

THE LIMITS OF SOCIAL SCIENCE

CAUSAL EXPLANATION AND VALUE RELEVANCE

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Causation and qualitative inquiry

The question of whether social science involves causal analysis, and if so what form this should take, has a complex history. There was a time when many of those committed to quantitative versions of social science denied that they were concerned with causation, insisting that their conclusions were limited to discovering correlations (see, for instance, Lundberg 1929: 13). This was particularly common among those employing non-experimental research designs: aware that they were unable to exercise the kind of physical control over variables that is intrinsic to experimental method, they cautioned strongly against confusing correlation with causation, and perhaps even saw no means of going beyond this. But, in addition, in the first half of the twentieth century there were philosophical views declaring that causation had been eliminated from modern physical science, and dismissing it as metaphysical – as involving illegitimate speculation beyond the empirical realm. For example, Bertrand Russell argued that ‘in advanced sciences such as gravitational astronomy the word “cause” never occurs’, and ‘the reason why physics has ceased to look for causes is that, in fact, there are no such things’. He famously concluded that ‘the law of causality [...] is a relic of a bygone age surviving, like the monarchy, only because it is erroneously supposed to do no harm’ (Russell 1913: 1). Along the same lines, Karl Pearson (1911: vi) called the notion of causation ‘a fetish amidst the inscrutable arcana of modern science’, insisting that scientific laws are simply patterns of perceived events that have been found to occur repeatedly, with no claim that the sequence is *necessary* or the product of some *underlying force*.¹

¹These arguments can be traced back at least as far as the scepticism of David Hume, although there is disagreement about what Hume really meant: see MacIntyre 1971: ch. 13; Owen 1999; Beebe 2006; Read and Richman 2007; Garrett 2009. For an account of the emergence of Hume’s position from previous developments, and of its subsequent influence in the philosophy of science, see Kurki 2008: ch. 1. It is worth noting that Comte, the inventor of the terms ‘positivism’ and ‘sociology’, also rejected

For both practical and theoretical reasons, then, many practitioners of quantitative method in social science, in the early twentieth century, tended to argue that there was no need to make causal claims – that predictive knowledge relying upon systematic correlational analysis was sufficient, and is perhaps all that is possible.

By contrast, at this same time influential defenders of older, and less positivistic, conceptions of social science not only opposed the idea that rigour in social science meant the adoption of quantitative techniques, but also argued that causal analysis is at the core of sociological inquiry. For instance, Florian Znaniecki (1934) rejected ‘statistical method’ precisely on the grounds that it is incapable of discovering causal relations. He insisted that these take a deterministic form, corresponding to a set of necessary and jointly sufficient conditions – not the probabilistic predictions produced by ‘enumerative induction’. He argued that what is required instead is ‘analytic induction’ through case study, which he claimed *is* capable of discovering causal laws. Others, notably Alfred Lindesmith (1947), developed this method, presenting it as starting from detailed investigation of particular cases, from which a hypothetical law can be generated, this being followed by examination of further cases to test the validity of the hypothesis. Both Znaniecki and Lindesmith treated any single case that did not match the hypothesis as refuting it, and therefore as demanding reformulation of the explanation. This process was seen as continuing until a point is reached where every new case investigated confirms the current hypothesis (see Hammersley 1989: ch. 7 and 8; Hammersley and Cooper 2012).

Another influential voice in the first half of the twentieth century defending qualitative case study against criticism by quantitative methodologists was Robert MacIver (1942). He too argued that the notion of cause is unavoidable in understanding the world, including social phenomena – that what is required for sociological explanation necessarily goes beyond correlation, the observation of ‘regular sequences’, to refer to productive forces of one sort or another. And, rather than seeing separate factors as each increasing the likelihood or intensity of an outcome to a calculable degree, he insisted that it is necessary to gain a clear understanding of how factors are *interrelated* to form the causal processes involved in systems of social relations.

So, Znaniecki, Lindesmith, and MacIver all rejected the nominalistic retreat into regarding knowledge of correlations as sufficient; though whereas Znaniecki and

the notion of causation, even while insisting on the role of laws in scientific explanation (Thompson 1976: 43; see also Turner 2003). On this whole line of philosophical thinking, see MacIver 1942: ch. 1 and 2; and for a more recent account, see Cartwright 2002, 2007. The nature of causation has long been a matter of dispute in philosophy: see Beauchamp 1974; Mackie 1974; Brand 1976; Mohr 1996; Beebe et al. 2009. Some years ago, Tooley (1987: 5) wrote: ‘Many accounts of the nature of causation and of laws have been advanced, but none has elicited anything remotely approaching general acceptance’. This remains true.

Lindesmith accepted the traditional scientific conception of causality as involving universal laws, MacIver rejected it. He writes:

a functional equation is an admirable device to symbolise certain highly general or universal relationships under hypothetical conditions, where, for example, a number of determinate factors or forces are assumed to constitute the structure of a closed system in a state of equilibrium. [However, it] has no relevance to a system that cannot be understood in terms of isolable factors or components. It has no application to a system the changes of which depend in any degree on the impact of factors lying outside it. (MacIver 1942: 52)

Moreover, all these writers believed that causation in the social realm is distinctive as a result of the role of subjective factors, such as perceptions, intentions and motives.

Today, these contrasting positions on the part of quantitative and qualitative researchers, *for* and *against* causal analysis (albeit interpreted in varying ways), have been largely reversed. Quantitative researchers now routinely make explicit causal claims, even while still warning (quite properly) against confusing correlation with causation; and methodologists focusing on such work have given much attention to the nature of causal analysis (see James et al. 1982; Hellevik 1984; Hage and Meeker 1988).² By contrast, many qualitative researchers today expressly *deny* the possibility of causal analysis in the social field. Like Znaniecki and Lindesmith, they treat the concept of cause as implying the existence of empirical laws, but *unlike* them they insist that no such laws have been discovered in the social realm and that none exist. They argue that social processes are contingent in character, in the sense that they are heavily dependent upon the agency of actors: that is, on their interpretations, intentions and decisions, *these being highly variable and context-sensitive*.³

Qualitative researchers also often treat the idea of social causation as *ethically suspect* and *politically undesirable*. We can get a sense of this from Lincoln and Guba's (1985: 129) statement that: 'if it could be shown that the concept of

²Hoover (2004) charts a somewhat similar pattern in economics, with the language of cause disappearing from econometrics, but then starting to reappear at the end of the twentieth century. This has been bolstered by some developments in philosophy and statistical theory, designed to provide rigorous means of causal inference from non-experimental data (see, for example, Pearl 2000; but see also McKim and Turner 1997).

³See Maxwell (2012: 35) for an outline of these views. There are exceptions to this generalization about the position taken by qualitative researchers today, Maxwell himself being one (see Maxwell 2004a, 2004b, 2012). The situation is rather different in the field of political science. Here a concern with the demands of causal analysis has been central to the development of Qualitative Comparative Analysis (Ragin 2008) and to the defence of case study work more generally (George and Bennett 2005: ch. 7 and *passim*; Bennett and Elman 2006; Gerring 2006). See also Mahoney and Goertz 2006; Goertz and Mahoney 2012.

cause is deficient, the assumption of determinism would be difficult to defend'. It is often argued by qualitative researchers that to adopt a deterministic position dehumanizes human beings, portraying them as – indeed, perhaps inducing them to act as if they were – automatons. A further criticism is that to treat human behaviour as causally determined serves to justify existing social arrangements and to deny the possibility of planned social change. More generally, Lincoln and Guba (1985: 129) comment that 'there are other reasons for [the] causality fetish' besides a positivistic conception of science: 'If causes are the key to prediction and control, knowledge of causes is tantamount to power. This fact may help us to understand why political figures have been willing to support scientific research, in the hope that such inquiry will produce information that can be used to good political effect'. Where Znaniecki, and others, had seen the prospect of social control based upon scientific knowledge as beneficial, these authors, and many qualitative researchers today, see it as oppressive.⁴

In denying that causal analysis can be applied to the social world, qualitative researchers have drawn, explicitly or implicitly, to varying degrees and in various ways, upon several influential philosophical traditions – notably, Kantian and Hegelian idealism, Husserlian phenomenology, and ordinary language philosophy. In the next section I will outline some key arguments they have derived from these sources.

Some philosophical influences on qualitative inquiry

Over the past 50 years diverse philosophical views have shaped qualitative research, and these are partly responsible for generating a disparate array of approaches within it. Here, I will focus on just three broad philosophical traditions that have had a significant effect on how qualitative researchers have set about describing and explaining social phenomena.

To a large extent, the idealist tradition had its origins in Kant's insistence that causality cannot be treated as present *in* the world; that it is, instead, a constitutive principle *through which we understand* that world. Also influential was his argument that it is intrinsic to the very notion of rationality that we have the freedom to act one way rather than another – for example, to treat people as ends not merely as means. In large part, Kant's philosophy was motivated by a commitment to protect the realm of morality from the materialism and naturalism associated with the growth of physical science (Velkley 1989). Hegel subsequently transformed these ideas by abandoning Kant's distinction between noumenal and phenomenal realms (between things-in-themselves and how they appear to us), with the result that from his point of view the world itself is characterized by the sorts of relation typical of thought, rather than those that materialists believe operate among material bodies. He argued that even physical causation is a species

⁴In my view, neither position is sensible

of 'logical' relation within a world conceptualized as an evolving whole with its own internal 'dialectic'. Moreover, he saw the task of identifying these relations as depending upon *systematic* interpretation: 'locating' phenomena within the theoretical context of a historical meta-narrative that explains them. Here, theory is treated as an essential means of conceptualizing phenomena, often in a manner that goes beyond, and reveals the distortion inherent in, empirical appearances. This idea was spread within social science particularly through the influence of Marxism and Critical Theory, but it has extended even beyond those who see themselves as belonging to these traditions.⁵

Another important influence on qualitative researchers, from the 1960s onwards, was Husserl's phenomenology, especially through the work of subsequent philosophers influenced by him, such as Heidegger, Sartre, Merleau-Ponty, and Schutz. Husserl defined the philosophical task as rigorous *description* of 'the things themselves', as they appear in our experience, and also of our modes of experiencing them.⁶ What are discovered through such investigations are relations operating *within* subjectivity; this time (by contrast with Hegel) a subjectivity that is universal rather than historically developing, and individual (albeit transcendental) rather than collective. Some qualitative researchers have taken over these ideas (often modifying them considerably in the process), for instance seeking to *describe* people's perspectives, patterns of social interaction, etc., *as these appear in the world*; in other words, seeking to portray these phenomena *in their own terms* rather than from some broader perspective in the way that both positivist and Hegelian/Marxist science do.

Interestingly, both the idealist and the phenomenological traditions generated more or less the same problem in the course of their development, as regards their support for non-causal interpretations of human social life. This was that the distinction originally drawn between internal and external relations came to be challenged or undermined. In the case of idealism, once Marx had turned Hegel's dialectic back on to its feet (Marx 1873/1970: 20), the focus of inquiry became social relations in a material world, rather than within a developing 'world spirit'; and it is hard not to see Marx's account of capitalism as attributing causal relations (see Ruben, 1979: 118–26). In the case of Husserl, once his followers became primarily interested in *being-in-the-world* rather than in the transcendental core of individual subjectivity, the same problem arose. It came to the surface, for example, in the disagreement between Sartre and Merleau-Ponty as to whether human actions are simply a matter of free will, any denial of this being treated as bad faith, or are always necessarily biologically, historically, and socially located, and therefore caught up within some kind of causal nexus (see Gutting, 2001: 203–8). Sartre effectively conceded this point in his later work.

⁵As an illustration of the terminological complexities of the philosophical field, it is striking that Pearson, the positivistic advocate of the 'grammar of science', labelled his own position a form of idealism (Pearson 1900: vii). What he meant was that he was committed to phenomenism, like some other positivists at the time, such as Mach.

⁶Like Hegel, Husserl abandoned Kant's distinction between noumena and phenomena.

The third, and final, influence I will mention is the sort of ‘descriptive metaphysics’ (Strawson 1959) that was the concern of much ordinary language philosophy in England in the 1950s and 1960s (see Anscombe 1957; Peters 1958; Melden 1961). Here the focus was on how concepts, including those relating to human action such as ‘reason’ and ‘cause’, are used in ordinary language-use, the assumption being that studying this allows us to understand their meaning, and how their misuse can confuse us. This involved resistance to attempts to treat everyday meanings as the defective realization of some single, underlying formal logic; or to regard them as imperfect reflections of the world that they are supposed to conceptualize, or as mere epiphenomena – whether deriving from brain functioning or from social processes (such as the mode of economic production). In the very nature of this kind of conceptual analysis, reasons must be seen as internally related to the behaviour for which they are held to account, so that one cannot treat the two as separate phenomena, even less as exhibiting a *general* relationship – assumptions that are normally treated as essential components of any notion of causality. Furthermore, it came to be argued that the giving of reasons must itself be viewed as a form of action, for example, one that is directed towards the tasks of justification or excusal, and this means that the statements produced must be seen as forming part of this action, so that reasons must become context-sensitive and contextually variable linguistic products, rather than motivating factors that exist inside people’s heads.

Qualitative approaches to explaining social action

All of these philosophical ideas tended to lead to a denial that human actions and institutions are produced by causal processes, in favour of conceptualizations portraying them instead as constituted by relations of an ‘internal’ – intelligible or meaningful – character. And it was also usually argued that these must be grasped through forms of understanding that are quite different from the methods used by natural scientists. At the same time, these ideas generated a diversity of orientations among qualitative researchers, rather than a single one. I will outline three broad common tendencies that illustrate some of the main lines of variation.⁷

Intentional explanation. Some qualitative researchers take people’s accounts of their intentions and motives largely at face value, and treat these as playing the key role in generating social phenomena. This is characteristic of some ‘biographical’ approaches, though it is not limited to these.⁸ Underpinning this sort of approach, often, is the idea that we cannot assume that the environment that actors experience

⁷In practice these are sometimes combined in various ways.

⁸For outlines of biographical approaches, see Bertaux 1981; Atkinson 1998; Roberts 2001; Bornat 2008.

corresponds to what is specified 'objectively' by social scientific analysis: there may be discrepancies not just in terms of what is salient but even in the very character of what is perceived and how it is interpreted. Moreover, participants' understandings of their surroundings, and indeed of themselves and their own actions, may change over time, rather than being fixed, stable, or standard. Thus, it is argued that – in any attempt to explain people's actions – the first task is to document their own understanding of themselves, of their actions, and of their world, rather than assuming that the motivational process corresponds to some pre-given and standard sociological model identifying objective causal factors. Moreover, often, description of these understandings is judged to be sufficient in itself for the task of explaining actions.

More than this, it is often argued that there is a problem concerning how the sort of 'objective' factors that are prioritized in many social scientific accounts (whether appealing to societal values and norms, interests arising from social divisions, or whatever) can be related to the perceptions and understandings of participants, in such a way as to show how these factors structure their behaviour. Some qualitative researchers have concluded that 'objective' and 'subjective' perspectives represent different, incommensurable 'realities'; or, indeed, that there is no such thing as objectivity, simply multiple subjective perspectives. One formulation of this is the idea that people are 'experts in their own lives', which has become a common position in the field of Childhood Studies (see, for example, Christensen and James 2008). This is often underpinned by an ethical commitment to the importance of *respecting* people's understandings of themselves and their world, rather than claiming that science can produce a superior account, one which identifies forces shaping people's behaviour, of which they are unaware, or assigns motives to them that they would disavow.

Explanation in terms of mutual shaping. A second, rather different, sort of approach to explaining social behaviour that can be found among qualitative researchers is illustrated by the arguments of Lincoln and Guba (1985). In place of the notion of cause, whether interpreted as referring to a set of necessary and jointly sufficient conditions, or in probabilistic terms to the presence of some factor that increases the likelihood or intensity of a particular type of outcome, they argue that the focus should be on systematic relations among a large number of factors that 'simultaneously mutually shape' one another.⁹ It is argued that these relations are not deterministic but contingent, and are 'circumstances-relative' (1985: 152). As a result, any knowledge we have of them can only amount to making '*plausible imputations*'. Moreover, which elements we include in our focus will depend upon our purposes, not only upon the structure of relations among the elements within the causal nexus. Lincoln and Guba (1985: 152) write that 'Understanding results

⁹At one point these authors suggest that there is 'an infinite number of mutually interacting shapers' (Lincoln and Guba 1985: 156).

from an appreciation of the myriad mutual shapings that are synchronously ongoing, and abstracting from that complexity a subsystem that serves the investigator's needs'. This understanding is 'shaped in equal proportion by the investigator's purpose and the phenomenon's presentational aspect'.¹⁰

Lincoln and Guba's account of causality differs from the first approach, to some degree, in the emphasis it gives to understanding the social relations in which people are involved, as a prerequisite for describing and explaining their behaviour. But the most important difference is that there is no suggestion that we must analyse these relations purely from *within* participants' perspectives. In this respect, the approach is analogous to older arguments in favour of pattern models of explanation (Diesing 1972; Williams 1976) and to more recent reliance upon approaches like actor network theory and activity theory (Engeström et al. 1999; Law and Hassard 1999).

Reasons as rhetorical. The final stance I will discuss is very different from the other two. This is the idea that it is misleading to think of motives, or for that matter of other potential causes, such as interests or value-commitments, as forces operating *on* or *within* individuals that *drive* their behaviour. Rather, these phenomena are linguistic or discursive in character, and are socio-culturally generated and deployed in order to make sense of and act in the world (Mills 1940; Blum and McHugh 1971). To apply a particular label to an event – for example, to describe a death as a murder, rather than as an accident – is to mobilize a whole set of other categories that must be filled, as far as possible, with relevant features from the situation concerned: there must be a murderer, a situation in which this could occur, a weapon or other means by which the victim was killed, and a motive. The label also potentially stimulates a series of responses – shock, outrage, fear, calling the police, mounting a search, questioning those involved, and so on. Rather than assuming that a label, such as 'murder', simply reflects reality and has consequences as a result of this, we can notice that the mobilizing effect of the labelling occurs whether or not it comes subsequently to be treated as true or false. Given this, it is suggested that the focus of analysis should be on how people attribute perceptions, intentions, motives, etc., both to themselves and to others. It is argued that research must examine how people formulate motives and other purported causes of behaviour – how these are attributed, challenged, etc., in processes of social interaction or within texts of various kinds; in other words, emphasis is shifted to the 'discursive construction of social reality'.

In some early versions, this approach incorporated causal explanation: 'vocabularies of motive' were treated as directing material causes down one line of action rather than another. An example is Sykes and Matza's argument that ideas about blame and responsibility serve as 'techniques of neutralization', allowing people to engage in criminal behaviour that they would otherwise have rejected as

¹⁰For a somewhat similar epistemological position, see Eisner 1992.

morally proscribed (Sykes and Matza 1957; Matza 1964; see also Cressey 1953). However, more recently, there has been a strong tendency to abandon this causal component, in favour of a more radically constructionist position.¹¹

An assessment

While all three of these approaches distance themselves from causal analysis, if we look more closely at what they entail we find that they are by no means entirely successful in avoiding it. The first two approaches still retain some notion of cause, if we interpret this in a broad sense as referring to factors (including perceptions, intentions, motives, social relations, etc.) that generate one kind of action rather than another, so that what results is not simply a matter of coincidence (see Mohr 1996: 13; Kurki 2008: ch. 6). Only the third approach, and then only in its radical constructionist form, appears to abandon the notion of causation completely. Moreover, even here, as we shall see, there are questions about whether, *in practice*, causal claims are completely escaped: they may still arise, for example, in seeking to explain why one vocabulary of motives, or one discursive strategy, is adopted, and/or in documenting the *effects* of actors' adopting these.

So, despite frequent disavowals, much qualitative research *is*, in effect, still concerned with putting forward causal explanations for why or how something occurred, or with offering accounts of the consequences of some event, intervention, etc. At the same time, perhaps because there is not usually an explicit commitment to or focus on causal analysis, none of the approaches outlined provides an entirely satisfactory basis for understanding what this involves, or for producing sound causal accounts. I will look again at each approach in turn.¹²

¹¹A key source for this whole approach is the work of Kenneth Burke, see Overington 1977. See also Mills 1940. While for Burke and Mills there was no question that there are phenomena existing independently of particular discursive acts, so that labels could turn out to be false, later exponents of this set of ideas often seemed to abandon this assumption. See Bruce and Wallis 1983 for a critique of this approach. Sharrock and Watson 1984 respond, and Bruce and Wallis 1985 reply. See also Housley and Fitzgerald 2008. For other versions of this argument, in a different field, see Laffey and Weldes 1997. It is striking to compare this attitude towards motive with that of MacIver, for whom motives are genuine causal factors, albeit ones about which it is difficult to provide conclusive evidence (see MacIver 1940). There have recently been counter-trends to the discursive turn in the form of psycho-social approaches to the study of human behaviour. For a response to these developments from within a constructionist perspective, see Wetherell 2008.

¹²There have been a number of calls for qualitative researchers to engage explicitly in causal analysis, on the argument that quantitative research has been founded upon a defective, Humean conception of causation which reduces it to observable regularities (see, for example, Kurki 2008). While there is some truth in this diagnosis, in my view it is often based on too sharp a distinction between regularity theories and other ways of understanding causation.

The first one treats people as having direct knowledge of what they are doing and why. And it emphasizes the importance of our grasping of how they see the world, and themselves, if we are to be able to understand their intentions and motives. Up to a point, there is much to be said for this. Those approaches that deny self-knowledge – whether behaviourism, some form of structuralist sociology, or discursive psychology – fly in the face of what it is surely reasonable to accept as true: if we were to doubt that we can have self-knowledge of some sorts, we would inevitably be pitched into complete epistemological scepticism, since there are few other sorts of knowledge that we treat as generally more reliable.¹³ That said, this does not mean that people's accounts of their intentions, motives, perceptions, etc. should be accepted at face value, or treated as always sufficient for explaining what they do and its consequences. We are all aware that we can be mistaken about our own behaviour, even about what we actually did, and certainly about our motives – such doubts are not an invention of depth-psychology. Similarly, we may not understand much about how the situations that prompted our actions were generated, yet this may be relevant both to explaining those actions and (even more obviously) to accounting for their consequences. Here, too, we can see that there is scope for research to produce more adequate explanations (in some sense) than those which we ordinarily produce as actors.

Turning to Lincoln and Guba's account of causality, this clearly has the advantage of emphasizing the importance of understanding the social relations in which people are involved, and in a way that is not restricted to viewing these exclusively from *within* participants' own perspectives. It is also surely true that patterns of social action are shaped by a wide range of factors, rather than being under the determinate control of one or two. Also, these authors' acknowledgement of the role of the analyst's purpose and perspective in selecting causal factors is to be welcomed. However, Lincoln and Guba do not provide much guidance about how we should go about generating and assessing hypotheses about the causes or effects of particular patterns of social relationship. They themselves write that 'there is a great difference between finding a statement persuasive and being able to say *why* it is persuasive. The development of appropriate criteria represents an intellectual task that is, at the moment, beyond us, but that, we are persuaded, will turn out to be doable' (Lincoln and Guba 1985: 157). Moreover, while they recognize the role of the analyst in selecting from a very wide range of social relations, they do not give much indication as to how this ought to be done.¹⁴

¹³The form of argument I am using here, about certainty, is to be found in Wittgenstein 1969. Discursive psychologists appeal to other parts of Wittgenstein's work (particularly to his *Philosophical Investigations* (1953)) to justify their position (see, for instance, Edwards 1997).

¹⁴In Chapter 2, I outline Weber's notion of value relevance which, it seems to me, provides a sound understanding of the proper basis for the selectivity necessarily involved in analysing causal relationships. See also Mackie 1974 and Roberts 1996.

As regards the third approach, in its constructionist form it seeks radically to detach itself from the whole business of causal analysis, in favour of a focus on relations that are internal to particular discourses or modes of discursive practice.¹⁵ However, generally speaking, it fails to make good its escape. Take, for example, Potter's (1996) version of discourse analysis, and specifically his concept of 'stake-inoculation'. This points to the way that people often preface a judgment they are putting forward by indicating why they have no stake in its being true. An example would be when someone says: 'I'm a member of the Conservative Party, but I can't deny that the gap between rich and poor in the UK today has become too great'. Built into Potter's account here are implicit causal claims: it is assumed that this strategy of stake inoculation is adopted because it generally has the effect of disarming one sort of countermove that audiences may make to dismiss what the speaker is about to say (in the example above, accusations such as 'you must be anti-capitalist'). In other words, the concept presupposes that actors believe that use of this strategy will have this disabling effect (that *they* assume a causal relationship). Moreover, besides the analyst attributing this causal assumption to actors, he or she also assumes that it is precisely *because* of this belief that actors use stake-inoculation strategies: that there is a causal relationship between belief and use here.¹⁶ In addition, it seems to be taken for granted that the effect of stake-inoculation will actually be, much of the time, to close off, or at least to make interactionally difficult, the moves anticipated, and that this is not happenstance but is to do with its *systematic effect* on hearers.

As should be clear then, in practice, albeit usually implicitly, qualitative researchers almost always rely in their descriptions and explanations upon the idea that causal relations operate in the social world. Indeed, very often they are concerned, for instance, with the ways in which various objective factors – such as social status or class, sex or gender, ethnicity, and so on – shape people's circumstances, attitudes, and behaviour. Given this, it is not surprising to find the frequent use of verb forms that imply causal relations – such as 'influence', 'shape', 'leads to', or 'results in'. And, even though qualitative researchers generally avoid terms that imply strong causality, such as 'determine', these are nevertheless occasionally employed (Hammersley 2008c). In fact, escaping from causal analysis would require a major re-specification of the task of social science, for example along the lines of ethnomethodology (Button 1991), and even this may not be successful in avoiding it.

¹⁵In its less constructionist form it offers a model of one important sort of causal process, one that is similar to that found in Weber's arguments about the role of religion in facilitating the rise of capitalism (Weber 1948: 280).

¹⁶Potter and his colleagues would almost certainly deny that they attribute any such beliefs to the people they study, but in my view these minimal views about agency cannot be avoided in the sort of analysis in which they engage: see Hammersley and Treseder 2007: 286–8.

How are we to explain the fact that, while qualitative researchers today frequently deny that causation operates in the social world, they nevertheless engage in causal analysis? One explanation is that they cannot answer the sorts of question they address without doing so. Another is that what they are rejecting is not causal analysis *per se* but, rather, particular interpretations of it, notably those that demand the specification of a set of necessary and jointly sufficient conditions, and/or those that treat documenting causal relations as necessitating the use of statistical analysis.

Furthermore, it is perhaps because they usually do not explicitly recognize that they are engaged in causal analysis that qualitative researchers rarely deploy systematically the full range of strategies required to generate, and especially to test, causal interpretations. They often rely upon just one of these strategies and may not even use this in a very systematic fashion. In effect, many of them seem to assume, much of the time, that identifying causes is relatively straightforward: that conclusions about these can be simply drawn from what people report about their intentions or motives, or that they can be directly observed at work in interactional processes, that they can be read off from the identification of discursive practices, and/or that currently influential theories will supply privileged insight into them (see Hammersley 2012b).

In the next section I want to examine, in broad terms, the resources that are available to qualitative researchers, and to social scientists more generally, for identifying causal relationships and thereby for providing sound explanations of social phenomena.

Strategies for causal analysis

While the aim of causal analysis is to document real world processes, we have no *direct* access to these via perception.¹⁷ Nor can we derive knowledge of them from empirical evidence *in a strictly logical or calculative fashion*.¹⁸ Instead, we have to rely upon two sorts of resource:

¹⁷Maxwell (2012: 38) and others (see Connelly 1998) have argued that it is possible to *observe* causal relations. There are undoubtedly occasions when the perceptual evidence is so strong as to be beyond reasonable doubt. One of the examples that Maxwell uses, appealing to Scriven 2008, is that of a hawk taking a pigeon. However, in my view this is not a useful paradigm for most of the causal relationships with which social scientists are concerned, where the perceptual evidence can only support much less certain lines of inference. Also treacherous here, of course, is any assumption that what is perceived is simply given and therefore automatically true. The existence of optical illusions warns against this. Furthermore, see Mohr (1996: 43) and Strawson (1985) on the argument that it is actually physical feelings rather than vision that is basic for understanding causation.

¹⁸Some use of statistical analysis seems to operate as if this were possible, and the same may be true of the ways in which Grounded Theorizing and Qualitative Comparative Analysis are sometimes employed.

- *Imaginative construction of plausible models* of relevant causal processes that could be involved in the situation(s) we are studying. This will draw upon past experience of other situations felt to be similar in significant respects, on analogies and metaphors that appear to offer illumination, and on thought experiments concerned with assessing whether the outcome would have been different if some element of the situation had varied. These imaginative processes may be stimulated by reading appropriate literature, even including fictional material.
- Equally important is the *analysis of empirical data* about relevant cases. This is essential for both the generation and the development of explanatory ideas (abduction or retroduction), and for testing their likely validity.¹⁹

It is very important to emphasize that the imaginative and empirical aspects of causal analysis are intimately interrelated: indeed they are complementary. Empirical analysis feeds the process of imaginative construction of causal models, and also serves as a corrective to it, but cannot on its own identify causes. If the two become separated, or if one is given priority, causal analysis is likely to be less successful. Excessive emphasis upon imaginative construction of causal models leads to speculative accounts that may bear little relationship to what happens in the world. On the other hand, excessive stress on empirical analysis tends to result in superficial accounts that do not capture the causal processes involved: it must be remembered that empirical data can only serve as *signs* of real-world processes – these must be *interpreted*, and it is necessary to make this a deliberate and thoughtful process.²⁰

There are two broad forms of empirical analysis available to qualitative researchers, and indeed to social scientists more generally, in seeking to identify causal relations: within-case and cross-case analysis. It is worth noting that these are strategies we all use to a degree in everyday life, where we are inevitably often concerned with identifying causal relations. And qualitative researchers have long relied upon them – albeit, as already noted, often only partially and not always very systematically. These two strategies involve the following:

¹⁹The terms ‘abduction’ and ‘retroduction’ appear to have been introduced by Peirce, see Fann 1970. However, they have come to be used in a variety of ways that depart from his usage. In particular, whereas Peirce treated abduction/retroduction as just one of three types of reasoning required in science, it is now often presented as if it were self-sufficient: see, for example, Coffey and Atkinson 1996: 155–6. Blaikie (2000) treats these terms as referring to different research strategies, but once again they are viewed as self-sufficient.

²⁰I am not claiming any novelty for the view presented here. Something like it can even be found in Comte (Thompson 1976). However, it does seem necessary at the present time to reiterate what is involved, since in my view there are many examples in social science today both of excessively speculative accounts and of ones that effectively treat data as speaking for themselves.

- *Within-case analysis* takes the form of detailed investigation of features and processes within a particular case, over some period of time (short or long), examining patterns of co-occurrence, of co-variation, and/or of sequence, relevant to putative causal processes. Generally speaking, it involves examining cases where the outcome that is of interest actually occurred, to look at what preceded it or what is otherwise associated with it, and to document participants' understandings of themselves, their situation, and their behaviour that might be relevant to any explanation. The aim is to use the data to think about what processes could generate the outcome, and also to test out ideas about this.
- *Cross-case analysis* involves systematic comparison of several cases, perhaps a large number, looking for patterns of co-occurrence or co-variation between outcome or effect variables and potential causes. All cases within a population, or some sample designed to be representative of it, may be studied, or cases may be selected for investigation strategically; for example, focusing upon those where candidate factors, or combinations of factors, are present and absent, or are at different levels of intensity, and where the effects of confounding factors can be discounted. Researchers might also investigate cases where what they suspect is the key factor is present to find out whether the outcome also occurred in those cases. The patterns discovered through comparing cases can suggest what causes what, and will also enable us to *test* our ideas about this to some degree.

As my discussion indicates, each of these empirical strategies can be used to produce data that will both generate explanatory ideas and test the validity of hypothetical explanations of what happened in particular cases. Moreover, usually neither can provide conclusive evidence on its own:

- Given that it is not usually possible simply to observe causal relations, within-case analysis can only offer suggestions, stronger or weaker, that a particular type of causal relationship is operating, or rule out some otherwise plausible hypothesis.
- Similarly, cross-case analysis alone can rarely provide entirely convincing evidence about causal relationships, in part because it is not usually possible to devise comparisons that enable all of the potentially relevant factors to be taken into account.²¹

Even when these two strategies are used together, they will not produce absolutely conclusive evidence, but they may well provide the resources for generating convincing explanations. To maximize the chances of this, they need to be used in a systematic way.

As regards within-case analysis, this requires seeking to document all relevant internal processes of a case in sufficient detail, and to check inferences about the potential causal processes involved against information from the case. Where cases are large and internally differentiated, considerable work may be required to gather all of the relevant information. With cross-case analysis, there must, once again, be an effort to ensure, as far as possible, that all the relevant information from each case has been collected, and that this is reliable; though, generally speaking, less information will be collected about each case and there will be less chance to check its

²¹This is what Ragin 2008 refers to as the problem of 'limited diversity'. See also Ragin and Sonnett 2004.

reliability, as compared with what is possible in within-case analysis (Hammersley 1992: ch. 11). In addition, there need to be systematic efforts to find and use data from whatever relevant comparative cases are available. To the extent that it is feasible, comparative cases must be studied in such a way as to identify *complexes* of factors that operate together, and to rule out competing hypotheses (see Ragin 1987). Moreover, as part of this it is necessary to identify the different functions that causal factors can play, for example, as background facilitator or as trigger (see Roberts 1996). Finally, in the use of both strategies there needs to be clarity about the limitations of the data collected, and of the inferences that can be drawn, as regards the likely validity of the answers being proposed to research questions.

A great deal of qualitative research relies primarily, if not exclusively, upon within-case analysis. Furthermore, while there is often imaginative use of data to produce likely explanations, there is frequently a low emphasis upon systematically testing these candidate accounts. As noted earlier, this perhaps arises from underestimating the difficulties involved in reliably drawing conclusions about causal relationships. It also probably stems from the fact that qualitative studies often focus on a wide range of *descriptive and explanatory* issues simultaneously, for instance with the aim of providing a rounded account of the case(s) being investigated (Hammersley 1992). There is considerable tension between the breadth of focus adopted and the depth of the analysis that can be carried out.²²

Qualitative researchers do also sometimes use cross-case analysis, comparing cases that are similar and different in relevant respects. Indeed, this is built into Analytic Induction, Grounded Theorizing, and Qualitative Comparative Method. However, not only is appeal to these methods – especially in the case of Grounded Theorizing – much more frequent than systematic deployment of them, but also each method has weaknesses as a form of comparative analysis (Cooper et al. 2012). In my view, there is particular scope for improvement in the use of cross-case analysis within qualitative studies, though the challenges this entails, not least in logistical terms, should not be underestimated.

Conclusion

In this chapter I began by noting that there had been a shift over time in the attitudes of qualitative researchers towards causal analysis: from explicit commitment on the part of many in the first half of the twentieth century to rejection by most qualitative researchers in most fields today. I looked at key philosophical ideas that had influenced this change, and at some common current approaches

²²It should be noted, however, that the trade-off here is no simple matter, once we recognize that causal analysis requires understanding how multiple factors may work *together* in producing an outcome, and acknowledge the importance, for grasping how this process operates, of the contexts in which it takes place.

to the tasks of description and explanation. At the same time, I suggested that, in practice, most qualitative researchers still make causal claims, but that their hypotheses are often not developed and tested very effectively.

I then outlined the two main resources for use in causal investigation: imaginative construction of causal narratives and the analysis of empirical data. I also discussed the two empirical strategies that can be used to discover causal processes – within-case and cross-case analysis – stressing that these are both essential and complementary. While use of these resources does not guarantee success, they are all that is available to any social researcher. And how well they are executed can increase or decrease their effectiveness significantly.

Qualitative case study has both strengths and weaknesses in pursuing causal analysis, as compared with more quantitative approaches. It can provide detailed understanding of what goes on within cases that may enable productive explanatory hypotheses to be developed and tested. And, in cross-case analysis, it may allow strategic selection of cases to facilitate both the development and assessment of causal explanations. However, complementarity operates at this level too: combining qualitative work with quantitative analysis, both within and across cases, can improve the chances of reaching sound causal conclusions.

In my judgment, there is some way to go before social science can realize its potential in this respect, and there is too much complacency on the part of many about what is currently being achieved. This has been a persistent problem. Writing in 1942, MacIver reported:

The writer made a survey of the articles dealing with [a variety of] subjects as published over a period of years in journals of sociology, psychology, economics, political science, and education. In the great majority of instances either no grounds or quite inadequate ones were given in support of the causal imputations they presented. Sometimes there was displayed a meticulous care in the refinement of statistical indices or in the calculation of correlation coefficients, followed by a sweeping, unguarded, or wholly unwarranted conclusion regarding the causal nexus. Sometimes a selective description of conditions attendant on the phenomenon was the only basis for quite definite imputation. Sometimes cases or examples were offered showing the presence of the alleged cause, as though that were sufficient to establish its causal relation to the social phenomenon. Not infrequently an order of priority or importance was assigned to a number of 'causes' with little or no attempt to justify or even to elucidate this rating. Occasionally an investigator ventured so far as to give numerical weights or percentages of 'influence' to the various 'factors'. When several authors dealt with the same social phenomenon they differed considerably regarding its causation. [...] The almost complete lack of any well-considered methodology was very noteworthy. (MacIver 1942: 73–4)

It is hard to avoid drawing the same conclusion about much social research today. And, as I have made clear in this chapter, MacIver's damning judgment is as true of qualitative as it is of quantitative work.