

Part I

Introduction

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Philosophy and Sociology

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Sociology is a discipline based on a philosophical idea: that there could be a science of social life. Beyond this bare thought there is a great deal of dispute. The disputes range from the question of whether it is true in any sense that there can be a science of the social, to the question of what kind of science it could be, to what “the social,” “social life,” “society” (or the many variants on this term) could be. The idea of a social science was born not in an empty field, but in a domain that was already crowded – with fields like philosophy, religion, ethics, legal science, and various other disciplines and sciences which had claims to explain or correctly describe this domain. There is no well-defined boundary to sociology as a discipline. Different national traditions have managed the relation to other disciplines differently, and operated in the context of disciplines that were and still are different. The term science and the German term *Wissenschaft* take in different territory. *Wissenschaft* includes any organized body of knowledge – and, in the hands of the neo-Kantian philosophers who dominated at the time of the birth of German sociology, the logical organization of the field in terms of fundamental concepts had special implications. In this chapter I will discuss both philosophical sources of sociology: the law and cause tradition begun by Comte and Mill and the tradition that develops from Kant.

COMPLEXITY: THE CORE ISSUE

John Stuart Mill grasped a basic issue with the idea that social science could be composed of causal laws, as the rest of science was: the complexity of the causes that work together to produce social consequences. Indeed, causal complexity is at

the historical center of discussions of the problem of social science knowledge: too many variables, too many interacting causes, and no good way to untangle these causes. The key problem arises from the addition and mixture of causal effects: unless the scientist is in a position to calculate the joint effects of two causes, and to extend the calculations to the addition of other causes, prediction of outcomes involving multiple causes is impossible. But the identification and discovery of predictive laws faces the same problem: the actual causal facts or relationships which appear empirically are already compounded of a long list of mixed-up causes, from which laws must be extracted and discovered. In a very simple case, one might be able to hypothesize both the laws and the mathematical nature of the additive relationship and find that one set of laws and one rule for combination of causes actually predicted the outcomes. But such simple cases are never found (Mill [1843] 1974: 591–603; Turner 1986: 40–59).

The most sophisticated American enthusiasts of the idea of science, including those who influenced “mainstream sociology,” such as Franklin Giddings, understood by 1901 that sociology was not going to consist of laws (Giddings 1901). The causal knowledge of sociology would consist, they thought, of correlations, and, at best, sociology would discover a set of variables whose correlations persisted in a variety of circumstances (Giddings 1924: 33). There was a difficult philosophical problem with this answer. What is the relation between cause and correlation? Karl Pearson, who was the source for Giddings, took the view that the distinction between correlation and cause was bogus, and that the laws of physics themselves were correlations, just strong ones, and that there was no intrinsic or special connection between the two variables in the correlation other than one coming before the other (Pearson [1892] 1911). Applying this to social science knowledge was problematic. When the correlations were represented by the kind of scattergrams found in the social sciences, with their wide dispersion around a regression line, it was less clear that one knew what the correlations meant, or that they meant anything at all. One of the main sociologists in this tradition, W. F. Ogburn (1934), compared the interpretation of these scattergrams to interpreting an editorial cartoon in a newspaper – meaning that the scattergrams were objective, but the interpretations were subjective and not part of science at all. This was an extreme view, but as we will see, it points to a problem shared with other accounts of the meaning of correlational sociology, and points to the larger problem of subjectivity and objectivity in relation to the project of interpretive sociology.

The correlational tradition evolved in an odd way. The basic ideas of correcting a correlation by partialing, by determining whether the causal effect went through another variable, and the addition of multiple variables in order to see if adding a variable influenced outcomes were there very early. The classic papers by G. U. Yule in the 1890s asked the question of whether providing public relief for the poor outside the poorhouse helped alleviate poverty or generated more poverty (Yule 1896, 1899). This was a typical multi-variate problem: one could look at rates of poverty across many administrative districts, but this alone would not prove anything. Districts also differed in many other characteristics, many of which might also affect the level of poverty. Moreover, a simple correlation would not suffice: the interesting question was change. Does the introduction of outdoor relief – that is to say aid other than in the poorhouse – have the consequence of changing the

number of poor people? Yule found that it did – that making it easier to be poor meant that more people chose to be poor – and that if one added other variables to correct for their effect, the basic results did not change, indicating that this cause could not have been spurious or a matter of confounding, and also, to the extent that the added causes represented the possible causes, there could be no cause that would make it spurious. By the 1920s, American sociologists were using methods involving partialing with intervening variables to determine whether schooling or Mexican background was the cause of illiteracy in the Southwest, and whether the influence was expressed through the lack of schooling or was a consequence of Mexican descent, which suggested language issues (Ross 1924).

These methods worked because they could rely on background knowledge about cause – the knowledge that such things as schooling and Mexican descent might have causal effects on literacy, or that making it easier to avoid work would lead to people avoiding work. This was not knowledge derived from the data. All the data allowed for was correlations. The pictures, the scattergrams, could have been interpreted by Martians having no background knowledge as having no causal significance, or as showing that literacy caused schooling or Mexican descent. Nor was it “subjective” in any usual sense. But it did not fit into the paradigm of “science” either.

So these methods were a puzzle: they made sense, but not in terms of the usual ideas of science. There was a strong temptation to assimilate them to the idea of experiment, through the notion of natural experiment, but this merely had the effect of highlighting the problem of assumptions. The result of this comparison was depressing: if one could correctly assume that the situation was like an experiment, meaning that all the relevant variables were included and randomization was approximated so that there were no spurious relations, one could draw causal conclusions. But these things were precisely what was not known and was difficult to warrant by background knowledge. Nevertheless, background knowledge provided a solution of sorts to the problem of cause: it could at least say what a possible causal relation was.

This tradition did produce a philosophy of science, both within sociology and within the larger methodological literature. It had an idea of what science consisted in: the discovery of quantitative relations. The available relations, in a world of overwhelming causal complexity, were correlations between quantitative variables that could be concocted to stand in for the kinds of facts that interest sociologists – facts about labor unrest, class, or attitudes toward different immigrant groups. The strategy was to look at what is objectively determinable, find the objectively determinable connections between them – which are correlations or associations rather than laws – and hope that the results add up to something. It was nevertheless evident very early that what it would add up to was not “science” in any familiar sense, and certainly not physics. But the pill was bitter, and there was no easy alternative model of science which fit the situation of sociology.

Cause was difficult to abolish by philosophical fiat, as Pearson tried to do. It came back under “Logical” positivism in the guise of the distinction between “genuine” laws and accidental laws. In the example of Ernest Nagel, the generalization “all the screws in this car are rusty” explains nothing, even though it is a true generalization (Nagel 1961: 49–52). Nor would it explain anything if it was stated

in general form – if we invented a term “scarscrews” for the screws in this particular car, and said, “all scarscrews are rusty.” But this distinction was easier to make and imagine than to put into practice. In the natural sciences, it was generally only necessary to place the generalization – for example about rusty screws – into a structure of larger generalizations, in terms of which the original generalization could be explained, such as, “screws with iron content that are exposed to oxygen and moisture rust.” In the social sciences, there were no such true generalizations.

THE POSITIVISM DISPUTE

What in the 1960s came to be called, and reviled as, “positivism” was a strange amalgam: it was on one side a celebration and defense of the kind of statistical sociology which was concerned with the problem of cause and in particular the problems of distinguishing causal and spurious relations which, on the other side, employed the language that the Logical Positivists had developed to account for physics and then extended to explanation in all science. The two were incompatible. The Logical Positivist model of explanation needed genuine laws. The methods of statistical sociology produced something different, namely correlations or associations.

This fundamental conflict was obvious enough to the more sophisticated participants. Hans Zetterberg, whose *On Theory and Verification in Sociology* (1963) was the most influential text on “theory construction” of its time, argued that the laws of sociology could only be probabilistic laws. There were also attempts to construe the statistical material, the early forms of causal modeling, in terms of laws (e.g., Simon 1954). None of this worked. The problem for the attempt to construe causal modeling in terms of laws was the status of the assumptions needed to draw causal conclusions from the models, notably the uncorrelated error terms assumption. Simon argued that this assumption was “empirical.” But it was not an assumption that could be tested – either directly or indirectly – in a definitive way (since any test using similar models would need to make new assumptions of the same kind). The problems for the idea that the laws of sociology were probabilistic were insurmountable. First, there were no such laws to be found, for reasons familiar since Mill: complexity meant that they would be hidden and impossible to discern, even if they were there. Second, it was impossible to derive these laws from one another in the kinds of hierarchical structures of laws. This is what the Logical Positivist approach to distinguishing genuine laws from accidental laws required.

The later history of this problem in philosophy takes a surprising twist, to be discussed below. But this twist came too late to stave off the long and confusing discussion of “Positivism” that occupied the decades of the 1960s, 1970s, and beyond. The profitable part of this discussion requires some background, which will be given in the next section. The unprofitable part concerned the long attempt to evade the message that there were to be no laws of sociology and the equally relentless effort by the critics of scientism to refute the evaders. The evasions mostly originated in the writings of Columbia sociologists associated with Merton and Lazarsfeld. Zetterberg came from this milieu as well, but was trained by F. Stuart

Chapin, who was himself trained in the earlier Pearson-Mach tradition applied to sociology by his own mentor, Franklin Giddings.

The primary source of the evasions was Robert Merton's influential writings on the relation of theory and method and middle-range theory, along with Lazarsfeld's ideas about the generalization of the findings of localized studies into higher-level generalizations (Merton [1957] 1968: 39–171; Turner 2009a, 2009b). This same idea of generalization was taken up by Glaser and Strauss in their book *The Discovery of Grounded Theory* (1967), which taught that one could generate theory by giving an explanation of findings in ordinary non-theoretical terms, and turning them into theory by substituting more general terms in the place of these terms. Merton made many gestures to the idea of sociology as a theoretical science and the idea of science as a body of propositions which could be derived from one another, but never addressed the question of how the dross of ordinary statistical association could be converted into this kind of gold. Instead he vaguely appealed to the idea that the accumulation of the kinds of statistical results Lazarsfeld and his students generated would, at some point in the future, become the basis for genuine theories unified into deductive wholes.

The discussion was fruitless because the model of science was incoherent. There was no way to get from statistical associations to “theory” in the sense of science – deductive theory in which one derived laws from one another. The relationship that virtually all these theory construction accounts relied on was the idea that a correlation between A and B and B and C warranted the claim that A and C were correlated. This warrant helped only under special circumstances (of particular combinations of relatively high correlations) and could not be relied on to continue to work to predict, for example, that A and other correlates of C, such as D, would be correlated, or that the correlates of D would be. The warranting relation was genuine, but very weak, and insufficient for anything like the long chains of deduction necessary for the deductive theory that was supposed to be the goal of this activity.

Nor were there any theories in the sense of science available in sociology to be critiqued. The actual Logical Positivists said almost nothing that supported the idea that “positivistic” social science as practiced by statistical sociologists would lead to genuine theories either. They did attempt to give philosophical analyses of such things as functional explanation, on the assumption that this is what sociologists were actually offering. But the analyses consistently showed that the extant examples of functional analysis were far from adequate or complete, and indeed were little more than empty speculation. One of the few philosophers to write extensively on social science who could be associated with “positivism” was Karl Popper. But Popper argued for a form of social science explanation involving rational reconstruction of situations of action, not the construction of deductive theory ([1957] 1961: 140–52).

In the German-speaking world, Popper became the subject of the *Positivismusstreit* or Positivist Dispute (Adorno, Albert, Dahrendorf, Habermas, Pilot & Popper [1969] 1976). Popper, however, was not (and insisted he was not) a positivist, and his actual writings on social science, including the idea of rational construction of situations of action, derived more or less directly from Weber ([1968] 1978: 6), and much of his work challenged the idea that there could be meaningful social science

explanations of such things as science. So this extensive discussion, which ranged over many topics, never engaged the issues with statistical sociology and the idea of generating a science out of these materials. It nevertheless raised interesting questions about the necessary role (and character) of interpretation in the face of meaningful social action, to which we will return.

THREE PATHS FROM KANT

The problem of interpretation has an autonomous history, which requires its own background. Interpretation is usually thought to be subjective, and thus antithetical to science. We have already encountered Ogburn making the sharp contrast between the objective scattergram and its interpretation, which is as subjective as interpreting a newspaper editorial cartoon. But what if science itself is an interpretation? Kant is esteemed as the greatest modern philosopher for his insight that our world is understood by us not as it is, but as we ourselves organize it intellectually, into categories and objects. The core idea was expressed nicely by Georg Simmel in the opening lines of his classic paper, "How Is Society Possible?"

Kant could propose and answer the fundamental question of his philosophy, How is nature possible?, only because for him nature was nothing but the representation (*Vorstellung*) of nature. . . . As the elements of the world are given to us immediately, there does not exist among them, according to Kant, that coherence (*Verbindung*) which alone can make out of them the intelligible regular (*gesetzmässig*) unity of nature; or rather, which signifies precisely the being-nature (*Natur-Sein*) of those in themselves incoherently and irregularly emerging world-fragments. (1910–11: 372)

If this account is correct, even making a scattergram into a fact requires the organizing activity of the mind. And the objectivity of the scattergram itself, in this account, comes from the fact that the organizing structures of the mind are shared with others. Neo-Kantianism salvaged from this problematic theory the idea that intellectual domains were constituted conceptually but added the idea that different domains were constituted conceptually by different presuppositions or guiding concepts.

This was an idea that proved to be extremely fecund, in three ways. The first is simply in the idea that groups have shared presuppositions. The idea that different groups have different worldviews, standpoints, and so forth is a staple of present sociology. It is familiar to us today in the form of such notions as paradigm, worldview, basic assumptions, and is part of the background to Michel Foucault's use of the concept of *episteme* (1980: 197) for what he called "the historical *a priori*" ([1966] 1970: xxiv), Bourdieu's concept of *habitus* (1977: 16–20), Mannheim's concept of ideology ([1929] 1936), Ludwig Fleck's thought styles ([1935] 1979), and so forth.

The interpretation by sociologists of "lay" concepts, the concepts which actors themselves use to constitute their own social worlds and interpret one another, is a characteristic activity of the social sciences: they must reinterpret and theorize the frameworks which lay actors use to interpret one another. Thus the social sciences

depend on lay frameworks, and are forced to do so because the conceptualization, framing, and interpretation of people by one another is a basic part of social life, social action, and social interaction – a resource, in the terms of Harold Garfinkel (1967), which the social scientist must also use to make sense of action, but must then turn into a topic, in order to analyze and account for the ways people organize their world conceptually. These social science concepts, as Anthony Giddens points out, can become lay ideas and thus part of the culture. Indeed, this is also the source of the value of the social sciences, for, as Giddens says, “The best and most original ideas in the social sciences, if they have any purchase on the reality it is their business to capture, tend to become appropriated and utilized by social actors themselves” (1987: 19).

It is difficult to overstate the influence and usefulness of the idea of a worldview. The general Kantian image of a world of presupposed symbols or concepts that organized reality was a handy explainer and means of interpretation in the face of differences of opinions, perceptions, focuses of interest and concern, and so forth. And this very abstract theoretical idea had a strong empirical element when it was closely associated with ideas about community. Fleck wrote about thought collectives and thought communities, which captures this connection. We could think of thought in the Kantian sense of conceptually organizing the world to make the world into a set of cognizable experiences and objects – into the familiar world of daily life – and make this into a fact about community life that could be studied and treated more or less like an empirical fact itself.

This gets us two paths from Kant: the idea that each of us depended on and shared the worldview or fundamental presuppositions of our society, and the idea that understanding the worldview of other people required us to conceptually organize or construct the phenomenon of communities possessing worldviews. Contemporary “cultural sociology” depends entirely on this image. The “culture” that makes up its subject matter is exactly this conceptually-organizing stuff that is shared by a community and produces its characteristic experiences of the world. What makes cultural sociology an empirical subject is the fact that it can study the outward manifestations of this stuff: books, television shows, movies, rap songs, and the like.

But there is an ambiguity here, which shows up especially in relation to other cultures. What is the framework from within which we interpret other cultures? Is it the framework of our own culture? And could it be otherwise? If we are explaining the theory of mind of the Akan in Ghana, how could we do it without reference to and comparison to, or translation into, our theory of mind? If this is the case, interpreting is more like translation from one language to another than the production of facts that are the same for everyone. If science is that which is the same for everyone, it must depend on universally shared presuppositions. What is social science then? Is it a universal set of organizing concepts, or is it an extension of our own lay concepts that clarifies distinctions that are already part of our own culture? Weber would have answered this question by saying that the clarity we seek is clarity for us, in our own historical situation. Durkheim and Parsons would have insisted that we must analyze in terms of universal concepts to be scientific (Wearne 1989).

The idea that sociology can provide a universally valid single conceptual scheme is the third path from Kant. It is historically important in sociology, and is important

today in philosophy. It is also one of the topics that divides contemporary sociology from contemporary philosophy. The third path from Kant goes back to Kant's original aim: to describe the conceptual conditions of natural science and therefore scientific truth, truth about the physical world, itself. Kant thought Newton was right about the physical world, but that the philosophical task of accounting for the truth of Newton's physics could not be completed by an empiricist account of knowledge. No collection of experiences or experiments could add up to the concepts of space and time: physical evidence, for example measurements, presupposed these concepts. So the task of the philosopher was to describe these concepts and account for their unique validity. The emphasis was on the uniqueness of the solution: there was one set of presuppositions that made physics valid. The presuppositions were thus a part of the science itself, inseparable from it and from its validity.

This general idea was turned into a formulaic method by the neo-Kantians. To have a science, one needed a single set of presuppositions in terms of which the concepts of the science could be validated and the statements within the science could be understood to be true. The formula was this: one science, one set of constitutive presuppositions organized hierarchically into a unified conceptual whole that accounted for and shows the logical or conceptual connections between all the concepts that defined the content of the science. This was a formula that could be made to work for all manner of sciences. The organizing conceptual principle of legal science was the concept of justice; for theology, God; for biology, life; and so forth. To be a science (or rather to be a science in the sense of the more expansive German notion, a *Wissenschaft*) for the neo-Kantians was to be a domain conceptually organized in this way.

This idea had a direct implication for sociology: if sociology was to be a science, the first order of business was to get its domain conceptually organized, and in the same way – as a hierarchical structure of concepts topped off by a single organizing idea. But this strategy of choosing a fundamental idea, as Simmel himself saw, ran into a serious problem. One could choose other organizing ideas, and one's choice of organizing idea was often ideological, or associated with a party position. In German sociology there was a flood of attempts to provide similar organizing structures for sociology, based on different ideas.

NEO-KANTIANISM IN TROUBLE

Although the pervasive image of the two worlds of nature and the symbolic, meeting to constitute our experienced world, is a compelling one, it is also one fraught with trouble. These troubles eventually became apparent within neo-Kantianism itself, and they were apparent to critics all along. One problem was the kind of relativism noted in the last paragraph: there seem to be many possible conceptual starting points, and no grounds on which to choose. This problem was "solved" in a way that proved to be historically important, in the middle of the twentieth century, by Talcott Parsons. Parsons combined the basic idea of neo-Kantianism with other arguments. He used the model of the physicist Willard Gibbs to answer the problem of complexity: Gibbs was famous for constructing an approximation of a thermodynamic system seeking equilibrium. The model was rigorous, but it worked by

ignoring variables. This suggested to Parsons that a simplification based on a short list of central variables could produce an approximation to the goal of a scientific theory consisting of differential equations. Instead of trying to ground his choice of an organizing idea phenomenologically or by some other means, he based it on a functional argument: that society would not be able to solve the Hobbesian problem of the war of all against all without norms. He then argued that a normative aspect was an essential feature of human action. Normativity or the valuative was the key aspect of action that none of the other social sciences had claimed. Hence it was the appropriate basis for a conceptual scheme that would make sociology a science.

In this way the problem of neo-Kantianism has reproduced itself in the history of sociology: as a problem of competing perspectives versus overarching claims for a single unifying or genuinely scientific perspective. But this is not simply a problem of pluralism. The problem involves the nature of the alternatives. If they are simply different means – different methodological approaches, for example, to the end of scientific truth – we may find that pluralism is irreducible in fact, because each means produces results that contribute to the goal of truth without any other means being decisively superior or encompassing the results. But we face a different problem using approaches with different fundamental presuppositions, for example, about what objects, such as “society,” exist. If the presuppositions are really fundamental, there is no basis on which to judge them. By definition, we have reached the end of the chain of justification.

The claim that there are alternative fundamental presuppositions that cannot be decided between is relativism. Underdetermination is something different: in cases of the underdetermination of theory by data, the data cannot decide between alternative theories, because the data are consistent with both theories. Underdetermination is a kind of factual situation that could be otherwise – it could be that there were no alternative theories that fit the data (though one of the important discoveries in twentieth-century philosophy in the wake of positivism was that it was possible to generate alternative theories with different ontologies, that is, different lists of things that exist, by varying the theories that already fit the data by providing alternatives to the theoretical terms of the theories which made different things “real”). One can speak of the presuppositions or even the ontological presuppositions of alternative theories, but in these cases the theories are themselves tested against the data, a test they may fail (Quine 1970; Gibson 1982: 84–90).

Underdetermination is not neutral between existence claims (Quine & Ullian [1970] 1978: 64–73). If one gets the same predictions with a theory that uses the term “society” and one that does not, there is a presumption that there is no such thing as society. “Society exists” is not the same as “a theory that employs the term ‘society’ is consistent with the data.” It is more a claim that one cannot account for the data without reference to society. The term “fundamental” has different implications. It means that the presuppositions are beyond correction entirely – by data or anything else. There is no test that fundamental presuppositions can fail.

There is an important issue here, and it bears on the problem of relativism. What is the status of the following ideas: that one selects between theories on the basis of how well they fit the data and that one would prefer the theory that does not require us to believe in unnecessary entities – such as “society”? These belong to the realm of reason, not fact or data. An argument for the idea that there are

conflicting fundamental presuppositions, the key notion behind relativism, is also an idea in the realm of reason – universal reason. Claims about this sort of thing are universal appeals. So even the relativist must concede that there is something universal and rational. Hence relativism is self-contradictory and untenable. “Fundamental presuppositions” are themselves only intelligible as presuppositions if we assume some sort of universal rationality, with binding norms of reason that govern our inferences.

If this is true, which most contemporary philosophy takes for granted, then not only is relativism false, but there is a distinction between two senses of normativity. There is “sociological” normativity, which is to say the norms observed in fact in a given society or social setting, and “genuine” normativity, the normativity of reason itself, or of the universal community of rational beings (cf. DeVries 2005: 262–8). To even talk about other presuppositions requires us to speak rationally – rationally in a universal sense that applies to the people we are discussing and their presuppositions – as well as discussing the topic sociologically.

THE STATUS OF THE PROBLEMS TODAY

Reason and normativity

This reasoning sounds very esoteric, but it is at the heart of contemporary Anglo-American philosophy. Is all reasoning “cultural” and thus culture-relative, or is there something in the way of universal reason or even morality that is beyond the reach of sociological or any other kind of natural explanation that binds people normatively? Is any rejection of the idea of universal reason a descent into irrationalism and an example, as a recent book title puts it, of the *Fear of Reason* (Boghossian 2007; see also Searle 2009)? This deep problem is the legacy of Kant and neo-Kantianism. Sociology, by employing the useful notions of worldviews, paradigms, and so forth, brings this problem on itself.

The most dramatic form of the problem in sociology is in the sociology of scientific knowledge, which attempts to explain scientific beliefs causally and sociologically (Barnes, Bloor & Henry 1996). Sociologists of this kind account for the development, acceptance, and the appearance of the truth of scientific claims in terms of the thinking of a specific community, a community of a particular kind, with its own distinct practices and worldview. Does this conflict with the idea that the results are rational, in accordance with the truths of the natural world? One answer is this: how could it not conflict? The idea of universal norms of binding rational justification is the basis of the claims of science to intellectual authority and universal validity. The sociologist of scientific knowledge depicts these results as the product of a historically contingent, non-universal, set of distinctive practices and presuppositions in terms of which the results are “true” for the members of the community. This is relativism plain and simple, because it denies universal validity, relativizing scientific truth to local practices.

Is there a way out of this problem? One way is to separate the problem into two aspects: the question of whether the science is correct and the problem of explaining why the scientists came to these beliefs. The two, it can be argued, are completely

separate questions. Claims about genuine truth, claims about what should be believed, belong in the domain of genuine norms, while explanations of what scientists actually believe come from the domain of sociological norms, norms that hold for a community. But for a believer in the idea that there is a universal rational framework which binds all rational persons – a Kantian in the primary sense – it is this framework that is supposed to explain rational behavior, not local frameworks of the kind appealed to by sociologists of science. “Binding” is an explanatory idea.

The sociologist’s explanation competes with the Kantian account that claims to be an explanation of rationality. And the sociological account has to be wrong, because it is a denial that the binding framework of rationality is actually doing its job of binding – if it were not the case that we experienced the binding character of rationality, there would be no such thing as a genuine norm of rationality. So Kantianism is a theory of the mind and its relation to the world as well as a theory about rational standards. The sociologist cannot deny this, because after all the sociologist is also trying to persuade us that the claims of sociology are true and not merely a result of local presuppositions – including when the claims of the sociologist *are* about local presuppositions!

But there is a complication. Where do the norms of rationality come from? Evolution? If so, no one can explain how, and in any case it is common to explain our deviations from rationality by appealing to evolution. More important, we need a general account of norms, not just an account of rationality. Some “genuine” norms are clearly local – linguistic norms, for example, the norms that determine what words and sentences really mean. For these we need explainers that are specific to the community of language users – which is to say we need something like a sociological explanation. But standard sociology has no such explanations, because the concept of genuine normativity is not a concern to sociology, and one needs to go back to classical social theory to find serious discussions of this problem at all.

The standard philosophical solution to these issues has been this: we need some kind of sociology – a philosophical one. Durkheim, with his appeals to the collective consciousness, was close to a solution, but his idea that there was a real mind-like entity out there causally influencing our thinking and binding us was problematic. If we could keep the idea of binding, we could keep the distinction between what is binding as a norm and what people actually do. And we could do this if we could find an unproblematic ground for this distinction. The solution is the idea of collective intentionality – that the binding comes from shared intentions which individuals may deviate from while still sharing in the collective sense – we might steal, but still be aware of and share in the collective disapproval of stealing (Searle 1995; cf. Gilbert 1989; Turner 2002: 35–57).

Are there such things as collective intentions? Or is this merely a bizarre philosophical invention? If the idea of collective intentionality made sense, it would provide an instant explanation and alternative account of the social world to sociological theory – notions like culture could be replaced by appeals to collective intention, and similarly for appeals to norms. Yet this seems to be explanation by philosophical fiat. Collective intentions are not part of the ordinary explained human or empirical world. Their existence – the existence of genuine norms behind actual usage – is inferred from supposed necessities of reasoning, for example from

the “fact” that sentences have meanings, or that there is a “correct” way to continue the series 2, 4, 6, 8.

Understanding

Worldviews, paradigms, and the like are not empirical facts in any normal sense: they are theoretical ideas that attempt to make sense of a very complex phenomenon of thinking, experiencing, feeling, and so forth. They have a purpose: making sense of behavior and beliefs that are otherwise puzzling. Is there a way to accomplish this purpose, and explain our capacity to understand, without appealing to these problem “facts”? One way out is to reject this whole line of theorizing. There is a long tradition in American sociology, rooted in the thought of George Herbert Mead, which does reject it. Herbert Blumer thought of symbolic interactionism as not merely a perspective like other perspectives within sociology, but as a complete alternative to sociology, psychology, *and* cultural anthropology. The reason anthropology is on this list is that Blumer rejected the idea of culture. The kind of understanding between people which is produced by taking the role of the other does not require a fixed set of cultural assumptions.

Mead and many of his philosophical and sociological contemporaries at the turn of the nineteenth century were inspired and fascinated by James Mark Baldwin’s account of child development. They reasoned that whatever the child got in the way of understanding the world and the social world had to make sense in terms of actual developmental processes, including social interaction. The idea that there was a culture which was out there to be “introjected,” as the mythological Freudian language had it, was not consistent with this idea: what one saw in observing children was the development of the self, as Cooley put it, through the looking-glass of other people’s reactions and perceptions, and the child learning roles and about roles, and coming to understand others by enacting them.

This mode of thinking about interaction has recently acquired a surprising set of philosophical allies in the philosophy of cognitive science and in neuroscience itself. There was, in the 1990s, an elaborate controversy over the way in which children acquired a concept of mind or a sense of the mindedness of other people (Stone & Davies 1995). One answer was that they acquired a “theory of mind” just as people acquire a culture. The other answer was that simulation was a basic mental process, and that in interaction we routinely simulate other people’s actions in our own mind in order to understand them. This reasoning received powerful support with the discovery of mirror neurons in monkeys (Gallese & Goldman 1998), and the development of a large experimental literature showing that our perceptions of the particular actions of others – dancing, for example – employed the same neurons that we would employ in performing the same acts (Calvo-Merino, Grèzes, Glaser, Passingham & Haggard 2006; Cross, Hamilton & Grafton 2006).

Simulation allows for an understanding of human understanding of other people and their actions that does not depend on the whole machinery of culture, assumptions, introjection, and the rest of it. And the fact that this kind of understanding is rooted in actual neuronal processes brings the whole problem of interpretation down from the clouds of hermeneutics. Hermeneutics modeled human interactional

understanding on the interpretation of texts. And this is what gave it a non-objective character. Ogburn's example of the newspaper cartoon and its interpretation comes to mind. For him it was the very model of subjectivity. Although simulation does not guarantee objective results, rooting it in a natural process changes things. It allows us to see how data can be used to correct initial errors, producing a better understanding (Ickes 2009; Nickerson, Butler & Carlin 2009).

Objectivity and subjectivity in causal models and elsewhere

The "positivism" debate ended in a muddle over the idea of laws and deductive theory. But the discussion of causality in social science that developed in philosophy took off in a different direction. In a 1983 paper, "Social Science and Social Physics," Clark Glymour argued that the obsession of social science with imitating physics was a mistake, and that causal modeling provided an appropriate and adequate approach to social science causality that did not depend on laws. Causal models were not like laws because laws hold unconditionally and in all circumstances. Causal models hold under conditions that are unknown and perhaps unknowable, and fail to hold when these conditions change. Nevertheless, where the conditions hold, the relations of causality captured by the models are real (cf. Glymour, Scheines, Spirtes & Kelly 1987).

In addition, Glymour pioneered, with several collaborators and alongside Judea Pearl (2000), an approach to the discovery of causal structures that used computer search methods to eliminate possible causal models, and detect spurious relationships and hidden variables. One of Glymour and his associates' most striking findings using these techniques was that the iconic quantitative study of 1970s sociology, Peter Blau and O. D. Duncan's *The American Occupational Structure* (1967), consisted entirely of spurious correlations (Glymour 1997: 222–3; Freedman 1983).

This new philosophical literature was very slow to penetrate sociology. But it changed the problem of objectivity in a special way. The aim of this approach would have been familiar to Ogburn: to come to conclusions with a minimum of subjectivity. But to make the search process work, it was necessary to proceed by elimination. If one could use one's background knowledge to eliminate a large number of potential causal connections, it would be possible to generate smaller sets of possible causal structures in a given domain. If the background knowledge in question was truly innocuous – for example that the number of years a parent spent in school could influence whether a child smoked, but not the other way around – one could eliminate many possible models with very little subjectivity.

A similar strategy, of minimizing reliance on all but the most innocuous background knowledge, is central to various forms of network analysis, from the kind popular in American sociology to the Actor Network Theory of Bruno Latour (1987). The idea in each case is that knowledge about people's intentions, beliefs, and motivations, which is "subjective," can be minimized, with the result that any findings will be "objective." The differences, however, are differences of degree. Even the placing of objective labels on network objects requires some degree, however minimal, of background knowledge or "subjective" understanding.

CONCLUSION

The early relation of sociology to philosophy was one in which philosophy supplied methodological ideals, and in which sociology competed with philosophy over subject matter, such as the nature of morality. The slow demise of the idea that sociology could be a physics-like science freed sociology from this relationship of subordination, and allowed its methods (especially in relation to causality) to gain recognition as valid in their own right. But the competition over subject matter remains, with philosophers increasingly inclined to attempt to take back subject matter that was earlier conceded to sociology. Anti-relativism has been the main motive for this attempt, and its main strategy has been to provide or defend alternative “philosophical” explanations of sociological facts, using such concepts as collective intentionality, rationality, and normativity.

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