that it contradicts what the scientist expects to find, given the state of knowledge in his or her discipline (see also, for example, Dunbar and Klahr 2012: 706–7; Lombrozo 2012: 266).

In my view there are good arguments for following each of these three models for how to decide what to study and theorize: something interesting, a problem, and something that surprised you. There is also some overlap between them. Both what is interesting and what is surprising are, for example, the result of finding something else than what was expected. And when you are surprised by something, you have a problem to solve.

Since the view that you should study something that has surprised or startled you is the least known of the three approaches, I shall elaborate a bit on it. If you, for example, start out from Peirce's view, and try to translate it into practical tips or rules for how to proceed, you will get something like the following.

You begin the research by observing some topic you are interested in. While the search for facts at this stage is quite general, it should nonetheless be intense since the aim is to go beyond the current state of knowledge. At some point later in your research you will find something surprising, something that does not fit the current state of knowledge. It is *this* that should be studied.

The process of observation, from Peirce's perspective, consists of two stages: the first is broad but penetrating; the second is focused on the surprise and more intensive. It is important, in other words, not to pick your final topic until you have been surprised. If you follow this rule, you will study something that might lead to new knowledge.

But proceeding in this way also means that you typically have to discard quite a bit of the work you have done before the

surprise. This can be quite difficult to do, but it is necessary so you will be free to exclusively focus on what caused the surprise. "It feels like cutting your feet and legs off," a colleague told me. But it is something that has to be done, because it is the second topic that will yield the most.

Finding a good topic is important, but it is not enough. You also need facts to study it—and this presents another problem. In the words of Robert K. Merton, what you now need to do is to locate some strategic research material (SRM). "By SRM is meant the empirical material that exhibits the phenomena to be explained or interpreted to such an advantage and in such accessible form that it enables the fruitful investigation of previously stubborn problems and the discovery of new problems for further inquiry" (Merton 1987: 10–11).

The two-stage process of observation that Peirce advocates is similar to Durkheim's argument about preconceptions. In both cases you start out with one view of things and have to wait to do the analysis until you feel that you have reached a deeper level (Durkheim) or found a new topic (Peirce).

To sum up, research can be triggered by a problem, by something interesting, and by something surprising. Regardless of what sets off the research, however, it is important to emphasize three things. First, the initial phase of observation should be carried out in a very free-ranging manner. Second, theorizing should be held off for a while. And third, the topic to study is not necessarily what you initially set out to analyze, but what after a while strikes you as being the most promising to pursue.

Description and Observation

While *description* and *observation* by no means are synonymous, description occupies an important place in observation and therefore also in theorizing. Description is very helpful at this

stage, since it allows you to gather facts without immediately linking these to a theory.

In many of the social sciences description is seen as inferior to analytic thinking and in general as unscientific and undesirable (for example, Abbott 2001: 121–22). Economists in particular have a low opinion of description, a topic that Amartya Sen addresses in "Description as Choice" (Sen 1980). In Sen's view description is an important as well as a difficult activity. The main reason for this is that it involves the choice of which details to focus on and which to leave out.

The typical attitude of economists toward description, Sen says, tends to lead to "confounding the nature of description as an activity and unnecessarily weakening the theoretical underpinning of many legitimate and useful activities in the social sciences" (Sen 1980: 368).

Among philosophers, it is perhaps Wittgenstein who has been the most interested in description (for example, Gert 1997). What you first have to understand, he says, is that there exist many different ways of making a description. Descriptions are "instruments for particular uses," and there is a difference between, say, describing your room and describing your mental state (Wittgenstein 1953: 290–91).

The kind of descriptions that Wittgenstein himself was most interested in were descriptions of words and how people use these, in which situations and with what intent and effect. By being extra sensitive to these issues, he believed, a number of difficult conceptual problems in philosophy could be solved.

While the interest of the social scientist differs from that of Wittgenstein, one can nonetheless follow his lead and make a special effort to observe very closely the way in which people use language and what they try to accomplish by doing so. Proceeding in this way allows you to tap into some of the most intriguing and complex aspects of social life.

Describing the ways in which words are used is essential for an understanding of the role of meaning in social life. It also draws your attention away from what is purely visual. In the terminology of modern anthropology, it leads to *thick description* as opposed to *thin description* (Geertz 2000).

The neo-Kantians argued that reality consists of an endless amount of details, and that you have to make a conscious choice if you want to be able to say anything at all. This was also the position of Max Weber, who in his methodological writings points out that it is "impossible in principle" to describe everything that happens (for example, Weber 1975: 173).

This position is by now well recognized. In order to move theorizing forward, however, a somewhat different message seems more important. This is that the more details you have, the better position you are in, especially when you try to theorize in a creative way. It is in the details that you will find the germs to the new theory.

It is also important to realize that the urge to push the details to the side and begin to generalize represents a temptation that should be resisted at the stage of observation. Detailed material is very useful when you try to theorize, and the reason for this, to repeat, is that it is typically untheorized. Of special interest among the details are so-called telling details. A *telling detail* is one that makes it easier to understand a phenomenon (for example, Pinch and Swedberg 2012).

Fieldwork and Observation

Like description, fieldwork has a number of qualities that makes it useful for observation during the prestudy. Everett C. Hughes, who helped to introduce the idea of fieldwork into modern sociology, was well aware of its advantages in this respect. He spelled these out

in what can be seen as his manifesto for how to do social research, "The Place of Field Work in Social Science" (Hughes 1984a).

According to this article, there is especially one quality to fieldwork that makes it important in this respect and that also makes it very useful for heuristic purposes. This is that it allows you to *see for yourself* (Hughes 1984a: 497).

The idea that the researcher should be in a position to check things out for himself or herself seems obvious enough, and something that any social scientist would want to do. If one looks at the history of the social sciences, however, it has taken a long time for the idea of fieldwork to be established. Anthropologists were the first social scientists to conduct fieldwork, and economists are currently in the process of discovering it.

In sociology, people like Weber, Simmel, and Durkheim never did any fieldwork, nor did they conduct any interviews themselves. The modern notion of an *interview*—a procedure during which both questions and answers are carefully recorded—did not have its breakthrough in sociology until after World War I (Platt 2002).

Fieldwork was introduced into sociology at about the same time and very much thanks to US sociologists, first by W.E.B. Du Bois and a bit later on a collective scale (that enabled the breakthrough) by the members of the Chicago School. According to Robert Park, its leader and also the teacher of Everett C. Hughes, it is crucial for social scientists to engage in firsthand observation.

What is meant by firsthand observation is clear from the following famous quote, summarizing Park's message:

> Go and sit in the lounges of the luxury hotels and on the door-steps of flop-houses, sit on the Gold Coast settees and on the slum shakedowns; sit in the Orchestra Hall and in the Star and Garter Burlesk. In short, gentlemen, go get the

> seat of your pants dirty in real research. (Park in Bulmer 1984: 97)

Hughes points out that not only sociologists make observations but also reporters, and sociologists can learn much from them (Hughes 1984a). He does not mention comedians and authors of novels and poetry, but they also make very sharp observations. To paraphrase Wittgenstein, there exist many ways to make observations, and these need to be observed.

When you make an observation, Hughes says, it is important to "distance yourself," which roughly means that you need to position yourself vis-à-vis people in such a way that it becomes possible to make objective observations, as opposed to participating in their lives and looking at things from their perspective.

As part of this process the observer will also begin to transform the information he or she has picked up into objective knowledge about groups and institutions. This is when observation truly becomes what Hughes refers to as "social observation" (for example, Hughes 1984b: 317, 499; cf. Mills 1959: 70).

In his discussion of fieldwork Hughes also uses the expression "observation 'on the hoof'"—meaning the kind of observation that only the skilled social scientist can engage in. When you make observations on the hoof, Hughes says, you view things in terms of social patterns or institutions (Hughes 1984a: 504–5).

According to the dictionary, the expression *on the hoof* also has a second meaning, which is suggestive in this context and no doubt appealed to Hughes. It means doing something while you are also doing something else.

Observation often entails this double kind of behavior: the observer seems to be doing one thing, while he or she is actually busy observing. Double behavior also has the capacity to unlock your creativity; it is sometimes only possible to come up with a new idea, when your brain is busy with some other task.

In a similar spirit Howard Becker (who had Hughes as a teacher) mentions how he learned to make observations as a young boy in Chicago. He did this primarily by riding the subway for hours and hours, just looking at what was going on. Later he did the same thing, when he worked as a jazz musician (Becker no date).

While facts of all kinds may be of heuristic importance at the stage of observation, primary data of the type generated through fieldwork are special in this regard. One reason for this is that you know exactly how they have been produced; another, that they are untheorized, in the sense that they have not been filtered through the mind of some other social scientist. This makes it not only easier to theorize the data, it also makes it more likely that you will find something new.

Statistics and Observation

At first it might seem that statistics would be useful only during the main study, because of the careful and methodical way in which a statistical study should be conducted. This, however, is not the case, and statistics can be part of what the theorizer draws on during the prestudy. This is true not only for descriptive statistics but also for more advanced versions.

During the early stage of the research, statistics can be used both for tentative explanations and for observation. Statistics are routinely used in social science for explanatory purposes, but as John Goldthorpe argues, they may also have a valuable function to play at the stage of observation. "It is important," he says, "that the use of rather advanced statistical techniques for . . . purposes of what might be called sophisticated description should be clearly distinguished from their use in attempts at deriving causal relations directly from data analysis" (Goldthorpe 2001: 11).

What Goldthorpe advocates is not so very different, as it turns out, from Hughes. In "The Place of Field Work in Social Science," Hughes says that there exist two ways of bringing out the social dimension through observation. One is having a skilled observer on the scene. The other is when the social scientist takes a small fact and looks at it through the lens of a large number of facts (Hughes 1984a: 504).

It is also clear that at the stage of discovery you do not have to use statistics in the professional and methodical way in which it should be used in the main study. You can for example, make quick trial runs, use nonrepresentative samples, and in other ways just try to get a good sense for what is going on. The purpose at this stage is to generate ideas; and shortcuts can be taken.

To exclusively base your observation on quantitative data that others have generated should in principle be avoided, unless this is more or less impossible (for example, Heltberg 2011). The main reason for this is that statistics, like all computer-based observations, represents a kind of disembodied facts that are often hard to read (for example, Dreyfus 2009).

None of the senses of the researcher are very much involved when you work with other people's quantitative data, except for sight. The whole experience is as a consequence very cognitive and often leaves little room for the subconscious to pick up things. This can make for a narrow type of analysis, and it increases the chances of overlooking something essential. In this sense computer research is not so different from library research, which was the norm in sociology before fieldwork was developed.

One advantage in making your own observations is that you pick up a lot of things that are not particularly useful but that allow what *is* useful to become visible. Some of this information comes close to what Michael Polanyi calls "tacit knowledge"—

that is, knowledge that is necessary to execute some activity, but where the actors are unable to articulate what they are doing.

This brings us to the subconscious, which cognitive scientists have by now explored for many decades. They have among other things mapped out the extremely complex ways in which human beings record sounds and visual stimuli. The study of memory represents another area where interesting progress has been made, and which is of much relevance to theorizing in social science.

Exactly how social elements interact with the physiological processes that are involved in seeing, hearing, and remembering is not clear. Still, it would appear that knowledge of these processes is relevant for an understanding of the complexities of observation. The old idea in sociology that the social and the biological should be firmly kept apart is not useful any longer.

For this reason it is important that those who are interested in theorizing keep up with what is going on in cognitive science. It would be especially helpful for theorizers if a way was found to translate the insights of cognitive science into practical rules for how to become better at observation. It should be stated once more that what is of most interest to theorizers are practical tips and rules for how to proceed when you theorize.

What about introspection? Has the time also come to reevaluate the value of this way of proceeding? Introspection has by now been banned from the social sciences for a long time, and the main reason for this is that it made researchers rely on their own opinions rather than on facts.

That this is unacceptable is still a sound rule of course. But when it comes to theorizing the question is somewhat different, and what needs to be discussed is whether introspection can be useful for heuristic purposes. In my own view using introspection should be an option for the theorizer. Or to be more precise, and

also supply the reason for this stance: *to carefully observe yourself*, and to do so for heuristic purposes, represents one way to get some ideas about the phenomenon you are interested in.

Take, for example, Peirce's description of himself when he looked at an impressionist painting of the sea. "As I gaze upon it I detect myself sniffing the salt-air and holding up my cheek to the sea breeze" (Peirce 1992b: 182). You can of course ask whether Peirce was correct when he said that this was the way he reacted to a painting of the sea. And for a quick and tentative answer—why not observe your own reactions?

Introspection in the form of self-observation can also be of help in establishing what meanings and moods actors invest their behavior with. One problem with meanings and moods is that it is very difficult to get into the head of the people you observe. Meanings and moods also tend to change and disappear. And again, one way to bypass this problem is to observe yourself (for the centrality of meaning, see, for example, Weber 1978; for the centrality of mood, see, for example, Damasio 2003).

But a better way to proceed, of course, is to somehow get into the mind of the people you observe. According to Weber, it is through the process of understanding that we are able to somehow enter into the mind of other people, and this understanding is typically either rational or "emotionally empathetic" (Weber 1978: 5). When someone behaves in a rational way it is relatively easy for the analyst to understand what is going on in the actor's head (Weber 1978: 4–7). A person is perhaps trying to solve a simple problem in arithmetic, and we immediately understand the way of reasoning that this involves.

When it comes to empathy the situation is more complex. Still "the more we ourselves are susceptible to such emotional reactions as anxiety, anger, ambition, jealousy, love, enthusiasm, pride, vengefulness, loyalty, devotion, and appetites of all sorts, and to the 'irrational' conduct that grows out of them, the more readily can we empathize with them" (Weber 1978: 6). Note that Weber is here reminding us that understanding is directly linked to self-observation.

The role of memory was mentioned earlier apropos the advances of cognitive science, and something also needs to be said about its importance for observation at the stage of the prestudy. Memory is crucial for ongoing activities, in that you cannot act at all unless you have calibrated what you are about to do with the help of memory and experience. This means that exploring memory has to be part of observation. What do people remember; what do they forget? What do people want to remember; what do they want to forget (for example, Wickelgren 2012)?

Just as observation means paying attention to memory, it also means paying attention to history. According to Tocqueville, who did quite a bit of observation on the hoof when he traveled around in the United States, we are all linked to the past through invisible threads (Tocqueville 2009). History connects what individuals do now to what they did in the past.

The category of history goes well beyond memory, even if it also includes it. The problem for the theorizer, at the stage of observation, is to somehow see how paying attention to the past will make it easier to discover what is happening just now. This is also a reason why it is helpful to know something about the way that historians theorize (for example, Bloch 1964; Iggers et al. 2008).

The Role of Earlier Social Science Studies and Theory

Some readers may by now have become impatient and ask, what about the role of earlier studies by social scientists on the topic you are observing? And what about your training and years of experience? Are these not important, and do they not

sharpen as well as guide our sense of observation, especially social observation?

The answer to both questions is a clear "yes," but I have wanted to first of all emphasize the importance of trying to see things in a new light. And to do this, it is helpful to proceed in unorthodox ways and to be as open as possible to new observations.

To get a good sense for a new topic you clearly need to be familiar with earlier studies. There are many and obvious reasons why this is the case. They may contain data that are relevant for understanding your topic. They may contain useful concepts and theories. It is also with their help that you are able to decide what is known about a topic and what is not.

A caveat, however, is in place. This is that is that it is easy to become overly influenced by the existing research on some topic. And this can inhibit your creativity. For these reasons, it is helpful to try to find a way of both keeping the existing literature at a distance *and* having access to it.

Since this may sound vague and contradictory, let me give an example of a successful way of doing this. It was devised by Tocqueville, who was extremely inventive when it comes to gathering data and analyzing them (for example, Swedberg 2009).

When Tocqueville traveled around in the United States in 1831–32, he gathered as much primary material as he could, but he also avoided reading studies of the United States by his contemporaries. The reason for this, he said, was that he did not want them to influence his view of the country before he had developed his own analysis.

Still Tocqueville also wanted to know what these other studies contained already at this stage. His solution was to ask his traveling companion, Gustave de Beaumont, to read them and tell him if they contained something he ought to know.

When we do our research most of us do not have such a helpful and competent traveling companion as Tocqueville did. But

there may be other ways of dealing with the existence of earlier studies so that they do not block our creativity. It goes without saying that these studies have to be worked through and referred to in the final study. The question is *when* to carefully work them through.

Studies on the topic you are interested in, but that have been written by scholars outside your own discipline, can be reviewed without much harm at an early stage. The reason for this is that they usually operate with different concepts and approach things in a different way. And this can have an invigorating effect on your imagination.

So much for studies by other social scientists. But what about the role of your training as a social scientist and general knowledge of social science for making observations? The answer is that both of these are indispensable for good work. They also confirm the well-known dictum that only scientists who are well prepared are in a position to make a discovery.

The more you have developed what Hughes calls a "sociological eye," the more you will be able to single out what is social about a phenomenon (Hughes 1984b). And the kind of methods you are used to working with will also influence the way in which a phenomenon is looked at and understood.

The need to be well trained in theory, in order to be good at theorizing, will be dealt with later in this book. And so will the topic of the close relationship between theory and method in the research process. Before addressing these two issues, however, it is important to address a different topic—that is, to discuss the process of theorizing in a more narrow sense, something that will be done in the next few chapters.