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17

CASE STUDY

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[C]onduct has its sphere in particular circumstances. That is why some people who do not possess theoretical knowledge are more effective in action (especially if they are experienced) than others who do possess it. For example, suppose that someone knows that light flesh foods are digestible and wholesome, but does not know what kinds are light; he will be less likely to produce health than one who knows that chicken is wholesome.

—Aristotle

What Is a Case Study?

Definitions of "case study" abound. Some are useful, others not. Merriam-Webster's dictionary (2009) defines a case study straightforwardly as follows:

> *Case Study.* An intensive analysis of an individual unit (as a person or community) stressing developmental factors in relation to environment.

According to this definition, case studies focus on an "individual unit," what Robert Stake (2008, pp. 119–120) calls a "functioning specific" or "bounded system." The decisive factor in defining a study as a case study is the choice of the individual unit of study and the setting of its boundaries, its "casing" to use Charles Ragin's (1992, p. 217) felicitous term. If you choose to do a case study, you are therefore not so much making a methodological choice as a choice of what is to be studied. The individual unit may be studied in a number of ways, for instance qualitatively or quantitatively, analytically or hermeneutically, or by mixed methods. This is not decisive for whether it is a case study or not; the demarcation of the unit's boundaries is. Second, the definition stipulates that case studies are "intensive." Thus, case studies comprise more detail, richness, completeness, and variance—that is, depth—for the unit of study than does cross-unit analysis. Third, case studies stress "developmental factors," meaning that a case typically evolves in time, often as a string of concrete and interrelated events that occur "at such a time, in such a place" and that constitute the case when seen as a whole. Finally, case studies focus on "relation to environment," that is, context. The drawing of boundaries for the individual unit of study decides what gets to count as case and what becomes context to the case.

Against Webster's commonsensical definition of case study, the Penguin *Dictionary of Sociology* (Abercrombie, Hill, & Turner, 1984, p. 34; and verbatim in the 1994 and 2006 editions) has for decades contained the following highly problematic, but unfortunately quite common, definition of case study:

> *Case Study.* The detailed examination of a single example of a class of phenomena, a case study cannot provide reliable information about the broader class, but it may be useful in the preliminary stages of an investigation since it provides hypotheses, which may be tested systematically with a larger number of cases.

This definition is indicative of much conventional wisdom about case study research, which, if not directly wrong, is so oversimplified as to be grossly misleading. The definition promotes the mistaken view that the case study is hardly a methodology in its own right, but is best seen as subordinate to investigations of larger samples. Whereas it is correct that the case study is a "detailed examination of a single example," it is wrong that a case study "cannot provide reliable information about the broader class." It is also correct that a case study *can* be used "in the preliminary stages of an investigation" to generate hypotheses, but it is wrong to see the case study as a pilot

method to be used only in preparing the real study's larger surveys, systematic hypotheses testing, and theory building. The Penguin definition juxtaposes case studies with large-sample, statistical research in an unfortunate manner that blocks, instead of brings out, the productive complementarity that exists between the two types of methodology, as we will see below.

John Gerring (2004, p. 342) has correctly pointed out that the many academic attempts to clarify what "case study" means has resulted in a definitional morass, and each time someone attempts to clear up the mess of definitions it just gets worse. If we need a definition of what a case study is, we are therefore better off staying with commonsensical definitions like that from Webster's above than with more loaded academic definitions like that from the Penguin *Dictionary of Sociology.*

▣ The Case Study Paradox

Case studies have been around as long as recorded history and today they account for a large proportion of books and articles in psychology, anthropology, sociology, history, political science, education, economics, management, biology, and medical science. For instance, in recent years roughly half of all articles in the top political science journals have used case studies, according to Alexander George and Andrew Bennett (2005, pp. 4–5). Much of what we know about the empirical world has been produced by case study research, and many of the most treasured classics in each discipline are case studies.

But there is a paradox here. At the same time that case studies are widely used and have produced canonical texts, it may be observed that the case study as a methodology is generally held in low regard, or is simply ignored, within the academy. For example, only 2 of the 30 top-ranked U.S. graduate programs in political science require a dedicated graduate course in case study or qualitative methods, and a full third of these programs do not even offer such a course. In contrast, all of the top 30 programs offer courses in quantitative methods and almost all of them require training in such methods, often several courses (George & Bennett, 2005, p. 10). In identifying this paradox of the case study's wide use and low regard, Gerring (2004, p. 341) rightly remarks that the case study survives in a "curious methodological limbo," and that the reason is that the method is poorly understood.

In what follows, we will try to resolve Gerring's paradox and help case study research gain wider use and acceptance by identifying five misunderstandings about the case study that systematically undermine the credibility and use of the method. The five misunderstandings can be summarized as follows:

Misunderstanding No. 1	General, theoretical knowledge is more valuable than concrete case knowledge.
Misunderstanding No. 2	One cannot generalize on the basis of an individual case; therefore, the case study cannot contribute to scientific development.
Misunderstanding No. 3	The case study is most useful for generating hypotheses; that is, in the first stage of a total research process, while other methods are more suitable for hypotheses testing and theory building.
Misunderstanding No. 4	The case study contains a bias toward verification, that is, a tendency to confirm the researcher's preconceived notions.
Misunderstanding No. 5	It is often difficult to summarize and develop general propositions and theories on the basis of specific case studies.

The five misunderstandings may be said to constitute the conventional view, or orthodoxy, of the case study. We see that theory, reliability, and validity are at issue; in other words, the very status of the case study as a scientific method. In what follows, we will correct the five misunderstandings one by one and thereby clear the ground for a use of case study research in the social sciences that is based on understanding instead of misunderstanding.

▣ Misunderstanding No. 1

General, theoretical knowledge is more valuable than concrete case knowledge.

In order to understand why the conventional view of case study research is problematic, we need to grasp the role of cases and theory in human learning. Here, two points can be made. First, the case study produces the type of concrete, context-dependent knowledge that research on learning shows to be necessary to allow people to develop from rule-based beginners to virtuoso experts. Second, in the study of human affairs, there appears to exist only context-dependent knowledge, which thus presently rules out the possibility for social science to emulate natural science in developing epistemic theory, that is, theory that is explanatory and predictive. The full argument behind these two points can be found in Flyvbjerg (2001, Chaps. 2–4). For reasons of space, I can only give an outline of the argument here. At the outset, however, we can assert that if the two points are correct, it will have radical consequences for the conventional view of

the case study in research and teaching. This view would then be problematic.

Phenomenological studies of human learning indicate that for adults there exists a qualitative leap in their learning process from the rule-governed use of analytical rationality in beginners to the fluid performance of tacit skills in what Pierre Bourdieu (1977) calls virtuosos and Hubert and Stuart Dreyfus (1986), true human experts. Here we may note that most people are experts in a number of everyday social, technical, and intellectual skills like giving a gift, riding a bicycle, or interpreting images on a television screen, while only few reach the level of true expertise for more specialized skills like playing chess, composing a symphony, or flying an airplane.

Common to all experts, however, is that they operate on the basis of intimate knowledge of several thousand concrete cases in their areas of expertise. Context-dependent knowledge and experience are at the very heart of expert activity. Such knowledge and expertise also lie at the center of the case study as a research and teaching method; or to put it more generally yet—as a method of learning. Phenomenological studies of the learning process therefore emphasize the importance of this and similar methods; it is only because of experience with cases that one can at all move from being a beginner to being an expert. If people were exclusively trained in context-independent knowledge and rules, that is, the kind of knowledge that forms the basis of textbooks, they would remain at the beginner's level in the learning process. This is the limitation of analytical rationality; it is inadequate for the best results in the exercise of a profession, as student, researcher, or practitioner.

In teaching situations, well-chosen case studies can help students achieve competence, while context-independent facts and rules will bring students just to the beginner's level. Only few institutions of higher learning have taken the consequence of this. Harvard University is one of them. Here both teaching and research in the professional schools are modeled to a wide extent on the understanding that case knowledge is central to human learning (Christensen & Hansen, 1987; Cragg, 1940).

It is not that rule-based knowledge should be discounted; such knowledge is important in every area and especially to novices. But to make rule-based knowledge the highest goal of learning is topsy-turvy. There is a need for both approaches. The highest levels in the learning process, that is, virtuosity and true expertise, are reached only via a person's own experiences as practitioner of the relevant skills. Therefore, beyond using the case method and other experiential methods for teaching, the best that teachers can do for students in professional programs is to help them achieve real practical experience, for example, via placement arrangements, internships, summer jobs, and the like.

For researchers, the closeness of the case study to real-life situations and its multiple wealth of details are important in two respects. First, it is important for the development of a nuanced view of reality, including the view that human behavior cannot be meaningfully understood as simply the rule-governed acts found at the lowest levels of the learning process, and in much theory. Second, cases are important for researchers' own learning processes in developing the skills needed to do good research. If researchers wish to develop their own skills to a high level, then concrete, context-dependent experience is just as central for them as to professionals learning any other specific skills. Concrete experiences can be achieved via continued proximity to the studied reality and via feedback from those under study. Great distance from the object of study and lack of feedback easily lead to a stultified learning process, which in research can lead to ritual academic blind alleys, where the effect and usefulness of research becomes unclear and untested. As a research method, the case study can be an effective remedy against this tendency.

The second main point in connection with the learning process is that there does not and probably cannot exist predictive theory in social science. Social science has not succeeded in producing general, context-independent theory and has thus in the final instance nothing else to offer than concrete, context-dependent knowledge. And the case study is especially well suited to produce this knowledge. In his later work, Donald Campbell (1975, p. 179) arrives at a similar conclusion. Earlier, he (Campbell and Stanley, 1966, pp. 6–7) had been a fierce critic of the case study, stating that "such studies have such a total absence of control as to be of almost no scientific value." Now he explained that his work had undergone "an extreme oscillation away from my earlier dogmatic disparagement of case studies." Using logic that in many ways resembles that of the phenomenology of human learning, Campbell explains,

> After all, man is, in his ordinary way, a very competent knower, and qualitative common-sense knowing is not replaced by quantitative knowing. . . . This is not to say that such common sense naturalistic observation is objective, dependable, or unbiased. But it is all that we have. It is the only route to knowledge—noisy, fallible, and biased though it be. (1975, pp. 179, 191)

Campbell is not the only example of a researcher who has altered his views about the value of the case study. Hans Eysenck (1976, p. 9), who originally saw the case study as nothing more than a method of producing anecdotes, later realized that "sometimes we simply have to keep our eyes open and look carefully at individual cases—not in the hope of proving anything, but rather in the hope of learning something!" Final proof is hard to come by in social science because of the absence of "hard" theory, whereas learning is certainly possible. More recently, similar views have been expressed by Charles Ragin, Howard Becker, and their colleagues in explorations of what the case study is and can be in social inquiry (Ragin & Becker, 1992).

As for predictive theory, universals, and scientism, so far social science has failed to deliver. In essence, we have only specific cases and context-dependent knowledge in social science.

The first of the five misunderstandings about the case study—that general theoretical (context-independent) knowledge is more valuable than concrete (context-dependent) case knowledge—can therefore be revised as follows:

> *Predictive theories and universals cannot be found in the study of human affairs. Concrete case knowledge is therefore more valuable than the vain search for predictive theories and universals.*

▣ Misunderstanding No. 2

> One cannot generalize on the basis of an individual case; therefore, the case study cannot contribute to scientific development.

The view that one cannot generalize on the basis of a single case is usually considered to be devastating to the case study as a scientific method. This second misunderstanding about the case study is typical among proponents of the natural science ideal within the social sciences. Yet even researchers who are not normally associated with this ideal may be found to have this viewpoint. According to Anthony Giddens, for example,

> Research which is geared primarily to hermeneutic problems may be of generalized importance in so far as it serves to elucidate the nature of agents' knowledgeability, and thereby their reasons for action, across a wide range of action-contexts. Pieces of ethnographic research like . . . say, the traditional small-scale community research of fieldwork anthropology—are not in themselves generalizing studies. But they can easily become so if carried out in some numbers, so that judgements of their typicality can justifiably be made. (1984, p. 328)

It is correct that one can generalize in the ways Giddens describes, and that often this is both appropriate and valuable. But it would be incorrect to assert that this is the only way to work, just as it is incorrect to conclude that one cannot generalize from a single case. It depends upon the case one is speaking of, and how it is chosen. This applies to the natural sciences as well as to the study of human affairs (Platt, 1992; Ragin & Becker, 1992).

For example, Galileo's rejection of Aristotle's law of gravity was not based upon observations "across a wide range," and the observations were not "carried out in some numbers." The rejection consisted primarily of a conceptual experiment and later of a practical one. These experiments, with the benefit of hindsight, are self-evident. Nevertheless, Aristotle's view of gravity dominated scientific inquiry for nearly 2,000 years before it was falsified. In his experimental thinking, Galileo reasoned as follows: If two objects with the same weight are released from the same height at the same time, they will hit the ground simultaneously, having fallen at the same speed. If the two objects are then stuck together into one, this object will have double the weight and will according to the Aristotelian view therefore fall faster than the two individual objects. This conclusion ran counter to common sense, Galileo found. The only way to avoid the contradiction was to eliminate weight as a determinant factor for acceleration in free fall. And that was what Galileo did. Historians of science continue to discuss whether Galileo actually conducted the famous experiment from the leaning tower of Pisa, or whether this experiment is a myth. In any event, Galileo's experimentalism did not involve a large random sample of trials of objects falling from a wide range of randomly selected heights under varying wind conditions, and so on, as would be demanded by the thinking of the early Campbell and Giddens. Rather, it was a matter of a single experiment, that is, a case study, if any experiment was conducted at all. (On the relation between case studies, experiments, and generalization, see Bailey, 1992; Griffin, Botsko, Wahl, & Isaac, 1991; Lee, 1989; Wilson, 1987.) Galileo's view continued to be subjected to doubt, however, and the Aristotelian view was not finally rejected until half a century later, with the invention of the air pump. The air pump made it possible to conduct the ultimate experiment, known by every pupil, whereby a coin or a piece of lead inside a vacuum tube falls with the same speed as a feather. After this experiment, Aristotle's view could be maintained no longer. What is especially worth noting in our discussion, however, is that the matter was settled by an individual case due to the clever choice of the extremes of metal and feather. One might call it a critical case: For if Galileo's thesis held for these materials, it could be expected to be valid for all or a large range of materials. Random and large samples were at no time part of the picture. Most creative scientists simply do not work this way with this type of problem.

Carefully chosen experiments, cases, and experience were also critical to the development of the physics of Isaac Newton, Albert Einstein, and Niels Bohr, just as the case study occupied a central place in the works of Charles Darwin. In social science, too, the strategic choice of case may greatly add to the generalizability of a case study. In their classical study of the "affluent worker," John Goldthorpe, David Lockwood, Frank Beckhofer, and Jennifer Platt (1968–1969) deliberately looked for a case that was as favorable as possible to the thesis that the working class, having reached middle-class status, was dissolving into a society without class identity and related conflict (see also Wieviorka, 1992). If the thesis could be proved false in the favorable case, then it would most likely be false for intermediate cases. Luton, then a prosperous industrial center outside of London with companies known for high wages and social stability—fertile ground for middle-class identity—was selected as a case, and through intensive fieldwork the researchers discovered that even here an autonomous working-class culture prevailed, lending general credence to the thesis

of the persistence of class identity. Below we will discuss more systematically this type of strategic sampling.

As regards the relationship between case studies, large samples, and discoveries, William Beveridge (1951; here quoted from Kuper & Kuper, 1985) observed immediately prior to the breakthrough of the quantitative revolution in the social sciences, "[M]ore discoveries have arisen from intense observation [of individual cases] than from statistics applied to large groups." This does not mean that the case study is always appropriate or relevant as a research method, or that large random samples are without value. The choice of method should clearly depend on the problem under study and its circumstances.

Finally, it should be mentioned that formal generalization, be it on the basis of large samples or single cases, is considerably overrated as the main source of scientific progress. Economist Mark Blaug (1980)—a self-declared adherent to the hypothetico-deductive model of science—has demonstrated that while economists typically pay lip service to the hypothetico-deductive model and to generalization, they rarely practice what they preach in actual research. More generally, Thomas Kuhn has shown that the most important precondition for science is that researchers possess a wide range of practical skills for carrying out scientific work. Generalization is just one of these. In Germanic languages, the term "science" (*Wissenschaft*) means literally "to gain knowledge." And formal generalization is only one of many ways by which people gain and accumulate knowledge. That knowledge cannot be formally generalized does not mean that it cannot enter into the collective process of knowledge accumulation in a given field or in a society. Knowledge may be transferable even where it is not formally generalizable. A purely descriptive, phenomenological case study without any attempt to generalize can certainly be of value in this process and has often helped cut a path toward scientific innovation. This is not to criticize attempts at formal generalization, for such attempts are essential and effective means of scientific development. It is only to emphasize the limitations, which follows when formal generalization becomes the only legitimate method of scientific inquiry.

The balanced view of the role of the case study in attempting to generalize by testing hypotheses has been formulated by Harry Eckstein:

> *[C]omparative and case studies are alternative means to the end of testing theories, choices between which must be largely governed by arbitrary or practical, rather than logical, considerations* . . . [I]t is impossible to take seriously the position that case study is suspect because problem-prone and comparative study deserving of benefit of doubt because problem-free. (1975, pp. 116, 131, emphasis in original; see also Barzelay, 1993)

Eckstein here uses the term "theory" in its "hard" sense, that is, comprising explanation and prediction. This makes Eckstein's dismissal of the view that case studies cannot be used for testing theories or for generalization stronger than my own view, which is here restricted to the testing of "theory" in the "soft" sense, that is, testing propositions or hypotheses. Eckstein shows that if predictive theories would exist in social science, then the case study could be used to test these theories just as well as other methods.

More recently, George and Bennett (2005) have demonstrated the strong links between case studies and theory development, especially through the study of deviant cases, and John Walton (1992, p. 129) has similarly observed that "case studies are likely to produce the best theory." Already, Eckstein noted, however, the striking lack of genuine theories within his own field, political science, but apparently failed to see why this is so:

> Aiming at the disciplined application of theories to cases forces one to state theories more rigorously than might otherwise be done—provided that the application is truly "disciplined," i.e., designed to show that valid theory compels a particular case interpretation and rules out others. As already stated, this, unfortunately, is rare (if it occurs at all) in political study. One reason is the lack of compelling theories. (1975, pp. 103–104)

The case study is ideal for generalizing using the type of test that Karl Popper called "falsification," which in social science forms part of critical reflexivity. Falsification is one of the most rigorous tests to which a scientific proposition can be subjected: If just one observation does not fit with the proposition, it is considered not valid generally and must therefore be either revised or rejected. Popper himself used the now famous example of "All swans are white," and proposed that just one observation of a single black swan, that is, one deviant case, would falsify this proposition and in this way have general significance and stimulate further investigations and theory building. The case study is well suited for identifying "black swans" because of its in-depth approach: What appears to be "white" often turns out on closer examination to be "black." Deviant cases and the falsifications they entail are main sources of theory development, because they point to the development of new concepts, variables, and causal mechanisms, necessary in order to account for the deviant case and other cases like it.

We will return to falsification in discussing the fourth misunderstanding of the case study below. For the present, however, we can correct the second misunderstanding—that one cannot generalize on the basis of a single case and that the case study cannot contribute to scientific development—so that it now reads:

> *One can often generalize on the basis of a single case, and the case study may be central to scientific development via generalization as supplement or alternative to other methods. But formal generalization is overvalued as a source of scientific development, whereas "the force of example" and transferability are underestimated.*

▣ Misunderstanding No. 3

> The case study is most useful for generating hypotheses, while other methods are more suitable for hypotheses testing and theory building.

The third misunderstanding about the case study is that the case method is claimed to be most useful for generating hypotheses in the first steps of a total research process, while hypothesis-testing and theory-building is best carried out by other methods later in the process, as stipulated by the Penguin definition of case study at the beginning of this chapter. This misunderstanding derives from the previous misunderstanding that one cannot generalize on the basis of individual cases. And since this misunderstanding has been revised as above, we can now correct the third misunderstanding as follows:

The case study is useful for both generating and testing of hypotheses but is not limited to these research activities alone.

Eckstein—contravening the conventional wisdom in this area—goes so far as to argue that case studies are better for testing hypotheses than for producing them. Case studies, Eckstein (1975, p. 80) asserts, "are valuable at all stages of the theory-building process, but most valuable at that stage of theory-building where least value is generally attached to them: the stage at which candidate theories are tested." George and Bennett (2005, pp. 6–9) later confirmed and expanded Eckstein's position, when they found that case studies are especially well suited for theory development because they tackle the following tasks in the research process better than other methods:

- Process tracing that links causes and outcomes (see Box 17.1)
- Detailed exploration of hypothesized causal mechanisms
- Development and testing of historical explanations
- Understanding the sensitivity of concepts to context
- Formation of new hypotheses and new questions to study, sparked by deviant cases

Even rational choice theorists have begun to use case study methods to test their theories and hypotheses, which, if anything, should help deflate the decades-old antagonism between quants and qualts over case study research (Bates, Greif, Levi, Rosenthal, & Weingast, 1998; Flyvbjerg, 2006).

Box 17.1 Falsifying Nobel Prize Theories Through Process Tracing

Some years ago, the editor of *Harvard Business Review* contacted me and asked for a comment on an article he was printing by Princeton psychologist Daniel Kahneman. The editor was puzzled by the fact that Kahneman's Nobel Prize–winning theories on decision making under uncertainty explained failure in executive decisions in terms of inherent optimism (Lovallo & Kahneman, 2003), whereas my group and I explained similar phenomena in terms of strategic misrepresentation, that is, lying as part of principal-agent behavior (Flyvbjerg, Holm, & Buhl, 2002). Who was right, the editor asked? Optimism is unintentional self-deception, whereas lying is intentional deception of others. The question therefore boiled down to whether deception, which caused failure—that much we agreed upon—was intentional or not. The statistical methods that both Kahneman and I had relied upon in our studies of deception could not answer this question. It was now necessary to process trace all the way into people's heads in order to understand whether intention was present or not. Through a number of case studies and interviews, my group and I established that deception is in fact often intentional, especially for very large and expensive decisions taken under political and organizational pressure. We thus falsified optimism as a global explanation of executive failure and developed a new and more nuanced theory that combines optimism and strategic misrepresentation in accounting for failure (Flyvbjerg, 2007).

Testing of hypotheses relates directly to the question of "generalizability," and this in turn relates to the question of case selection. Here, generalizability of case studies can be increased by the strategic selection of cases (for more on the selection of cases, see Ragin, 1992; Rosch, 1978). When the objective is to achieve the greatest possible amount of information on a given problem or phenomenon, a representative case or a random sample may not be the most appropriate strategy. This is because the typical or average case is often not the richest in information. Atypical or extreme cases often reveal more information because they activate more actors and more basic mechanisms in the situation studied. In addition, from both an understanding-oriented and an action-oriented perspective, it is often more important to clarify the deeper causes behind a given problem and its consequences than to describe the symptoms of the problem and how frequently they occur. Random samples emphasizing representativeness will seldom be able to produce this kind of insight; it is more appropriate to select some few cases chosen for their validity.

Table 17.1 summarizes various forms of sampling. The *extreme*, or *deviant, case* can be well suited for getting a point across in an especially dramatic way, which often occurs for well-known case studies such as Sigmund Freud's "Wolf-Man" and Michel Foucault's "Panopticon." The deviant case is also

Table 17.1 Strategies for the Selection of Samples and Cases

Type of Selection	*Purpose*
A. Random selection	To avoid systematic biases in the sample. The sample's size is decisive for generalization.
1. Random sample	To achieve a representative sample that allows for generalization for the entire population.
2. Stratified sample	To generalize for specially selected subgroups within the population.
B. Information-oriented selection	To maximize the utility of information from small samples and single cases. Cases are selected on the basis of expectations about their information content.
1. Extreme/deviant cases	To obtain information on unusual cases, which can be especially problematic or especially good in a more closely defined sense. To understand the limits of existing theories and to develop new concepts, variables, and theories that are able to account for deviant cases.
2. Maximum variation cases	To obtain information about the significance of various circumstances for case process and outcome; e.g., three to four cases that are very different on one dimension: size, form of organization, location, budget, etc.
3. Critical cases	To achieve information that permits logical deductions of the type, "If this is (not) valid for this case, then it applies to all (no) cases."
4. Paradigmatic cases	To develop a metaphor or establish a school for the domain that the case concerns.

particularly well suited for theory development, because it helps researchers understand the limits of existing theories and to develop the new concepts, variables, and theories that will be able to account for what were previously considered outliers.

In contrast, a *critical case* can be defined as having strategic importance in relation to the general problem. The above-mentioned strategic selection of lead and feather for the test of whether different objects fall with equal velocity is an example of critical case selection. This particular selection of materials provided the possibility to formulate a type of generalization that is characteristic of critical cases, a generalization of the sort, "If it is valid for this case, it is valid for all (or many) cases." In its negative form, the generalization would be, "If it is not valid for this case, then it is not valid for any (or only few) cases" (see also Box 17.2).

Box 17.2 Critical Case for Brain Damage

An occupational medicine clinic wanted to investigate whether people working with organic solvents suffered brain damage. Instead of choosing a representative sample among all those enterprises in the clinic's area that used organic solvents, the clinic strategically located a single workplace where all safety regulations on cleanliness, air quality, and the like, had been fulfilled. This model enterprise became a critical case: If brain damage related to organic solvents could be found at this particular facility, then it was likely that the same problem would exist at other enterprises that were less careful with safety regulations for organic solvents. Via this type of strategic choice, one can save both time and money in researching a given problem, and one may generalize in the following manner from a critical case: "If it is valid for this case, it is valid for all (or many) cases." In its negative form, the generalization would be, "If it is not valid for this case, then it is not valid for any (or only few) cases." In this instance, the occupational medicine clinic found brain damage related to organic solvents in the model enterprise and concluded that the problem needed to be dealt with in all enterprises in its jurisdiction.

How does one identify critical cases? This question is more difficult to answer than the question of what constitutes a critical case. Locating a critical case requires experience, and no universal methodological principles exist by which one can with certainty identify a critical case. The only general advice that can be given is that when looking for critical cases, it is a good idea to look for either "most likely" or "least likely" cases, that is, cases that are likely to either clearly confirm or irrefutably falsify propositions and hypotheses. A model example of a "least likely" case is Robert Michels's (1962) classic study of oligarchy in organizations. By choosing a horizontally structured grassroots organization with strong democratic ideals—that is, a type of organization with an especially low probability of being oligarchic—Michels could test the universality of the oligarchy thesis, that is, "If this organization is oligarchic, so are most others." A corresponding model example of a "most likely" case is

W. F. Whyte's (1943) study of a Boston slum neighborhood, which according to existing theory should have exhibited social disorganization, but in fact showed quite the opposite (see also the articles on Whyte's study in the April 1992 issue of the *Journal of Contemporary Ethnography*).

Cases of the "most likely" type are especially well suited to falsification of propositions, while "least likely" cases are most appropriate for tests of verification. It should be remarked that a most likely case for one proposition is the least likely for its negation. For example, Whyte's slum neighborhood could be seen as a least likely case for a hypothesis concerning the universality of social organization. Hence, the identification of a case as most or least likely is linked to the design of the study, as well as to the specific properties of the actual case.

A final strategy for the selection of cases is choice of the *paradigmatic case.* Thomas Kuhn has shown that the basic skills, or background practices, of natural scientists are organized in terms of "exemplars," the role of which can be studied by historians of science. Similarly, scholars like Clifford Geertz and Michel Foucault have often organized their research around specific cultural paradigms: A paradigm for Geertz lay for instance in the "deep play" of the Balinese cockfight, while for Foucault, European prisons and the "Panopticon" are examples. Both instances are examples of paradigmatic cases, that is, cases that highlight more general characteristics of the societies in question. Kuhn has shown that scientific paradigms cannot be expressed as rules or theories. There exists no predictive theory for how predictive theory comes about. A scientific activity is acknowledged or rejected as good science by how close it is to one or more exemplars, that is, practical prototypes of good scientific work. A paradigmatic case of how scientists do science is precisely such a prototype. It operates as a reference point and may function as a focus for the founding of schools of thought.

As with the critical case, we may ask, "How does one identify a paradigmatic case?" How does one determine whether a given case has metaphorical and prototypical value? These questions are even more difficult to answer than for the critical case, precisely because the paradigmatic case transcends any sort of rule-based criteria. No standard exists for the paradigmatic case because it sets the standard. Hubert and Stuart Dreyfus see paradigmatic cases and case studies as central to human learning. In an interview with Hubert Dreyfus (author's files), I therefore asked what constitutes a paradigmatic case and how it can be identified. Dreyfus replied,

> Heidegger says, you recognize a paradigm case because it shines, but I'm afraid that is not much help. You just have to be intuitive. We all can tell what is a better or worse case—of a Cézanne painting, for instance. But I can't think there could be any rules for deciding what makes Cézanne a paradigmatic modern painter. . . . [I]t is a big problem in a democratic society where people are supposed to justify what their intuitions are. In fact, nobody really can justify what their intuition is. So you have to make up reasons, but it won't be the real reasons.

One may agree with Dreyfus that intuition is central to identifying paradigmatic cases, but one may disagree it is a problem to have to justify one's intuitions. Ethnomethodological studies of scientific practice have demonstrated that all variety of such practice relies on taken-for-granted procedures that feel largely intuitive. However, those intuitive decisions are accountable, in the sense of being sensible to other practitioners or often explicable if not immediately sensible. That would frequently seem to be the case with the selection of paradigmatic cases. We may select such a case on the basis of taken-for-granted, intuitive procedures but are often called upon to account for that selection. That account must be sensible to other members of the scholarly communities of which we are part. This may even be argued to be a general characteristic of scholarship, scientific or otherwise, and not unique to the selection of paradigmatic social scientific case studies. For instance, it is usually insufficient to justify an application for research funds by stating that one's intuition says that a particular research should be carried out. A research council ideally operates as society's test of whether the researcher can account, in collectively acceptable ways, for his or her intuitive choice, even though intuition may be the real, or most important, reason why the researcher wants to execute the project.

It is not possible consistently, or even frequently, to determine in advance whether or not a given case—Geertz's cockfights in Bali, for instance—is paradigmatic. Besides the strategic choice of case, the execution of the case study will certainly play a role, as will the reactions to the study by the research community, the group studied, and, possibly, a broader public. The value of the case study will depend on the validity claims that researchers can place on their study, and the status these claims obtain in dialogue with other validity claims in the discourse to which the study is a contribution. Like other good craftspeople, all that researchers can do is use their experience and intuition to assess whether they believe a given case is interesting in a paradigmatic context, and whether they can provide collectively acceptable reasons for the choice of case.

Concerning considerations of strategy in the choice of cases, it should also be mentioned that the various strategies of selection are not necessarily mutually exclusive. For example, a case can be simultaneously extreme, critical, and paradigmatic. The interpretation of such a case can provide a unique wealth of information, because one obtains various perspectives on and conclusions about the case according to whether it is viewed and interpreted as one or another type of case. Finally, a case that the researcher initially thought was one type may turn out to be another, upon closer study (see Box 17.3).

Box 17.3 From Critical Case to Extreme Case, Unwittingly

When I was planning a case study of rationality and power in urban policy and planning in Aalborg, Denmark, reported in Flyvbjerg (1998a), I tried to design the study as a "most likely" critical case in the following manner: If rationality in urban policy and planning were weak in the face of power in Aalborg, then, most likely, they would be weak anywhere, at least in Denmark, because in Aalborg the rational paradigm of policy and planning stood stronger than anywhere else. Eventually, I realized that this logic was flawed, because my research of local relations of power showed that one of the most influential "faces of power" in Aalborg, the Chamber of Industry and Commerce, was substantially stronger than its equivalents elsewhere. This had not been clear at the outset because much less research existed on local power relations than research on local planning. Therefore, instead of a critical case, unwittingly I ended up with an extreme case in the sense that both rationality and power were unusually strong in Aalborg. My study thus became one of what happens when strong rationality meets strong power in the arena of urban policy and planning. But this selection of Aalborg as an extreme case happened to me; I did not deliberately choose it. It was a frustrating experience, especially during those several months after I realized I did not have a critical case until it became clear that all was not lost because I had something else. As a case researcher charting new terrain, one must be prepared for such incidents, I believe.

Misunderstanding No. 4

The case study contains a bias toward verification, that is, a tendency to confirm the researcher's preconceived notions.

The fourth of the five misunderstandings about case study research is that the method maintains a bias toward verification, understood as a tendency to confirm the researcher's preconceived notions, so that the study therefore becomes of doubtful scientific value. Jared Diamond (1996, p. 6), for example, holds this view. He observes that the case study suffers from what he calls a "crippling drawback," because it does not apply "scientific methods," which Diamond understands as methods useful for "curbing one's tendencies to stamp one's preexisting interpretations on data as they accumulate."

Francis Bacon (1853, p. xlvi) saw this bias toward verification not simply as a phenomenon related to the case study in particular, but as a fundamental human characteristic. Bacon expressed it like this:

> The human understanding from its peculiar nature, easily supposes a greater degree of order and equality in things than it really finds. When any proposition has been laid down, the human understanding forces everything else to add fresh support and confirmation. It is the peculiar and perpetual error of the human understanding to be more moved and excited by affirmatives than negatives.

Bacon certainly touches upon a fundamental problem here, a problem that all researchers must deal with in some way. Charles Darwin (Barlow, 1958, p. 123), in his autobiography, describes the method he developed in order to avoid the bias toward verification:

> I had . . . during many years followed a golden rule, namely, that whenever a published fact, a new observation or thought came across me, which was opposed to my general results, to make a memorandum of it without fail and at once; for I had found by experience that such facts and thoughts were far more apt to escape from the memory than favorable ones. Owing to this habit, very few objections were raised against my views, which I had not at least noticed and attempted to answer.

The bias toward verification is general, but the alleged deficiency of the case study and other qualitative methods is that they ostensibly allow more room for the researcher's subjective and arbitrary judgment than other methods: They are often seen as less rigorous than are quantitative, hypothetico-deductive methods. Even if such criticism is useful, because it sensitizes us to an important issue, experienced case researchers cannot help but see the critique as demonstrating a lack of knowledge of what is involved in case study research. Donald Campbell and others have shown that the critique is fallacious, because the case study has its own rigor, different to be sure, but no less strict than the rigor of quantitative methods. The advantage of the case study is that it can "close in" on real-life situations and test views directly in relation to phenomena as they unfold in practice.

According to Campbell, Ragin, Geertz, Wieviorka, Flyvbjerg, and others, researchers who have conducted intensive, in-depth case studies, typically report that their preconceived views, assumptions, concepts, and hypotheses were wrong and that the case material has compelled them to revise their hypotheses on essential points. The case study forces upon the researcher the type of falsifications described above. Ragin (1992, p. 225) calls this a "special feature of small-*N* research," and goes on to explain that criticizing single case studies for being inferior to multiple case studies is misguided, since even single case studies

"are multiple in most research efforts because ideas and evidence may be linked in many different ways."

Geertz (1995, p. 119) says about the fieldwork involved in most in-depth case studies that "The Field" itself is a "powerful disciplinary force: assertive, demanding, even coercive." Like any such force, it can be underestimated, but it cannot be evaded. "It is too insistent for that," says Geertz. That he is speaking of a general phenomenon can be seen by simply examining case studies, such as those by Eckstein (1975), Campbell (1975), and Wieviorka (1992). Campbell (1975, pp. 181–182) discusses the causes of this phenomenon in the following passage:

> In a case study done by an alert social scientist who has thorough local acquaintance, the theory he uses to explain the focal difference also generates prediction or expectations on dozens of other aspects of the culture, and he does not retain the theory unless most of these are also confirmed. . . . Experiences of social scientists confirm this. Even in a single qualitative case study, the conscientious social scientist often finds no explanation that seems satisfactory. Such an outcome would be impossible if the caricature of the single case study . . . were correct—there would instead be a surfeit of subjectively compelling explanations.

According to the experiences cited above, it is falsification and not verification that characterizes the case study. Moreover, the question of subjectivism and bias toward verification applies to all methods, not just to the case study and other qualitative methods. For example, the element of arbitrary subjectivism will be significant in the choice of categories and variables for a quantitative or structural investigation, such as a structured questionnaire to be used across a large sample of cases. And the probability is high that (1) this subjectivism survives without being thoroughly corrected during the study, and (2) that it may affect the results, quite simply because the quantitative/structural researcher does not get as close to those under study as does the case study researcher and therefore is less likely to be corrected by the study objects "talking back." George and Bennett (2005, p. 20) describe this all-important feature of case study research like this:

> When a case study researcher asks a participant "were you thinking X when you did Y," and gets the answer, "No, I was thinking Z," then if the researcher had not thought of Z as a causally relevant variable, she may have a new variable demanding to be heard.

Statistical methods may identify deviant cases that can lead to new hypotheses, but in isolation these methods lack any clear means of actually identifying new hypotheses. This is true of all studies that use existing databases or that collect survey data based on questionnaires with predefined standard questions. Unless statistical researchers do their own archival work, interviews, or face-to-face surveys with open-ended questions—like case study researchers—they have no means of identifying left-out variables (George & Bennett, 2005, p. 21). According to Ragin (1992, p. 225; see also Ragin, 1987, pp. 164–171):

> This feature explains why small-*N* qualitative research is most often at the forefront of theoretical development. When *N*'s are large, there are few opportunities for revising a casing [that is, the delimitation of a case]. At the start of the analysis, cases are decomposed into variables, and almost the entire dialogue of ideas and evidence occurs through variables. One implication of this discussion is that to the extent that large-*N* research can be sensitized to the diversity and potential heterogeneity of the cases included in an analysis, large-*N* research may play a more important part in the advancement of social science theory.

Here, too, this difference between large samples and single cases can be understood in terms of the phenomenology for human learning discussed above. If one thus assumes that the goal of the researcher's work is to understand and learn about the phenomena being studied, then research is simply a form of learning. If one assumes that research, like other learning processes, can be described by the phenomenology for human learning, it then becomes clear that the most advanced form of understanding is achieved when researchers place themselves within the context being studied. Only in this way can researchers understand the viewpoints and the behavior that characterizes social actors. Relevant to this point, Giddens states that valid descriptions of social activities presume that researchers possess those skills necessary to participate in the activities described:

> I have accepted that it is right to say that the condition of generating descriptions of social activity is being able in principle to participate in it. It involves "mutual knowledge," shared by observer and participants whose action constitutes and reconstitutes the social world. (1982, p. 15)

From this point of view, the proximity to reality, which the case study entails, and the learning process that it generates for the researcher will often constitute a prerequisite for advanced understanding. In this context, one begins to understand Beveridge's conclusion that there are more discoveries stemming from intense observation of individual cases than from statistics applied to large groups. With the point of departure in the learning process, we understand why the researcher who conducts a case study often ends up by casting off preconceived notions and theories. Such activity is quite simply a central element in learning and in the achievement of new insight. More simple forms of understanding must yield to more complex ones as one moves from beginner to expert.

On this basis, the fourth misunderstanding—that the case study supposedly contains a bias toward verification, understood as a tendency to confirm the researcher's preconceived ideas—is revised as follows:

The case study contains no greater bias toward verification of the researcher's preconceived notions than other methods of inquiry. On the contrary, experience indicates that the case study contains a greater bias toward falsification of preconceived notions than toward verification.

Misunderstanding No. 5

It is often difficult to summarize and develop general propositions and theories on the basis of specific case studies.

Case studies often contain a substantial element of narrative and one can get into a terrible quicksand today talking about the matter of narrative in social science (for a good overview of narrative inquiry, see Chapter 25 in this volume by Susan Chase; Todd Landman, in press). After certain strands of discourse theory have defined any text as narrative and everything as text, it seems that narrative is everything. But if something is everything, maybe it is nothing, and we are back to square one. It is difficult to avoid the subject of narrative completely, however, when considering the case study and qualitative research. In my own work, when I think about narrative, I do not think of discourse theory but of Miles Davis, the jazz icon. When asked how he kept writing classics through a four-decades-long career, he answered, "I first write a beginning, then a middle, and finally the ending." Narrative suggests questions about plot, that is, a sequence of events and how they are related, and Davis set out the naked minimum. Obviously, plots and narratives may be hatched in many ways. But if you write the kind of classic narrative that Davis talks about, with a beginning, a middle, and an end, you typically first try to get the attention of the reader, often by means of a hook, that is, a particularly captivating event or problematic that leads into the main story. You then present the issues and who are involved, including their relationships. Gradually, you reel in the reader to a point of no return, from where the main character—who in a case study need not be a person but could be, say, a community, a program, or a company—has no choice but to deal with the issues at hand, and in this sense is tested. At this stage, typically, there is conflict and the conflict escalates. Finally, harmony is restored by the conflict being resolved, or at least explained, as may be the appropriate achievement in a social science narrative.

To Alasdair MacIntyre (1984, pp. 214, 216), the human being is a "story-telling animal," and the notion of a history is as fundamental a human notion as the notion of an action. Other observers have noted that narrative seems to exist in all human societies, modern and ancient, and that it is perhaps our most fundamental form for making sense of experience (Mattingly, 1991, p. 237; Novak, 1975, p. 175; see also Abbott, 1992; Arendt, 1958; Bal, 1997; Carr, 1986; Fehn, Hoestery, & Tatar, 1992; Rasmussen, 1995; Ricoeur, 1984). Narrative thus seems not only to be the creation of the storyteller, but seems also to be an expression of innate relationships in the human mind, which we use to make sense of the world by constructing it as narrative.

The human propensity for narrative involves a danger, however, of what has been called the narrative fallacy. The fallacy consists of a human inclination to simplify data and information through overinterpretation and through a preference for compact stories over complex data sets (Taleb, 2010, p. 63). It is easier to remember and make decisions on the basis of "meaningful" stories than to remember strings of "meaningless" data. Thus, we read meaning into data and make up stories, even where this is unwarranted. As a case in point, consider the inspirational accounts of how the Internet led to a "new economy" where productivity had been disconnected from share prices; or the fairy tale that increasing real estate prices are enough to sustain economic growth in a nation. Such stories are easy to understand and act on—for citizens, policy makers, and scholars—but they are fallacies and as such they are treacherous. In social science, the means to avoid the narrative fallacy is no different from the means to avoid other error: the usual systematic checks for validity and reliability in how data are collected and used.

Dense narratives based on thick description will provide some protection against the narrative fallacy. Such narratives typically approach the complexities and contradictions of real life. Accordingly, they may be difficult or impossible to summarize into neat formulas, general propositions, and theories (Benhabib, 1990; Mitchell & Charmaz, 1996; Roth, 1989; Rouse, 1990; White, 1990). This tends to be seen by critics of the case study as a drawback. To the case study researcher, however, a particularly "thick" and hard-to-summarize narrative is not a problem. Rather, it is often a sign that the study has uncovered a particularly rich problematic. The question, therefore, is whether the summarizing and generalization, which the critics see as an ideal, is always desirable. Friedrich Nietzsche (1974, p. 335, para. 373) is clear in his answer to this question. "Above all," he says about doing science, "one should not wish to divest existence of its *rich ambiguity*" (emphasis in original).

Lisa Peattie (2001, p. 260) explicitly warns against summarizing dense case studies: "It is simply that the very value of the case study, the contextual and interpenetrating nature of forces, is lost when one tries to sum up in large and mutually exclusive concepts." The dense case study, according to Peattie, is more useful for the practitioner and more interesting for social theory than either factual "findings" or the high-level generalizations of theory.

The opposite of summing up and "closing" a case study is to keep it open. Two strategies work particularly well in ensuring openness. First, when writing up their case studies, authors may demur from the role of omniscient narrator and summarizer. Instead, they may choose to tell the story in its diversity, allowing the story to unfold from the many-sided, complex, and sometimes-conflicting stories that the actors in the case have

told researchers. Second, authors of case studies may avoid linking their study with the theories of any one academic specialization. Instead, they may choose to relate the case to broader philosophical positions that cut across specializations. In this way, authors leave scope for readers of different backgrounds to make different interpretations and draw diverse conclusions regarding the question of what the case is a case of. The goal is not to make the case study be all things to all people. The goal is to allow the study to be different things to different people. Here it is useful to describe the case with so many facets—like life itself—that different readers may be attracted, or repelled, by different things in the case. Readers are not pointed down any one theoretical path or given the impression that truth might lie at the end of such a path. Readers will have to discover their own path and truth inside the case. Thus, in addition to the interpretations of case actors and case narrators, readers are invited to decide the meaning of the case and to interrogate actors' and narrators' interpretations in order to answer that categorical question of any case study: "What is this case a case of?"

Case stories written like this can neither be briefly recounted nor summarized in a few main results. The case story is itself the result. It is a "virtual reality," so to speak. For the reader willing to enter this reality and explore it inside and out, the payback is meant to be a sensitivity to the issues at hand that cannot be obtained from theory. Students can safely be let loose in this kind of reality, which provides a useful training ground with insights into real-life practices that academic teaching often does not provide.

If we return again briefly to the phenomenology for human learning, we may understand why summarizing case studies is not always useful and may sometimes be counterproductive. Knowledge at the beginner's level consists precisely in the reduced formulas that characterize theories, while true expertise is based on intimate experience with thousands of individual cases and on the ability to discriminate between situations, with all their nuances of difference, without distilling them into formulas or standard cases. The problem is analogous to the inability of heuristic, computer-based expert systems to approach the level of virtuoso human experts, even when the systems are compared with the experts who have conceived the rules upon which these systems operate. This is because the experts do not use rules but operate on the basis of detailed case experience. This is *real* expertise. The rules for expert systems are formulated only because the systems require it; rules are characteristic of expert *systems,* but not of real human *experts.*

In the same way, one might say that the rule formulation that takes place when researchers summarize their work into theories is characteristic of the culture of research, of researchers, and of theoretical activity, but such rules are not necessarily part of the studied reality constituted by Bourdieu's (1977, pp. 8, 15) "virtuoso social actors." Something essential may be lost by this summarizing—namely the possibility to understand virtuoso social acting, which, as Bourdieu has shown, cannot be distilled into theoretical formulas—and it is precisely their fear of losing this "something" that makes case researchers cautious about summarizing their studies. Case researchers thus tend to be skeptical about erasing phenomenological detail in favor of conceptual closure.

Ludwig Wittgenstein shared this skepticism. According to Gasking and Jackson, Wittgenstein used the following metaphor when he described his use of the case study approach in philosophy:

> In teaching you philosophy I'm like a guide showing you how to find your way round London. I have to take you through the city from north to south, from east to west, from Euston to the embankment and from Piccadilly to the Marble Arch. After I have taken you many journeys through the city, in all sorts of directions, we shall have passed through any given street a number of times—each time traversing the street as part of a different journey. At the end of this you will know London; you will be able to find your way about like a born Londoner. Of course, a good guide will take you through the more important streets more often than he takes you down side streets; a bad guide will do the opposite. In philosophy I'm a rather bad guide. (1967, p. 51)

This approach implies exploring phenomena firsthand instead of reading maps of them. Actual practices are studied before their rules, and one is not satisfied by learning only about those parts of practices that are open to public scrutiny; what Erving Goffman (1963) calls the "backstage" of social phenomena must be investigated, too, like the side streets that Wittgenstein talks about.

With respect to intervention in social and political affairs, Andrew Abbott (1992, p. 79) has rightly observed that a social science expressed in terms of typical case narratives would provide "far better access for policy intervention than the present social science of variables." Alasdair MacIntyre (1984, p. 216) similarly says, "I can only answer the question 'What am I to do?' if I can answer the prior question 'Of what story or stories do I find myself a part?'" In a similar vein, Cheryl Mattingly (1991, p. 237) points out that narratives not only give meaningful form to experiences we have already lived through, they also provide us a forward glance, helping us to anticipate situations even before we encounter them, allowing us to envision alternative futures. Narrative inquiries do not—indeed, cannot—start from explicit theoretical assumptions. Instead, they begin with an interest in a particular phenomenon that is best understood narratively. Narrative inquiries then develop descriptions and interpretations of the phenomenon from the perspective of participants, researchers, and others.

William Labov and Joshua Waletzky (1966, pp. 37–39) write that when a good narrative is over, "it should be unthinkable for a bystander to say, 'So what?'" Every good narrator is continually warding off this question. A narrative that lacks a moral that can be independently and briefly stated, is not necessarily pointless. And a narrative is not successful just because it allows a brief moral. A successful narrative does not allow the question to be raised at all. The narrative has already supplied the answer before the question is asked. The narrative itself is the answer (Nehamas, 1985, pp. 163–164).

A reformulation of the fifth misunderstanding, which states that it is often difficult to summarize specific case studies into general propositions and theories, thus reads as follows:

> *It is correct that summarizing case studies is often difficult, especially as concerns case process. It is less correct as regards case outcomes. The problems in summarizing case studies, however, are due more often to the properties of the reality studied than to the case study as a research method. Often it is not desirable to summarize and generalize case studies. Good studies should be read as narratives in their entirety.*

It must again be emphasized that despite the difficulty or undesirability in summarizing certain case studies, the case study as such can certainly contribute to the cumulative development of knowledge, for example, in using the principles to test propositions described above under the second and third misunderstandings.

Current Trends in Case Study Research

This chapter began by pointing out a paradox in case study research, namely, that even as case studies are widely used in social science and have produced many of the classic texts here, it may be observed that the case study as a methodology is generally held in low regard, or is simply ignored, within large and dominant parts of the academy. This state of affairs has proved remarkably long-lived.

However, as pointed out by George and Bennett (2005, pp. 4–5), recently a certain loosening of positions has taken place. A more collaborative approach is gaining ground, where scholars begin to see that different methodological approaches have different strengths and weaknesses and are essentially complementary. The old and often antagonistic division between quants and qualts is losing ground as a new generation of scholars trained in both quantitative and qualitative methods is emerging. For these scholars, research is problem-driven and not methodology-driven, meaning that those methods are employed that for a given problematic best help answer the research questions at hand. More often than not, a combination of qualitative and quantitative methods will do the task best. Finally, some of the most ambitious claims regarding how the quantitative revolution would make possible a social science on a par with natural science in its ability to explain and predict have been scaled back, making room for the emergence of a more realistic and balanced attitude to what social science can and cannot do. The chapters in this volume on mixed methods, by John Creswell (Chapter 15), and Charles Teddlie and Abbas Tashakkori (Chapter 16), are good examples of this loosening of positions and more balanced attitude.

If the moment of the quantitative revolution in social science is called positivistic, as is often the case, then today we are in a postpositivist and possibly post-paradigmatic moment (Schram, 2006). My own efforts at developing a social science suited for this particular moment have been concentrated on what I call "phronetic social science," named after the ancient Greek term for practical wisdom, or common sense, *phronesis* (Flyvbjerg, 2001; Schram & Caterino, 2006). And this is what the new social science is: commonsensical. It is common sense to give up wars that cannot be won, like the methods war over quantitative versus qualitative methods, or the science wars, which pit social science against natural science. It is also common sense to finally acknowledge that case studies and statistical methods are not conflicting but complementary (see Box 17.4).

Box 17.4 Complementarity in Action: From Case Studies to Statistical Methods, and Back

My current research on megaprojects was originally sparked by events at the Channel tunnel, which links the United Kingdom and France, and the Danish Great Belt tunnel, linking Scandinavia with continental Europe. These are the two longest underwater rail tunnels in Europe, each costing several billion dollars. Soon after construction of the Channel tunnel began, costs started escalating, and at the opening of the tunnel, in 1994, costs had doubled in real terms leaving the project in serious financial trouble. But maybe the British and French had just been unlucky? Perhaps the Danes would do better on the Great Belt tunnel? Not so. Here the cost overrun was larger still, at 120% in real terms, and the tunnel proved financially nonviable even before it opened to traffic in 1997, several years behind schedule. I did a case study of these two hugely expensive projects in order to document and understand the apparent incompetence in their planning and execution (Flyvbjerg, Bruzelius, & Rothengatter, 2003). The study raised the inevitable question of whether the Channel and Great Belt tunnels were outliers regarding cost overrun and viability or whether such extreme lack of ability to build on budget was common for large-scale infrastructure projects. Searching the world's libraries and asking colleagues, I found that no study existed that answered these questions in a statistically valid manner. I therefore decided to do such a study and my group and I now turned from case studies to statistical methods. To our amazement, our studies showed, with a very high level of statistical significance, that the Channel and Great Belt projects were not outliers, they were normal; nine out of ten projects have cost overrun. Even more surprisingly, when we extended our data back in time we

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found that for the 70 years for which we were able to find data there had been no improvement in performance regarding getting cost estimates right and staying on budget. The same apparent error of cost underestimation and overrun was being repeated decade after decade. We now began debating among ourselves whether an error that is being repeated over and over by highly trained professionals is really an error, or whether something else was going on. To answer this question, we went back to case studies and process tracing (see Box 17.1). We found that cost overrun and lack of viability were not best explained by simple error but by something more sinister and Machiavellian, namely strategic misrepresentation of costs and benefits by promoters during appraisal in order to get projects funded and built. From my initial case-based curiosity with the outcomes at the Channel and Great Belt tunnels—and by going from case studies to statistical methods and back—my group and I had uncovered a deep-rooted culture of deception in the planning and management of large-scale infrastructure projects (Flyvbjerg, 2007). As a recent spin-off from this research, my group and I are now investigating whether the success of one in ten projects in staying on budget—documented in our statistical studies—may be replicated or is due to luck. Here, again, we are back to case study research, now studying success as a deviant case.

The complementarity between case studies and statistical methods may be summarized as in Table 17.2. The main strength of the case study is depth—detail, richness, completeness, and within-case variance—whereas for statistical methods it is breadth. If you want to understand a phenomenon in any degree of thoroughness—say, child neglect in the family or cost overrun in urban regeneration—what causes it, how to prevent it, and so on, you need to do case studies. If you want to understand how widespread the phenomenon is, how it correlates with other phenomena and varies across different populations, and at what level of statistical significance, then you have to do statistical studies. If you want to understand both, which is advisable if you would like to speak with weight about the phenomenon at hand, then you need to do both case studies and statistical analyses. The complementarity of the two methods is that simple, and that beautiful.

When you think about it, it is amazing that the separation and antagonism between qualitative and quantitative methods often seen in the literature, and in university departments, have lasted as long as they have. This is what happens when tribalism and power, instead of reason, rules the halls of academia. As such, it is testimony to the fact that academics, too, are all too human, and not testimony to much else. The separation is not a logical consequence of what graduates and scholars need to know in order to do their studies and do them well; quite the opposite. Good social science is opposed to an either/or and stands for a both/and on the question of qualitative versus quantitative methods. The *International Encyclopedia of the Social & Behavioral Sciences*

Table 17.2 Complementarity of Case Studies and Statistical Methods

	Case Studies	*Statistical Methods*
Strengths	■ Depth	■ Breadth
	■ High conceptual validity	■ Understanding how widespread a phenomenon is across a population
	■ Understanding of context and process	■ Measures of correlation for populations of cases
	■ Understanding of what causes a phenomenon, linking causes and outcomes	■ Establishment of probabilistic levels of confidence
	■ Fostering new hypotheses and new research questions	
Weaknesses	■ Selection bias may overstate or understate relationships	■ Conceptual stretching, by grouping together dissimilar cases to get larger samples
	■ Weak understanding of occurrence in population of phenomena under study	■ Weak understanding of context, process, and causal mechanisms
	■ Statistical significance often unknown or unclear	■ Correlation does not imply causation
		■ Weak mechanisms for fostering new hypotheses

(Smelser & Baltes, 2001, p. 1513) is certainly right when it points out that the case study and statistical methods can "achieve far more scientific progress together than either could alone."

This being said, it should nevertheless be added that the balance between case studies and statistical methods is still biased in favor of the latter in social science, so much so that it puts case studies at a disadvantage within most disciplines. For the time being, it is therefore necessary to continue to work on clarifying methodologically the case study and its relations to other social science methods in order to dispel the methodological limbo in which the method has existed for too long. This chapter is intended as such clarification.

Note

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References

Abbott, A. (1992). What do cases do? Some notes on activity in sociological analysis. In C. C. Ragin & H. S. Becker (Eds.), *What is a case? Exploring the foundations of social inquiry* (pp. 53–82). Cambridge, UK: Cambridge University Press.

Abercrombie, N., Hill, S., & Turner, B. S. (1984). *Dictionary of sociology.* Harmondsworth, UK: Penguin.

Arendt, H. (1958). *The human condition.* Chicago: University of Chicago Press.

Bacon, F. (1853). Novum organum. In *Physical and metaphysical works of Lord Bacon* (Vol. 1). London: H. G. Bohn.

Bailey, M. T. (1992). Do physicists use case studies? Thoughts on public administration research. *Public Administration Review, 52*(1), 47–54.

Bal, M. (1997). *Narratology: Introduction to the theory of narrative* (2nd ed.). Toronto: University of Toronto Press.

Barlow, N. (Ed.). (1958). *The autobiography of Charles Darwin.* New York: Norton.

Barzelay, M. (1993). The single case study as intellectually ambitious inquiry. *Journal of Public Administration Research and Theory, 3*(3), 305–318.

Bates, R., Greif, A., Levi, M., Rosenthal, J.-L., & Weingast, B. (1998). *Analytic narratives.* Princeton, NJ: Princeton University Press.

Benhabib, S. (1990). Hannah Arendt and the redemptive power of narrative. *Social Research, 57*(1), 167–196.

Beveridge, W. I. B. (1951). *The art of scientific investigation.* London: Heinemann.

Blaug, M. (1980). *The methodology of economics: Or how economists explain.* Cambridge, UK: Cambridge University Press.

Bourdieu, P. (1977). *Outline of a theory of practice.* Cambridge, UK: Cambridge University Press.

Campbell, D. T. (1975). Degrees of freedom and the case study. *Comparative Political Studies, 8*(1), 178–191.

Campbell, D. T., & Stanley, J. C. (1966). *Experimental and quasi-experimental designs for research.* Chicago: Rand McNally.

Carr, D. (1986). *Time, narrative, and history.* Bloomington: Indiana University Press.

Christensen, C. R., & Hansen, A. J. (Eds.). (1987). *Teaching and the case method.* Boston, MA: Harvard Business School Press.

Cragg, C. I. (1940). Because wisdom can't be told (Harvard Business School Reprint 451–005). *Harvard Alumni Bulletin,*1–6.

Diamond, J. (1996, November 14). The roots of radicalism. *The New York Review of Books,* pp. 4–6.

Dreyfus, H., & Dreyfus, S. (with Athanasiou, T.). (1986). *Mind over machine: The power of human intuition and expertise in the era of the computer.* New York: Free Press.

Eckstein, H. (1975). Case study and theory in political science. In F. J. Greenstein & N. W. Polsby (Eds.), *Handbook of political science* (Vol. 7, pp. 79–137). Reading, MA: Addison-Wesley.

Eysenck, H. J. (1976). Introduction. In H. J. Eysenck (Ed.), *Case studies in behaviour therapy.* London: Routledge and Kegan Paul.

Fehn, A., Hoestery, I., & Tatar, M. (Eds.). (1992). *Neverending stories: Toward a critical narratology.* Princeton, NJ: Princeton University Press.

Flyvbjerg, B. (2001). *Making social science matter: Why social inquiry fails and how it can succeed again.* Cambridge, UK: Cambridge University Press.

Flyvbjerg, B. (2006). A perestroikan straw man answers back: David Laitin and phronetic political science. In S. F. Schram & B. Caterino (Eds.), *Making political science matter: Debating knowledge, research, and method* (pp. 56–85). New York and London: New York University Press.

Flyvbjerg, B. (2007). Policy and planning for large-infrastructure projects: Problems, causes, cures. *Environment and Planning B: Planning and Design, 34*(4), 578–597.

Flyvbjerg, B., Bruzelius, N., & Rothengatter, W. (2003). *Megaprojects and risk: An anatomy of ambition.* Cambridge, UK: Cambridge University Press.

Flyvbjerg, B., Holm, M. K. S., & Buhl, S. L. (2002). Underestimating costs in public works projects: Error or lie? *Journal of the American Planning Association, 68*(3), 279–295.

Gasking, D. A. T., & Jackson, A. C. (1967). Wittgenstein as a teacher. In K. T. Fann (Ed.), *Ludwig Wittgenstein: The man and his philosophy* (pp. 49–55). Sussex, UK: Harvester Press.

Geertz, C. (1995). *After the fact: Two countries, four decades, one anthropologist.* Cambridge, MA: Harvard University Press.

George, A. L., & Bennett, A. (2005). *Case studies and theory development in the social sciences.* Cambridge, MA: MIT Press.

Gerring, J. (2004). What is a case study and what is it good for? *The American Political Science Review, 98*(2), 341–354.

Giddens, A. (1982). *Profiles and critiques in social theory.* Berkeley: University of California Press.

Giddens, A. (1984). *The constitution of society: Outline of the theory of structuration.* Cambridge, UK: Polity Press.

Goffman, E. (1963). *Behavior in public places: Notes on the social organization of gatherings.* New York: Free Press.

Goldthorpe, J. H., Lockwood, D., Beckhofer, F., & Platt, J. (1968–1969). *The affluent worker* (Vols. 1–3). Cambridge, UK: Cambridge University Press.

Griffin, L. J., Botsko, C., Wahl, A.-M., & Isaac, L. W. (1991). Theoretical generality, case particularity: Qualitative comparative analysis of trade union growth and decline. In C. C. Ragin (Ed.), *Issues and alternatives in comparative social research* (pp. 110–136). Leiden, The Netherlands: E. J. Brill.

Kuper, A., & Kuper, J. (Eds.). (1985). *The social science encyclopedia.* London: Routledge and Kegan Paul.

Labov, W., & Waletzky, J. (1966). Narrative analysis: Oral versions of personal experience. In *Essays on the verbal and visual arts: Proceedings of the American Ethnological Society* (pp. 12–44). Seattle, WA: American Ethnological Society.

Landman, T. (in press). Phronesis and narrative analysis. In B. Flyvbjerg, T. Landman, & S. Schram (Eds.), *Real social science: Applied phronesis.* Cambridge, UK: Cambridge University Press.

Lee, A. S. (1989). Case studies as natural experiments. *Human Relations, 42*(2), 117–137.

Lovallo, D., & Kahneman, D. (2003, July). Delusions of success: How optimism undermines executives' decisions. *Harvard Business Review,* 56–63.

MacIntyre, A. (1984). *After virtue: A study in moral theory* (2nd ed.). Notre Dame, IN: University of Notre Dame Press.

Mattingly, C. (1991). Narrative reflections on practical actions: Two learning experiments in reflective storytelling. In D. A. Schön (Ed.), *The reflective turn: Case studies in and on educational practice* (pp. 235–257). New York: Teachers College Press.

Merriam-Webster Online Dictionary. (2009). *Case study.* Available at http://www.merriam-webster.com/dictionary/case%20study

Michels, R. (1962). *Political parties: A study of the oligarchical tendencies of modern democracy.* New York: Collier.

Mitchell, R. G., Jr., & Charmaz, K. (1996). Telling tales, writing stories: Postmodernist visions and realist images in ethnographic writing. *Journal of Contemporary Ethnography, 25*(1), 144–166.

Nehamas, A. (1985). *Nietzsche: Life as literature.* Cambridge, MA: Harvard University Press.

Nietzsche, F. (1974). *The gay science.* New York: Vintage.

Novak, M. (1975). "Story" and experience. In J. B. Wiggins (Ed.), *Religion as story.* Lanham, MD: University Press of America.

Peattie, L. (2001). Theorizing planning: Some comments on Flyvbjerg's *Rationality and power. International Planning Studies, 6*(3), 257–262.

Platt, F. (1992). "Case study" in American methodological thought. *Current Sociology, 40*(1), 17–48.

Ragin, C. C. (1987). *The comparative method: Moving beyond qualitative and quantitative strategies.* Berkeley: University of California Press.

Ragin, C. C. (1992). "Casing" and the process of social inquiry. In C. C. Ragin & H. S. Becker (Eds.), *What is a case? Exploring the foundations of social inquiry* (pp. 217–226). Cambridge, UK: Cambridge University Press.

Ragin, C. C., & Becker, H. S. (Eds.). (1992). *What is a case? Exploring the foundations of social inquiry.* Cambridge, UK: Cambridge University Press.

Rasmussen, D. (1995). Rethinking subjectivity: Narrative identity and the self. *Philosophy and Social Criticism, 21*(5–6), 159–172.

Ricoeur, P. (1984). *Time and narrative.* Chicago: University of Chicago Press.

Rosch, E. (1978). Principles of categorization. In E. Rosch & B. B. Lloyd (Eds.), *Cognition and categorization* (pp. 27–48). Hillsdale, NJ: Lawrence Erlbaum.

Roth, P. A. (1989). How narratives explain. *Social Research, 56*(2), 449–478.

Rouse, J. (1990). The narrative reconstruction of science. *Inquiry, 33*(2), 179–196.

Schram, S. F. (2006). Return to politics: Perestroika, phronesis, and post-paradigmatic political science. In S. F. Schram & B. Caterino (Eds.), *Making political science matter: Debating knowledge, research, and method* (pp. 17–32). New York and London: New York University Press.

Schram, S. F., & Caterino, B. (Eds.). (2006). *Making political science matter: Debating knowledge, research, and method.* New York and London: New York University Press.

Smelser, N. J., & Baltes, P. B. (Eds.). (2001). *International encyclopedia of social & behavioral sciences.* Elmsford, NY: Pergamon.

Stake, R. E. (2008). Qualitative case studies. In N. K. Denzin & Y. S. Lincoln (Eds.), *Strategies of qualitative inquiry* (3rd ed., pp. 119–150). Thousand Oaks, CA: Sage.

Taleb, N. N. (2007). *The black swan: The impact of the highly improbable* (2nd ed.). London and New York: Penguin.

Walton, J. (1992). Making the theoretical case. In C. C. Ragin & H. S. Becker (Eds.), *What is a case? Exploring the foundations of social inquiry* (pp. 121–137). Cambridge, UK: Cambridge University Press.

White, H. (1990). *The content of the form: Narrative discourse and historical representation.* Baltimore: Johns Hopkins University Press.

Whyte, W. F. (1943). *Street corner society: The social structure of an Italian slum.* Chicago: University of Chicago Press.

Wieviorka, M. (1992). Case studies: History or sociology? In C. C. Ragin & H. S. Becker (Eds.), *What is a case? Exploring the foundations of social inquiry* (pp. 159–172). Cambridge, UK: Cambridge University Press.

Wilson, B. (1987). Single-case experimental designs in neuro-psychological rehabilitation. *Journal of Clinical and Experimental Neuropsychology, 9*(5), 527–544.