

Case Study Method

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12 CASE STUDY AND THEORY

Martyn Hammersley, Roger Gomm and Peter Foster

A common charge against case study research is that its findings are not generalizable in the way that those of social surveys are. The question often raised is: How do we know that these findings are representative? Some advocates of case study respond to this by arguing that it is directed towards a *different kind* of general conclusion from that offered by survey research: they suggest that case study work is designed to produce theories. Thus, Yin (1994) argues that it aims at 'analytical' not 'empirical' generalization, while Mitchell (Chapter 8) claims that it involves 'logical' rather than 'statistical' inference.¹

Various accounts have been produced of the role of case studies in producing theoretical conclusions. Abramson (1992) provides two versions which he acknowledges are contradictory: one framed in terms of a Baconian inductivist conception of science, the other in terms of Popperian falsificationism. Skocpol (1979, pp. 33–40; 1984) calls on the procedure of 'comparative historical analysis', which she sees as based on the methods of agreement and difference outlined by John Stuart Mill. Others appeal to interpretive or causal realism, arguing that case study gives access to the inner lives of people, to the emergent properties of social interaction, and/or to the underlying causal mechanisms which generate human behaviour (Burgess, 1927; Waller, 1934; Connolly, 1998). Finally, there is the notion of 'analytic induction', originally outlined by Znaniecki, which he contrasts with the 'enumerative induction' that underpins statistical method (Znaniecki, 1934; see also Lindesmith, 1937) - an account of how theory can be produced through case study which remains influential today (see Becker, 1998; see also Chapter 11).

These various rationales can be organized under two main headings: those which appeal to direct perception of causal relations; and those which emphasize the role of

comparative method, in one form or another. We will discuss each type of rationale in turn.

Case study as revealing theoretical relations *in situ*

One way in which the theoretical value of case study is sometimes conceptualized is the argument that it can uncover the causal *processes* linking inputs and outputs within a system. These are the terms, for [p. 235 ↓] instance, in which Lacey (1970, 1976) justified studying a single school in order to throw light on the effects of academic differentiation on social class variations in educational performance. He pointed out that what went on within schools had previously been treated as a black box, and suggested that by opening this up the processes could be revealed through which differences in pupils' home backgrounds are translated into social class inequalities in educational outcomes. Moreover, some writers go beyond this to argue that through case study we can actually see causal relationships occurring in particular instances. Thus, Glaser and Strauss (1967) claim that 'in field work...general relations are often discovered *in vivo*; that is, the field worker literally sees them occur' (p. 40).²

The idea that case study provides direct insight into causal relations can be filled out in different ways. The two most influential ones are both found in an early article by Willard Waller (1934). He begins from Gestalt psychology, arguing that the traditional Humean objection to the direct perception of causal relationships is based on a false associationist psychology. He argues that perception is always of patterns, rather than of isolated sense data; and that some of these patterns are temporal and capture causal relations. For Gestalt theory, he notes, cause is 'an elementary datum of experience'; and that 'if one perceives a single instance correctly, he can generalize from that instance' (p. 287). Thus, Waller rejects the view of some positivists that causality is a metaphysical assumption, and the idea that demonstrating causal relationships requires 'extra-mental manipulations' (p. 287). He argues that:

In studying any set of phenomena directly, we pass them before our eyes in the attempt to discover recurrent patterns and, if possible, to

make out the entire configuration of events...These recurrent patterns gradually crystallise into concepts. Concepts result from the capacity of the mind to perceive the similarity of configurations perceived in succession. Concepts may be defined as transposable perceptual patterns to which we have given names. Imagination is often called into play to fit together pieces of configurations, to perceive with insight configurations of events which have not actually been present to the senses. A high degree of insight into causal relations is implicit in the scientific concept. A concept must be transposable not only from one set of phenomena to another but also from one mind to another. The most effective way to communicate concepts is always to describe or to point to phenomena and to give to each configuration of events its name. (pp. 289–90)

On this basis, he suggests that science is 'akin to the artistic process; it is a process of selecting out those elements of experience which fit together and recombining them in the mind' (p. 290).

In the same article, Waller introduces a different (though by no means incompatible) argument that is specific to the understanding of [p. 236 ↓] other people and their actions. He refers to this variously as 'sympathetic understanding', 'sympathetic penetration' or 'sympathetic insight'. He argues that: The social sciences differ from the physical sciences in that our knowledge of human beings is internally as well as externally derived.' He quotes Cooley in support: '[Sympathetic penetration] is derived from contact with the minds of other men, through communication, which sets going a process of thought and sentiment similar to theirs and enables us to understand them by sharing their states of mind' (pp. 294–5).

This idea can be traced back to nineteenth-century German views about the role of *Verstehen* in historical scholarship (and beyond this to the writings of Vico in the eighteenth century). The argument is that since actions, institutions and societies are human products - or, in more recent language, are social constructions - they can be understood by human beings in a direct way that is not possible when it comes to physical objects.³ In Waller's terms, understanding consists of 'imagining what it would

be like to be somebody else' (p. 295). And he sees case study as the literary form 'which most usefully condenses and organises sympathetic insight' (p. 295).

In a much more recent article, Paul Connolly (1998) adopts a position which is similar in some key respects; though it appeals to a rather different epistemological view, what has come to be labelled 'critical realism' (see Harré, 1970, 1986; Bhaskar, 1975). Drawing on the work of Sayer (1992), he distinguishes between 'extensive' and 'intensive' research designs, arguing that the task of intensive research is 'identifying and analysing the particular social processes and practices that cause change'. Connolly (1998) suggests that detailed description can 'uncover the meaning' people 'attach' to their own and others' behaviour, and thereby 'begin to unravel the causes of an individual's or a group's behaviour' (p. 124). According to him, the primary goal of ethnographic studies is to discover the causal relationships operating in the case studied, rather than to test whether these relationships occur elsewhere; though he sees such analyses as drawing on accounts of causal mechanisms operating in other cases produced by earlier studies, and as being a resource for later work in other contexts.

Like Waller, Connolly treats quantitative and qualitative method as complementary, but with an emphasis on the value of the latter. He claims that quantitative work 'aims to produce generalisations but can tell us little about causal relations, while [qualitative work] can help to identify relations of causality, but is unable to generalise from these' (p. 124). While he does not see this difference as clear-cut, he believes that it points to the relative strengths of the two approaches. Thus, he concludes that while quantitative methods can try to isolate the effects of one variable on another, 'they can still never, in the last analysis, conclude with the degrees of certainty associated with qualitative methods [p. 237 ↓] that a particular correlation...is a causal one'. Indeed, he suggests that 'it remains the role of qualitative research to prove or disprove [causal claims] by exploring and analysing the meanings and justificatory frameworks that those involved attach to their actions' (p. 125). Here, then, as with Waller, we have the idea that causal relations can be found by direct study of particular cases - and, in particular, of the interpretations, intentions and motives of the people whose behaviour is to be explained.

Connolly is less explicit than Waller about the means by which causal relations can be uncovered through case study. What seems to be involved, though, is that once a correlation has been found by statistical means, case study researchers should investigate the causal mechanisms by which it was produced, through documenting the processes occurring in one or more cases relevant to that correlation. Thus, to use Connolly's own example, given the statistical documentation of inequalities in educational outcome between majority and minority ethnic groups in England, case studies are able to reveal the differential treatment of pupils from different ethnic backgrounds which generates those inequalities.

Neither version of the argument that case study can discover causal relations is unproblematic. Waller is surely right that our experience of the world is not simply an unrelated collection of sense data. We perceive patterns, some of these are temporal, and some of them embody causal relationships. However, Waller himself recognizes that perceptions can be mistaken, and this admission immediately raises the question of how causal attributions are to be checked. His answer to this is that we need to develop insight. He comments that 'the one and only remedy for false insight is true insight'; and that 'the really great men of sociology' had no 'method', in the sense of a fixed procedure. They searched for insight: 'they went "by guess and by God", but they found out things. They strove to perceive with insight' (Waller, 1934, p. 297). It hardly needs saying that this is a very unsatisfactory answer. It leaves us with the question: How are we to judge who has true insight and who merely claims it?

The notion of *Verstehen* also raises problems. While rejecting the idea that it amounts to introspection. Waller still treats it as a psychological matter, in which identical mental processes must be stimulated in interpreter and interpreted. Yet it is difficult to see how such identification could ever be validated in particular cases, or why it is assumed to be necessary. In response to such questions, there was a shift within German hermeneutics in the late nineteenth century from seeing understanding in psychological terms to treating it as a matter of cultural interpretation (see Palmer, 1969). However, this reformulation did not narrow the scope for error in *Verstehen*-, in fact it raised the possibility of discrepant but equally valid interpretations of the same historical [p. 238 ↓] scene. These developments within hermeneutics foregrounded the question of the assumptions on which interpretations are based. After all, while it is true that - as human beings - both interpreter and interpreted will share much in common, it is also true that

they are often members of different cultures, and/or are located differently in society and history. Some writers have drawn sceptical or historicist conclusions from this argument; but, for those who do not, it highlights the need to check causal interpretations, and raises the question of how this is to be done.

Connolly's argument that case study can uncover causal mechanisms which generate correlations involves a couple of immediate problems. One is that the cases falling under the terms of any correlation will rarely be similar to one another in all relevant respects. And where these form part of a larger system, the correlation may arise from the distribution of these differences within the system, rather than from commonalities among cases. Thus, ethnic inequalities in educational outcomes can be produced by differences *among* schools as well as by what goes on *within* them (see Gomm et al. Chapter 6; Gomm et al., 1998). The second problem is that any case is descriptively inexhaustible, and any description involves cultural interpretations that are always potentially open to question. It is not simply a matter of the researcher looking to see what processes are going on in a case. All manner of processes will be occurring there, and the identification of any one of them will involve cultural interpretations about which there may be reasonable disagreement.⁴

Connolly's appeal to critical realism highlights some further issues. One is the question of whether, or in what sense, meanings can be causes; and, therefore, of what we mean by 'cause'.⁵ There is also the question of whether causal explanations rely on or imply theoretical ideas about universalistic relations among types of phenomena. Connolly seems to believe that ethnographic analysis of a single case can identify a causal relationship without the researcher being concerned with whether this relationship is found in other cases. And critical realism encourages this by treating causality in terms of powers possessed by particular agents and objects, rather than in terms of relations among categories of phenomena. While this can be a valuable perspective, it obscures the problem of how claims about such powers are to be validated.⁶

In our view, it is precisely the *general* nature of causal claims that allows us to check what caused what in a particular situation. Any explanation for events in one context necessarily treats them as an effect of a sort that is produced (under certain conditions)

by some specific *type* of cause; so that it can always be found in other contexts - at least in principle. In other words, explanations rely on assumptions about general causal relationships which cannot be validated solely through study of a single case.⁷

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Our conclusion from this is that causal attribution necessarily depends on comparative analysis. While Hume's critique was based on an implausible associationist psychology, he was surely correct that we do not literally see the causal relationship when one billiard ball hits another: we only see a sequence of two events which we take to be causally related; an assumption which could always be mistaken.⁸ Moreover, the causal relations operating in the social world are likely to be more complex than those in Hume's example: more factors are involved, and in diverse types of relationship (see Hage and Meeker, 1988). As a result, they are even less likely to be directly observable. The Humean argument is, of course, the basis for the frequently repeated insistence that correlation is not causation, and can never be entirely conclusive evidence for it. And, in our view, finding a correlation in a single case, or in a small number of cases, is usually an even less secure basis for identifying a causal relationship than finding a correlation in a larger sample of cases.⁹

Case study and comparative method

Comparative method requires that data be available from more than one case, perhaps from a substantial number, such that the effects of various candidate causal factors can be controlled or assessed. The most powerful version of comparative method is experimentation, which involves creating the cases that are required for testing a causal claim. By contrast, the case study researcher has to search for naturally occurring cases that will provide the necessary comparative leverage. We will examine two influential interpretations of comparative method that have been appealed to by case study researchers: eliminative induction and analytic induction.

Eliminative induction

As we noted earlier, in her analysis of social revolutions Skocpol employs a form of comparative historical analysis which she claims is based on Mill's methods of agreement and difference. These methods were part of a codification of what Mill referred to, following Bacon, as eliminative induction. The first of these methods involves examining cases with a view to identifying factors which always occur when a particular outcome results: it searches for necessary conditions. By contrast, the method of difference involves searching for differences between those cases that display a particular type of outcome and those which do not. Here, the goal is to identify sufficient conditions. Mill recognized that the two methods could be used together, and [p. 240 ↓] Skocpol applies both in seeking to explain successful social revolutions. The main cases she examines are the French, Russian and Chinese revolutions' In looking at these, she identifies some factors shared in common; and she compares these cases with ones in which attempts at revolution were not successful, and with revolutions that were only political rather than social, with a view to identifying significant differences.

Skocpol argues that comparative historical method of this kind is analogous to multivariate statistical analysis - that it is the appropriate method in situations where only a very small number of cases are available for investigation; though she does not provide any sustained argument for this parallel (Skocpol, 1979, pp. 35–6). At the same time, she specifically denies that the form of analysis she employs is 'purely inductive' in character; and this signals some deviation from Mill. Indeed, her primary model seems to be the actual practice of such writers as Tocqueville, Marc Bloch and (especially) Barrington Moore; none of whom explicitly modelled his work on Mill's canons.¹⁰ In effect, Skocpol appeals to the latter simply as a conveniently explicit formulation of the method for testing causal hypotheses which she believes comparative historians should use.

Skocpol's work has been subjected to methodological criticism, notably by Nichols (1986) [see also Chapter 10]. One criticism is that there is a lack of clarity about what she is seeking to explain. Is it only the success of the social revolutions she examines or their occurrence? And, if it is their success, how is 'success' being defined here?

Another criticism is that she applies Mill's methods selectively, using them to eliminate some features of the cases studied (notably, ideological factors) but not applying them to the components of the compound causal factors which make up her theory. Nichols also points to problems raised for Skocpol's analysis by the fact that there are so few cases of successful social revolution.¹¹

In order to explore the issues raised by this criticism, we need to look at Mill's conception of scientific method in more detail, so as to assess its viability as a basis for case study research. Before that, however, a little historical background is required. As already noted, to a large extent, we can find the origin of Mill's canons in the *New Organon* of Francis Bacon, published in 1620. And Bacon's position needs to be understood in the context of his rejection of the concept of science presented in Aristotle, which had dominated medieval thinking.

For Aristotle, science is knowledge of what is necessary, in the sense of what cannot be otherwise. Strictly speaking, there can be no knowledge of things that are contingent - we can only have opinions about these. Furthermore, scientific knowledge consists of a demonstrable understanding of the causes of properties (see Smith, 1995, pp. 47–9); and to demonstrate that something necessarily has a certain property [p. 241 ↓] requires a syllogistic argument from first principles that are self-evident. Aristotle believed that all sound reasoning or argument had this form; and that it paralleled the structure of reality, where consequences stem from the essential characteristics of things (see Hankinson, 1995, pp. 109–11).

For Aristotle, the process of inquiry was as follows. First, through observing particular facts we identify their common features. He seems to regard this as involving a direct apprehension of self-evident truths, rather than as a process of inference; though his account is open to different interpretations (see Smith, 1995, pp. 49–51). In the second stage of inquiry, the universals apprehended through observation are used as premises in deductive arguments, which are designed to provide causal explanations for the phenomena observed.

It is a feature of Aristotle's position, on our interpretation, then, that science operates on the basis of first principles which are non-inferential in character. At the highest level, these are general rules of reasoning, such as the law of non-contradiction, which

apply to inquiry in all fields. Other principles are specific to each science, defining the genus or domain of things it studies and the differentiation of this into various species. These definitions identify essential forms or types of substance, from which can be derived other properties that members of a specific genus or species universally and permanently have. For example, though human beings have a sense of humour, having that feature is not part of the essence or definition of a human being. Such further properties flow from the essential form of each thing. Facts about these properties are what a 'science' is meant to give causal knowledge of. Thus, it consists of syllogisms in which the flow of conclusions from premises parallels the causal processes they represent (Woolhouse, 1988, pp. 51–3).¹²

Bacon rejected several key elements of this account. First, he criticized the Aristotelian idea that scientific thinking is deductive, in other words that it moves from universal premises to universal conclusions. He argued instead that it is inductive, that sound universal conclusions can only be reached by inference from the study of particulars; though he did also recognize the importance of deducing implications from inductively established principles - both to test these and to provide for practical applications. He claimed that, in achieving their first principles, Aristotelians relied on experience of a few cases from which they then leapt prematurely to universal conclusions. And he outlined various kinds of prejudice (the 'idols') which could distort induction. What is required for rigorous inquiry, he insisted, is careful and systematic investigation of cases, employing what he calls eliminative induction, a method which relies on 'the greater force of the negative instance' (Quinton, 1980).¹³

The starting point of inquiry for Bacon (1960) is the preparation of 'a natural and experimental history, sufficient and good' (p. 130). In [p. 242 ↓] other words, all known relevant cases must be laid out, and these must then be organized into a table of presence, a table of absence and a table of degrees. The table of presence includes all those cases where the effect which is to be explained is present. In the table of absence must be listed all those cases which share features in common with the cases included in the table of presence, but where the effect is not present. And in the table of degrees cases are presented which show a correlation between variation in the feature to be explained and variation in other characteristics. These tables allow identification of features of the cases that can be eliminated as causes because they are not always

present when the effect occurs or because they *are* present when it *does not* occur. As this makes clear, the aim is to identify what is ‘always present or absent with the given nature [that is, the thing to be explained], and always increases and decreases with if (Bacon quoted in Woolhouse, 1988, p. 21). Moreover, it is worth emphasizing that, contrary to the way he is often interpreted, and to his own description of the process as ‘mechanical’. Bacon does not see eliminative induction as excluding the need for imaginative thinking about the path of causation. He does not believe that perusal of the tables always points clearly or reliably to the true cause. Indeed, he outlines various ‘supports and rectifications of induction’, some of which are similar in character to the ideas that case study researchers trade on (Woolhouse, 1988, pp. 21–3). For example, he argues that we should look for ‘shining examples’; for ‘solitary instances’ - those which display the effect but seem to have little else in common with other cases displaying it; and for ‘instances of the fingerpost’ - in other words, crucial cases that help us decide between competing interpretations.

Mill's views were similar to those of Bacon in many respects. He was concerned to combat the idea that there is some other source of knowledge than experience: whether this be intuition, reliance on mathematical idealizations or reasoning from innate ideas. Mill believed that such methods were not only false but also encouraged conservatism in morals and politics. Undermining their stronghold in mathematics and science seems to have been one of his key motives in writing *A System of Logic* (1843; Ryan 1974, Chap. 3; see also Skorupski, 1993, Chap. 2).

Mill held that since all knowledge comes from the senses, the only form of inference that leads to genuinely new knowledge is induction. Thus, he argues that science is ultimately based on the spontaneous inductions that all people engage in during the course of life - which he labels enumerative induction. It involves noticing that several particular *A*'s are *B* and concluding on this basis that all *A*'s are *B*. Often, this is done in a largely unconscious way, but it can be made more self-conscious and explicit.¹⁴ Even more important, though, according to Mill, is eliminative induction; and this is what his canons or methods [p. 243 ↓] are designed to formalize.¹⁵ This builds on enumerative induction, but is specifically designed to eliminate false explanations, and thereby to leave the investigator with the true one.

Along with his championing of inductive inference, Mill also argued against the view that causal relations involve necessity, in the sense that they could not be otherwise. As we saw, for Aristotle the structure of reality paralleled that of the syllogistic proof, and true knowledge could only be of what *must* be, not simply of what contingently *is*. For Mill, by contrast, causal relations are simply regularities to be found in the world.¹⁶ Mill's rejection of the idea that causal powers are involved, such that *A necessarily* produces *B*, is simply the other side of his rejection of deduction as a source of knowledge. From Mill's point of view, and that of many positivists, any notion of causal *necessity* is metaphysical, and must be abandoned as not open to empirical demonstration.¹⁷

Going back to Nichols' criticisms of Skocpol, these relate to some fundamental problems with Mill's canons as a basis for comparative case analysis. One concerns how we arrive at a proper formulation of the thing to be explained. For Mill this does not seem to be an issue of any significance. He apparently believes that we can produce a theory to account for *any* type of event.¹⁸ And this leads to his acceptance that there can be multiple causes of the same phenomenon. By contrast, Skocpol gives considerable attention to the formulation of what it is she is setting out to explain, engaging in a critical discussion of previous work on revolutions and similar events to argue that 'successful social revolutions' is the appropriate focus. As we shall see, the character of what is to be explained is also a key concern for advocates of analytic induction. For them, phenomena have to be categorized in ways that are open to theoretical investigation; and this means the category employed must pick out phenomena produced by the same cause, and only by that cause. Moreover, for advocates of analytic induction, by contrast with Skocpol, the appropriate categorization of what is to be explained has to be found through empirical inquiry, rather than on the basis of prior theoretical reflection.

A second problem with Mill's position is that elimination of false explanations will only result in the identification of the true explanation if all relevant features of the cases have been included in the investigation. And this is made difficult by the fact that features of cases are not simply evident to the senses. Identifying them involves a process of conceptualization. This is especially clear in Skocpol's research: it is not difficult to see that such a large and complex set of events as the French Revolution is made up of a huge number of potentially relevant features, most of which are not

(and were never) observable in any straightforward sense. Mill underplays the role of conceptualization in eliminative induction, then; presumably because he is keen to [p. 244 ↓] downgrade the significance of intuition and hypothesis in science.¹⁹ This is another area where Skocpol specifically departs from Mill, emphasizing the role of theory in conceptualizing and selecting among potential explanatory factors (Skocpol, 1979, pp. 33–42).²⁰

The third problem concerns the number of cases that needs to be investigated to reach a sound conclusion. For Mill, the task of inductive method is to produce knowledge that is demonstrably true, in the sense of knowledge whose validity is certain; analogous to the way the validity of a conclusion follows from true premises in a well-formed syllogism (see Scarre, 1998, pp. 112, 117–18). Yet, to infer the true cause of something with certainty by means of the methods of agreement and difference requires that *every* relevant case be studied. And the number of cases to which any causal claim relates is infinite; it includes all of those occurring in the past, the present and the future. Moreover, while Skocpol sees eliminative induction not as a logical procedure that guarantees sound conclusions, but rather as a means of checking the validity of theoretical interpretations, of discovering error rather than of establishing truth, the number of cases investigated is still a critical factor affecting the rigour of the test. And the need for a relatively large number of cases if false hypotheses are to be eliminated becomes even clearer once we take account of the potential causal role of absences of particular features and of combinations of features.²¹

In summary, there are problems intrinsic to eliminative induction, as formulated by Mill and to some extent as practised by Skocpol. These concern the conceptualization of what is to be explained and the selection and formulation of causal factors. In addition, contrary to what Skocpol claims, this method cannot provide convincing conclusions about causal relationships through comparison of only a handful of cases (see Lieberson, Chapter 10). While the checks she is able to exercise on the validity of her theory through comparative analysis may be better than nothing, they do not rule out all plausible alternatives. Moreover, the modifications she makes to Mill's methods weaken its capacity in this respect.

Analytic Induction

Following the wane of Mill's influence in the late nineteenth and early twentieth centuries, there were some important changes in ideas about scientific method. Some writers continued to see it as inductive, but for them induction came to be interpreted as probabilistic, under the influence of developments in logic and the mathematics of probability (see Passmore, 1966, Chap. 6). Techniques were invented for inferring the characteristics of a finite population from data about a sample drawn from it, with specified levels of confidence; and there were also [p. 245 ↓] attempts to apply probability theory to scientific induction (most notably, that of Keynes, 1973). Others, however, rejected inductivist accounts of science, in favour of the hypothetico-deductive method. Most striking here is the work of Karl Popper, who denied that there could be a 'logic of discovery' - that induction plays any role in science. For him, where theoretical ideas come from is of no significance. The only requirement is that they produce hypotheses that are falsifiable, and that these are subjected to test. While the validity of no hypothesis- and thus of no theory - can be *proven*, a single failure in the course of testing establishes its falsity.²²

In the same period, much attention came to be given to scientific method by American sociologists, in an attempt to put their work on a scientific footing. This gave rise to discussions about the relative importance of 'statistical' versus 'case study' method (see Bulmer, 1984; Hammersley, 1989). Some saw the former as providing the foundation for scientific investigation of social life, on the grounds that quantitative measurement is essential to all science.²³ By contrast, advocates of case study argued that quantitative measurement is not a necessary feature of scientific work. It is only a feature of *some* sciences: here the non-quantitative character of biology (at the time) was often contrasted with the quantitative character of physics. What *is* a defining feature of science, it was insisted, is that it produces universal laws, not mere statements of probability.²⁴ Moreover, the need for universal laws was seen as important in instrumental terms as well: on the grounds that the exception 'is the growing point of science' (see Mead, 1917, p. 221). The problem with a statistical approach, then, is not just that its conclusions are not of a scientific form, but that it

introduces 'laxity' into the research process: cumulation becomes simply the addition of further explanatory factors, rather than leading to reformulation of the original elements of the theory.

One of the most influential versions of this argument appeared in Florian Znaniecki's book *The Method of Sociology* (1934). In it he contrasted what he referred to as enumerative and analytic induction. He characterized enumerative induction as involving examination of a set of cases identified as belonging to a particular common-sense category, and the description of features which predominate among them. This amounted to treating a factor which occurred frequently in cases where there was a particular outcome as a cause of that outcome. Znaniecki sees this approach as characteristic of practical rather than of scientific thinking.²⁵ He argued that the production of probabilistic 'explanations' arose when investigations started from, and stuck to, common-sense categorizations of the phenomena to be explained - rather than seeking scientific ones that are causally homogeneous.

So, Znaniecki draws a sharp distinction between practical common-sense understanding of the world, on the one hand, and scientific [p. 246 ↓] knowledge, on the other. And he treats conceptualization and hypothesis as playing a much more significant role in scientific inquiry than did Mill. He also sees causation as involving necessity. Indeed, in this respect he goes back to Aristotle; though he specifically rejects the idea that universal categories can be identified by direct apprehension, emphasizing the need for thorough empirical study of cases (see Znaniecki, 1934, pp. 222–8). Similarly, for him, causal analysis amounts to defining the essential features of the thing to be explained: those which make it what it is. These features are determined by closed, or semi-closed, systems of social action in which the relationship between cause and effect can be formulated in deductive terms. It is an especially important feature of Znaniecki's position that, for this to be achieved, what is being explained will often have to be redefined; this is what creates the gap between common-sense and scientific concepts. Thus, Znaniecki blends elements from the eliminative induction of Bacon and Mill with an Aristotelian understanding of the nature of causation.²⁶

Alfred Lindesmith (1937) put forward a similar conception of scientific method as a basis for case study around the same time; though he does not use the term 'analytic

induction' or refer to Znaniecki.²⁷ He, too, draws a sharp distinction between his own approach and 'the statistical or multivariate method' (Lindesmith, 1968, pp. 13–14). And, like Znaniecki, he sees the logic of analytic induction as that of the experiment, and argues that 'a valid theory...must account for the basic or essential aspects of [a phenomenon] by indicating that they form a system or pattern which is logically implied or predicted by the theory' (Lindesmith, 1968, p. 9).

Znaniecki did not provide a clear demonstration of the method of analytic induction in action. By contrast, Lindesmith applied this approach in a full-scale investigation of opiate addiction (see Lindesmith, 1937, 1938; see also Lindesmith, 1968). Starting with information about a few cases of addiction, he developed a hypothesis that fitted those cases. He then collected and analysed further cases, and this forced him to reformulate the hypothesis. He continued the investigation until additional cases no longer required him to revise the hypothesis; though he notes that new data may yet stimulate further revisions to the theory in the future (see Lindesmith, 1968, pp. 7–10). Lindesmith argues that the physical effects of opiates are not a sufficient explanation for addiction - social factors must also be taken into account. The necessity of this is made clear, he suggests, by the fact that there are cases of people who have taken these drugs but have not become addicted. His conclusion is that addiction occurs only where the person concerned recognizes that the distress he or she is suffering results from withdrawal of the drug, and decides to use it again to alleviate that distress. Thus, for Lindesmith, addiction involves a person using [p. 247 ↓] the drug in order to stay normal, rather than to pursue a drug-induced euphoria.²⁸

Another major example of the application of analytic induction is the work of Cressey, whose initial focus was embezzlement. However, he found that he had to reformulate this as 'financial trust-violation', in order to eliminate cases where positions of financial trust had been taken with the intention of stealing money. In other words, the reformulation was required so as to produce a causally homogeneous category of cases to be explained. In this respect, Cressey's work is closer to what Znaniecki recommends than is Lindesmith's, with its shift from a common-sense category (embezzlement) to a scientific one (financial trust-violation) (see Cressey, 1950).²⁹

Like eliminative induction, analytic induction has also attracted some criticism, most notably that of W.S. Robinson (Chapter 8; see also Goldenberg, 1993). Robinson argued that this procedure is structured so as only to discover necessary, not sufficient, conditions for the phenomenon being explained. In other words, for the most part, the cases studied are ones where the phenomenon to be explained is found. In Mill's terms, what is applied is the method of agreement, not the method of difference. Robinson insisted that a cause must specify both necessary and sufficient conditions. He also argued that during the twentieth century it had become clear that some scientific laws are probabilistic rather than deterministic, and that this means that a statistical approach to the study of causal relations in the social world is required: large, and representative, samples of cases need to be studied.

In fact, Lindesmith had recognized the importance of looking at cases where addiction has not occurred, to check whether the causal process his theory identified was present. Thus, he cites the case of someone who had been given morphine medically and not become addicted, but who later in life became 'a confirmed addict (Lindesmith, 1938, pp. 600–3). Through this kind of comparison, he tries to show that where the features which his theory identifies are not all present addiction does not result, and that where they are all present it does. However, he comments that: 'obviously the number of instances in which a coincidence of this kind is likely to occur is very small, but those that have been found, unequivocally and without exception, indicate that if morphine is withdrawn carefully, without the patient recognizing or noticing the symptoms of abstinence, no craving for the drug develops' (Lindesmith, 1938, p. 602).³⁰ Thus, Lindesmith seems only to have investigated a small number of cases of difference, with the result that the sufficiency of his explanation has only been tested in a very limited way. Much the same is true of Cressey's work he relies primarily on reports of the prior experience of those who had later committed trust-violation.

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There is also a question about the character of the theories produced by analytic induction, and about their relationship to evidence. If it is true that the aim of analytic induction is to identify the essence of what is to be explained, in the sense of showing that effect follows necessarily from cause, is not the resulting theory deductive rather

than inductive? In other words, is it not a tautology?³¹ This seems to be the case with Lindesmith's theory. Turner comments: '[Lindesmith] has outlined the essential stages in becoming addicted by the time that he has arrived at his full definition of the phenomenon. The essential stages are implicit in the concept of addiction as he presents it (Turner, Chapter 9, p. 201). As noted earlier, for Lindesmith, addiction occurs where withdrawal symptoms are experienced, are recognized for what they are, *and where a decision is made to use the drug to eliminate these symptoms*. The problem arises particularly with the last of these three elements, which seems to threaten the distinction between cause and effect. And, indeed, Lindesmith has argued that his focus is on the *process* of addiction, within which no clear distinction can be drawn between cause and effect. Appealing to the work of Dewey, he argues that this is true of causation generally (see Lindesmith, 1981). Yet, in other places, even within the same article, he seems to retain the idea that cause and effect are independent of one another; and it is difficult to see how this could be abandoned within the context of either eliminative or analytic induction.³²

The prospects for comparative analysis in case study

We have outlined two interpretations of comparative method in case study research: that appealing to Mill's eliminative induction, and that formulated under the heading of analytic induction. We have also noted criticisms that have been made of both. It seems to us, however, that for the most part the criticisms simply point to areas in which reformulation may be required. Indeed, the two approaches have complementary strengths and weaknesses, as well as sharing much in common. Thus, in many ways analytic induction represents a useful modification of eliminative induction, as formulated by Mill, in giving a greater role to the process of conceptual thinking in formulating both what is to be explained and the factors explaining it. And, in relation to Skocpol's application of eliminative induction, the two applications of analytic induction we discussed point to the need for studying a relatively large number of cases.³³ On the other side, analytic induction requires modification, so as to provide for systematic testing of the hypothesis that has been developed: by seeking cases where the

explanatory factor is known to be present so as to check whether it [p. 249 ↓] always has the effect predicted. In other words, analytic induction needs to employ the method of difference in a systematic way as well as the method of agreement, and on a more substantial scale than Lindesmith and Cressey do. Moreover, this could be taken further than it is in Mill's account of induction, along the lines suggested by both Bacon and Popper: seeking cases that would offer the stiffest test for a causal hypothesis. These modifications would enable comparative analysis to put forward relatively strong claims about necessary and sufficient conditions.

Nevertheless, serious problems remain with case study researchers' use of comparative method. Some of these are practical ones. It should be remembered that the paradigm of the comparative method is the experiment, and the essential feature of that technique is that the cases needed for comparison can be created through the manipulation of relevant variables. While case study work may have an advantage in minimizing procedural reactivity, its corresponding weakness is that all the cases are rarely available that would allow a full test of any hypotheses generated. Furthermore, as we have seen, quite a large number of cases may be required; and, given the intensive demands of case study work, it is unusual for more than a few cases to be studied in a single investigation. Much depends here, of course, on the size of the cases concerned. Where these are relatively small, as with the work of Lindesmith and Cressey, it may be practicable to investigate a relatively large number; but where cases are large - in temporal and/or spatial terms - the number that can be investigated at any one time is likely to be highly restricted, unless a large team of researchers is involved. While this problem does not necessarily rule out use of the comparative method, it does mean that case study researchers must build on one another's work. Follow-up studies need to investigate further cases selected specifically to develop and test the theory in new ways. Unfortunately, there are currently very few examples of cumulative case study work of this kind.³⁴ And, of course, there may be some types of event, 'successful social revolutions' could be one, that are so rare that effective comparative analysis is impossible.

Besides these practical problems with comparative method, there are also some more fundamental methodological ones. One is that use of comparative method relies on an assumption about the nature of the social world that is open to reasonable question. In

fact, it is one that many qualitative researchers today explicitly reject. This is the idea that the social world is structured in terms of regularities that can be expressed as laws (see Lincoln and Guba, Chapter 2). Doubts about this have been based, in part, on the claim that social research has failed to produce convincing examples of such laws. But these doubts have also relied on philosophical arguments, for example that human beings exercise free will and that this is incompatible with social determinism.

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It is unclear whether these doubts are well founded: much depends on what is meant by laws' and on how such notions as 'free will' are interpreted. Thus, a number of writers have suggested that what case studies produce are 'retrospective' or 'fuzzy' generalizations, which capture strong possibilities rather than deterministic or even probabilistic outcomes (Scriven, 1972; Stenhouse, 1978; Bassegy, 1999). Of course, there are questions to be asked here about the kind of 'possibility' involved, and about whether this type of law differs in fundamental ways from those in the natural sciences. Either way, the dependence of comparative analysis on the existence of laws, of some kind, and the uncertainties surrounding this, need to be recognized and explored.³⁵

Closely related to the issue of whether there are social laws is the fact that use of the comparative method involves abstracting from the details, from the uniqueness, of particular cases: the focus is on those respects in which cases are exemplars of some theoretical category. This is opposed by many case study researchers on the grounds that cases must be treated as wholes, or bounded systems, if they are to be properly understood (see, for example, Stake, Chapter 1; Simons, 1996). What is less clear, though, is how this position avoids reliance on causal assumptions in understanding individual cases. And there are other problems too. Thus, Simons sees case studies as investigating the unique with a view to producing universal knowledge. She describes this as a paradox, but believes it can be transcended. In this she appeals to the model of great art and literature. But she leaves obscure the nature of this process of transcendence.³⁶ Furthermore, if her argument is correct, it is unclear why we need case study research. Do not art and literature suffice?

We believe that it is important to draw a distinction between case study work that is designed to describe the features of a particular set of cases, or to explain what occurred in those cases, on the one hand, and research that is concerned with developing and testing theories, on the other. Case study *can* be used for the latter task, but it requires a different approach from work directed towards descriptive and explanatory goals. In theoretical research, interest in cases is indeed restricted to the ways in which they exemplify the relevant theoretical category. By contrast, where the aim is description and/or explanation, the task is to document what occurred in the particular case(s) being studied, and why. This will necessarily involve much more detailed attention to the distinctive features of those cases.

It is worth adding that the task of explanation (as contrasted with that of theory development) will frequently involve tracing part of the sequence of events which eventually resulted in what is to be explained. This is a point that Becker emphasizes in his discussion of narrative explanation (see Chapter 11 and Becker, 1998). He notes that often we can only understand an outcome by tracing the path by which it came [p. 251 ↓] about; and this may involve taking account of a wide range of factors operating at different stages. However, Becker goes on to argue that such explanation is identical with analytic induction; and this links back to Lindesmith's argument that his focus was on the addiction process - that his aim was to identify how 'the craving for drugs is generated in one identifiable, unitary type of experience'. Thus, he describes this experience as 'a complex interactional process involving many elements or variables in a series of happenings or events' (Lindesmith, 1968, p. 13).

However, it seems to us that this argument is misconceived: what analytic induction produces, as with experimental research, is a conditional theory - not a description of any particular process by which some type of event occurred in a particular case. It tells us what will happen *if* certain conditions are met. We can, of course, apply this theory to explain what happens in particular cases in which we have an interest. But no single theory of this kind will usually capture all the factors that are relevant to the task of explanation. In the case of addiction, for example, we may also want to know whether some people experience withdrawal symptoms more severely than others, or whether association with other addicts increases the likelihood that the symptoms will be recognized for what they are and that the decision will be made to use the

drug to relieve these.³⁷ Moreover, what factors are relevant in an explanation is not determined by whether they form part of a single coherent theoretical system but rather by pragmatic considerations, for example whether they offer some assistance in solving a practical problem or provide a basis for assigning blame.

Another problem is that both the versions of comparative method we have discussed assume *deterministic* laws: in other words, laws which state that if *A* occurs then *B* will *always* follow, wherever certain conditions are met. This is why both eliminative and analytic induction can treat a single negative instance as disconfirming a hypothesis. Yet if laws in the social world were to be probabilistic rather than deterministic in character, as they may well be, the task facing case study researchers seeking to draw theoretical conclusions would be much more difficult. If laws are probabilistic, a negative result in one case cannot be treated as conclusive disproof; even aside from the possibility of methodological error. Instead, a relatively large sample of cases would need to be investigated in order to detect trends that reflect such laws.³⁸

Conclusion

In this chapter we have examined the arguments supporting the idea that case study research can produce causal explanations or theories.

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We looked first at those versions of this argument which appeal to the capacity of case study to uncover causal relations *in situ*. We argued that there are serious problems with the main forms of this argument. Adequate knowledge of such relations is not simply given in perception, and so causal hypotheses have to be checked. And this requires comparison across cases. In the second section, we looked at two forms of comparative analysis to which case study researchers often appeal. We discussed Mill's concept of eliminative induction, and compared it with the more recent notion of analytic induction. We argued that the latter corrected some problems in Mill's approach - notably neglect of the role of theory and hypothesis - but underplayed his method of difference. We argued that a version of comparative analysis drawing on

both could be viable; but we also pointed to a number of remaining problems. Some of these are practical in character, for example the fact that effective use of comparative analysis probably requires the investigation of a relatively large number of cases. Others are more fundamental, for example to do with whether we can reasonably assume deterministic laws of human behaviour. This issue links back to a point that we mentioned in the earlier section: whether meanings can be causes, and if they can, what sort of causes they are.

In summary, then, there are important and difficult problems still to be resolved concerning the role of case studies in producing valid theories. It is perhaps worth saying, though, that we do not see resolution of these problems as depending solely on abstract analysis of the kind we have engaged in here. To a large extent, case study research directed towards producing theory can only make progress through practical investigation of what is and is not achievable. At the same time, case study researchers need to be aware of the problems that face them in this task, and must address these.

Notes

1 Of course, some survey research is explicitly concerned with producing theories. Whether case study is directed towards a different *kind* of theory, for example one underpinned by systematic rather than genetic causation, or whether it is simply a different route to the same kind of theory, is not something about which there is agreement among case study researchers. For the distinction between genetic and systematic causation, see Cressey (1953, Introduction).

2 Both Lacey and Glaser and Strauss also use the comparative method in their attempts to identify causal relationships. Thus, Lacey justifies his selection of Hightown Grammar for study on the grounds that it controls a key variable (see Hammersley, 1985). Glaser and Strauss's concept of theoretical sampling is specifically concerned with maximizing and minimizing differences among cases in order to develop well-grounded theories.

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3 The concept of *Verstehen* also draws on a more general notion that was applied by some of the Romantics to experience of the natural world: the idea that ‘visible appearances of nature excite in us by an inherent law ideas of the invisible things on which they are dependent’ (see Anschutz, 1968, p. 148). On the influence of the romantics on Cooley, see Hammersley (1989, pp. 61–3).

4 We have pointed out elsewhere how questionable are some of the interpretations made in the research on racism in education with which Connolly is concerned (see Foster et al., 1996; Hammersley, 1998).

5 The book by Sayer which Connolly cites in support of his position includes only a brief discussion of this issue, referring the reader to Bhaskar (1989, Chap. 3) (Sayer, 1992, pp. 110–11). This is a difficult and complex issue. For different philosophical views about reasons and causes, see Anscombe (1963), Ayer (1963), Goldman (1970) and Davidson (1980). On causation in general, see Brand (1976) and Sosa and Tooley (1993).

6 This is illustrated, it seems to us, by Sayer's (1992, Chap. 7) discussion of ‘verification and falsification’.

7 It is worth noting that the reason why Waller believes that generalization can be made from a single correctly perceived case is precisely that the case is taken to be an instance of a generally occurring causal relationship.

8 Hume writes that observation gives us no impression of ‘any power or necessary connection; any quality, which binds the effect to the cause, and renders the one an infallible consequence of the other’ (quoted in Woolhouse, 1998, p. 148).

9 In a later article she also investigates the Iranian Revolution (see Skocpol, 1982).

10 On Tocqueville's use of comparison, see Smelser (1971). For a useful discussion of Bloch's advocacy of comparative method, see Sewell (1967). Sewell points out that for Bloch this method is primarily concerned with testing hypotheses not generating them (p. 217). See also Hill and Hill (1980) who suggest that Bloch drew his comparativism from historical linguistics; though he did not follow this model consistently. On

Barrington Moore, see Skocpol (1994, Chap. 1) and Skocpol and Somers (1994, pp. 79–80).

11 While Skocpol noted this problem in her initial analysis, she did not regard it as a barrier (Skocpol, 1979, pp. 33–42). And she does not change her mind about this in response to criticism (see Skocpol, 1986). For a critique of Skocpol's treatment of ideology see Sewell (1985); Skocpol (1985) is her reply.

12 It is worth noting that the surviving documents from Aristotle's scientific work do not correspond closely to the approach outlined here, which is the one presented in the *Posterior Analytics* (see Hankinson, 1995). For one thing, his scientific work includes knowledge of 'that which is for the most part' as well as of 'that which is always'; in other words, probabilistic as well as deterministic laws.

13 As Quinton points out, some twentieth-century criticism of Bacon, including that of Popper, has tended to overlook this emphasis on the negative instance (see also Urbach, 1982, 1987). Note also, though, that what Bacon was proposing here can be interpreted as a more systematic form of what Aristotle saw as the first stage of inquiry. Indeed, the methods of agreement and difference had been anticipated by philosophers working within an Aristotelian framework in medieval times (see Losee, 1972, pp. 32–4; see also Weinberg, 1965). On the whole issue of what Bacon retained from the older point of view, and where he broke with it, see Malherbe (1996).

14 Careful scrutiny of instances and methodical recording of results was also emphasized by Bacon as essential to science (see Quinton, 1980, p. 55).

15 Besides the methods of agreement and difference, Mill also identifies the method of residues, and that of concomitant variation. We will not give these any attention here: they are generally regarded as of secondary importance to the methods of agreement and difference.

16 This idea was anticipated in the work of the medieval philosophers Duns Scotus and William of Ockham, who argued that God can do anything that does not involve a logical contradiction. From this it follows that everything which happens in the world is contingent on His will (see Losee, 1972, pp. 33–4).

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17 In the late nineteenth and early twentieth centuries many positivists even rejected the concept of cause itself as metaphysical (as had Comte). Waller's article, discussed earlier, is in part a polemic against one version of this view, found in the writings of Karl Pearson (see Pearson, 1892).

18 This is not entirely true: he does recognize that there are 'capricious' phenomena, and others where regularities break down (see Skorupski, 1989, p. 174). Nevertheless, Mill gives little attention to the task of formulating what is to be explained; largely because he tends to treat this as a matter of observation.

19 Completion of *A System of Logic* seems to have been stimulated by the appearance of Whewell's *Philosophy of the Inductive Sciences*, which assigned an important role to hypothesis. On the debate between Mill and Whewell, see Strong (1955) and Scarre (1998).

20 In fact, Mill himself argued that eliminative induction could not be applied in sociology. Here he recommends instead what he calls the 'physical method'. Skocpol recognises this, though she does not discuss Mill's argument (see Skocpol and Somers, 1994, p. 88). Mill's 'physical method' involves 'concrete deduction', which he sees as characteristic of astronomy. What seems to be implied here is that sociology must be founded on *psychological* laws which will themselves have been produced through eliminative induction. The sociological task is then to use these laws to deduce explanations and predictions that take account of the compositional effects produced by the operation of multiple causes.

21 For discussions of these complexities, see Mackie (1967) and Skorupski (1989, Chap. 6).

22 See Popper (1959, 1963). We noted earlier that Abramson (1992) appeals to Popper's work to justify case study work. This appeal is rare, even though case study's role in investigating crucial cases is emphasized by some writers.

23 The inscription on the wall of the Social Sciences Research Building at the University of Chicago, often seen as a bastion of case study method, reads: 'When you cannot

measure, your knowledge is meagre and unsatisfactory - Lord Kelvin' (see Bulmer, 1984, p. 151).

24 Note that this argument was used before the widespread acceptance of quantum theory.

25 It is important to note that Znaniecki's definition of 'enumerative induction' is different from that of Mill. In particular, it implies that this form of inference is explicitly probabilistic.

26 Also in line with Aristotle, Znaniecki sees a first task in sociological work as identifying the various types or species of social action. His book *Social Actions* (1936) is devoted to this task. For a useful discussion of Znaniecki, see Bierstedt (1969).

27 Lindesmith refers to Znaniecki in later work, quoting him with approval but suggesting that the distinction between enumerative and analytic induction is an old one that is also discussed by writers on logic like Keynes (1973).

28 Lindesmith emphasizes that the literature was not studied at the beginning of the research, for fear of introducing bias, but that it was explored extensively later in pursuit of negative cases (Lindesmith, 1938, p. 2; 1968, p. 7). For criticism of his theory, see McAuliffe and Gordon (1974) and Weinberg (1997).

29 It should be noted that Lindesmith did restrict his focus to drugs which produce withdrawal distress, thereby excluding cocaine and marijuana, for example - on the grounds that a theory of addiction could not cover both categories (see Turner, Chapter 9, p. 201). Both Lindesmith (1952, p. 492) and Cressey (1953, p. 14) trace analytic induction back to Mill's methods. And, indeed, some of Mill's formulations show similarities, for example:

The process of tracing regularity in any complicated, and at first sight confused set of appearances, is necessarily tentative [...] the simplest supposition which accords with the more obvious facts, is the best to begin with; because its consequences are the most easily traced. This rude hypothesis is then rudely corrected, and the operation repeated; [p. 255 ↓] and the comparison of the consequences deducible from

the corrected hypothesis, with the observed facts, suggests still further correction, until the deductive results are at last made to tally with the phenomena. (Mill, 1974; Book III, pp. 496–7)

What is missing from this account, though, is precisely the idea that categorizations of the thing to be explained may need to be revised.

30 The kind of comparison Lindesmith uses here has the advantage of controlling some other factors: those that are permanent features of the person concerned. However, the two cases being compared - the initial administration of the drug, and the later use of the drug which resulted in addiction - are not independent: the second may have been influenced by the person's earlier experience with the drug. It is worth noting that Skocpol uses the same strategy - she compares events in Russia in 1905, as a failed revolution, with those in 1917; though she uses the method of difference in other ways as well.

31 Robinson (1952) mentions this, but does not discuss it This is a complicated issue: much depends on what is meant by 'deductive'. See Skorupski's distinction between the narrow and the broad definitions of analyticity (Skorupski, 1989, pp. 85–6).

32 The problem could be avoided in the case of Lindesmith's research by defining 'addiction' as sustained use of opiates over a long period, as distinct from the initial decision to use them in order to alleviate withdrawal symptoms.

33 Lindesmith studied over fifty cases, and Cressey over a hundred.

34 The best example we are aware of is the work of Haigreaves (1967), Lacey (1970), Ball (1981), and Abraham (1995) on the effects of differentiation of pupils in school (see Hammersley, 1985).

35 The issue of what type of law, if any, applies to human social life has been studied most effectively in the philosophy of history (see Scriven 1959; Dray 1964; Martin, 1977; see also Hammersley, 1992, Chap. 2). Social anthropology has been the discipline where the debate about comparative method has been most intense. For evidence of the strong reaction against this method on the British scene in recent years, see Holy (1987) and Ingold (1992).

36 The parallel between historical investigation and art was considered in considerable depth in the nineteenth century, for example by Dilthey (see Hodges, 1949; see also Znaniecki, 1934, pp. 195–6).

37 Of course, these other factors may form part of other theories. However, the idea that we could put all relevant theories together to produce a complete explanation is an illusion: the number of explanatory factors that could be appealed to is potentially infinite.

38 For discussion of the implications of the probabilistic character of laws, see Lieberman (1985, 1992).

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