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This anthology surveys an intellectual landscape vastly and importantly reshaped over the last 25 years. Historically, the philosophy of the social sciences has been an inquiry loosely organized around the problem of the scientific status of social knowledge. This problematic emerged with social sciences themselves in the latter part of the nineteenth century and continued, in one form or another, to dominate discussion through the better part of the next. A trio of core issues — the scientific status of intentional explanations (and agency), the nature of rationality, and the methodological hallmarks of science — seemingly persist through current discussion and debate. But the substance attached to these issues has fundamentally shifted and altered. Without examining details of the substantive changes, the shifts in the subject matter remain obscured. This introduction examines these shifts and proposes an explanation of how and why they occur.

Whatever science is thought to be, it is, at the minimum, a science of the natural world. The questions this formulation raises are: can we have scientific knowledge of the social world? If so, what does “scientific knowledge” mean? Philosophy of science focuses primarily on answers to the second question. Philosophy of social science traditionally has taken those answers and attempted to determine if the conditions making scientific knowledge possible in the natural realm obtain for the social order as well. The guiding assumption in all of this is that an answer to the question of what constitutes the nature of scientific knowledge
provides, *inter alia*, a demarcation criterion, a way of cutting the difference between scientific inquiry and mere pretenders.¹

The structure of this anthology reflects the editors’ views of the change in the underlying problematic governing philosophy of social science. The issues are no longer organized around the familiar topics borrowed from philosophy of science: what is a law, what is an explanation, what are the ontological units (e.g., holism v. individualism), which sciences are primary (reductionism), what is the structure of theories, and so forth. Rather, we now find a field organized around a poorly bounded collection of cross-cutting debates and issues. Some involve the appropriation of a natural science by the social sciences, some claim to incorporate explanations of how natural sciences function within a social scientific framework, and some simply propose new and better ways to do the traditional job of explanation and prediction. Many topics compete in the struggle to unify the understanding of how social science *does* function as well as how it *ought* to.

Debates about standards of rationality and causality remain, in interesting and important ways, central to concerns in the area, but with important shifts in epistemological emphases. Among the new problems are these. How do presumptions about agency, normativity, and value -- those ghostly qualities thought to constitute and animate us -- fit with the idea of a science of the social, of society as a stable, regularity-manifesting machine? Ironically, with the ascendancy of rational choice explanations, the natural sciences themselves have become objects of explanation, even of justification, by a methodology of decision theory most closely associated with the social sciences. The issue now is whether naturalism presupposes “rationality” in a normative space, or whether “natural” facts explain rationality and normativity.

**The Origins of the Philosophy of Social Science**

Natural science preceded social science, but in a sense, the philosophy of science and the philosophy of social science were born together. Questions of scientific methodology prior to the emergence of the social sciences had a distinctly different character.² The question of what is a law, what is an explanation, and many related questions did not take a well-defined general form until they had been faced with the problem of applying them to social science. The problems presented to notions of scientific inquiry by the social sciences are arguably what makes it intellectually important to answer questions such as what *in general* is “science,” or “scientific explanation,” or “scientific law.” Not unreasonably, one may regard the philosophy of science and the philosophy of social science as both originating in the problem of the scientific status of the social sciences. The notion of science as consisting of a special method emerges only in the nineteenth century in the face of this problem. The question of what serves to define science and the issue of whether social inquiry conforms to the “method” were contested
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from the start. The writings of those who set the agenda for what were to become rival conceptions of the philosophy of science, Auguste Comte and J. S. Mill, on the one hand, and on the other hand, John F. W. Herschel and William Whewell, divided on the subject of social science in the same way. The former were on the side of the suspicion of fictions, including the theoretical entities of social theory, which Comte argued belonged to the prepositive or prescientific “metaphysical” stage of the development of social thought; the latter on the side of the insistence that explanation required theories that made sense of data, going beyond it rather than merely summarizing it, as in fitting a curve to data points.3

Subsequent philosophical writing on the social sciences has never left these problems behind, though it has reproduced them in various different combinations. But it is more important that the social sciences have themselves never produced results that could be uncontroversially and unambiguously assimilated to the usual philosophical answers to these questions.4 One indication of the significance of this is the fact that while little of the literature in the natural sciences concerns itself with philosophy, the situation in the social sciences is quite different. The problem of whether the social sciences are sciences has historically been closely bound up with the development of the subject matter itself.5 The disciplines of the social sciences themselves produce a large literature on the various claims of these disciplines to be sciences, much of which is inspired by writings about the philosophy of science and the philosophy of social science. Not infrequently these are the writings of philosophers of science of generations past, and necessarily so, since the writings of recent generations, for example, Larry Laudan and Bas van Fraassen, are irrelevant to the problem that inspires them: of “justifying” social science or instructing it in how to become “scientific.”

What this suggests is that the philosophy of social science stands in a fundamentally different relationship to its subject matter than does the philosophy of science. The philosophy of natural science can treat its subject matter as, if not a finished object of analysis, then at least one that is autonomous--existing independently from philosophical speculation on it. Philosophy of social science lacks precisely this kind of second order or “meta” relationship to its subject matter. Philosophical considerations, especially with regard to claims to scientific status, have been intrinsic to the identity of and to movements within the social sciences.

The historically central problem of the scientific status of social science no longer constitutes the core of contemporary philosophical discussions about the social sciences. One reason for this shift is apparent, and it occurred on the side of philosophy of science: 150 years of reflection on the elements of the “scientific method” has not resulted in a consensus that there is a “scientific method,” much less a full-blown demarcation criterion. Confidence that there is a methodological essence to science has decreased as what counts as science has come to appear more historically plastic and contingent. The temptation or need to engage in ongoing disputes about the scientific status (or lack thereof) of putative knowledge of the social has, accordingly, waned.
During one period, however, philosophy of science left its mark on social science itself. The high tide of logical positivism and the “unity of science” movement, when positivism made highly influential and public attempts both to provide general answers to questions about the nature of science knowledge (including related notions of law, explanation, theory, and reasoning) and to apply these results to the social sciences, coincided with a period of rapid expansion in the social sciences. So, despite the fact, as Tom Ubel points out in his chapter, that the social sciences *per se* were never an important concern of positivism and the unity of science movement, positivism’s impact was significant. Its formulations were thought to hold the methodological key to becoming a science at the time the social sciences were most anxious about doing so.\(^5\)

Throughout the literature of this period of intense interest, which may be (very roughly) dated from the early 1930s through the 1960s, one nevertheless finds a telling tension between the analytic philosophical style of the logical positivists and the concerns of those who were “building science,” for example, engaged in the actual business of using empirical data in the form of experiments in large data sets to actually generate scientific results. The tension is telling because it signals that something is seriously awry. Unlike the attitude towards the natural sciences – that is, that they were doing something quite right, and philosophy would help pinpoint exactly what that is – the approach of positivism to social science suggested that all disciplines so named required wholesale reform.

Ironically, those thinkers in the newly termed “behavioral” science who took up the call for reform needed to rely on some of the more arcane and problematic tricks in the positivist bag, for example, ideas about theoretical entities imported from philosophical reflections on the unobservable theoretical entities of microphysics. The contexts in which the positivist account of theoretical entities originated involved relationships between the entities and the measurements that indicated their existence; or, what was the same, the theoretical necessity or convenience of employing the entities was bound in a web of strict laws or idealizations that took that form. Lengthy discussions ensued about such topics as Craig’s theorem. The odd objects that made up the microphysical world fit this model.

Hempel’s own comments on these entities is emblematic of a deep problem. Hempel considers what he labels “the theorietician’s dilemma,” a puzzle that arises from the possibility of replacing theoretical terms by surrogates that involve only observables. The “dilemma” is this: “If the terms and principles of a theory serve their purpose they are unnecessary . . . ; and if they do not serve their purpose they are surely unnecessary. But, given any theory, its terms and principles either serve the purpose or they do not. Hence, the terms and principles of any theory are unnecessary.” (Hempel [1958] 1965:186). Hempel goes on to reject the dilemma on the grounds that establishing deductive connections between observables is not the sole purpose of theories: theoretical concepts may enable greater simplicity of theoretical formulation, and may be more fruitful than formulations without theoretical concepts. His discussion of this issue, interestingly,
is elaborated in terms of psychological concepts, and the influential formulations of the philosopher Gustav Bergmann and the psychologist Kenneth Spence.

The ironic consequences of this approach can be seen in the work of the social psychologist Donald Campbell. Psychological entities, such as attitudes, had to be inferred from rough statistical material produced by difficult to interpret experiments that seldom produced quantitatively close results of the sort that could be idealized into laws. The problems produced were given clever solutions, and led to an experimental tradition of great richness and subtlety. In this sense the theoretical terms were “fruitful.” As “science,” however, this was a fiasco—the extent that the entities behaved regularly was so limited that they could not be usefully theorized about, and fundamental approaches could not be decided among. The project of attitude theory faltered, reduced to tautologies like “attitudes have behavioral effects if they are salient, and attitudes are shown to be salient by the fact that they have effects.” The clues offered by positivism to the mystery of how to become a science led to a large body of practical activity, but not to successful theory.

The exponents of wholesale reform did nothing to alleviate the mismatch between the realities of experiment and social and historical research and the gold standard of physics-like explanatory theory. One need only consider Hempel’s classic exposition, “The function of general laws in history” (1942). Here Hempel notoriously urges a quite abstract formal model for social science practice, a model which was not then, nor ever has been, applicable to that practice. The tensions did not go unnoticed. There were a good many dissenters to the project of the scientification of social knowledge even within the scientific community. A notable example here is Percy Bridgman. Bridgman, a distinguished physicist and the inventor of the concept of “operational definition,” a concept that psychologists and behavioral scientists adopted and popularized, publicly doubted that the social sciences could ever have what he called “significant measurement.” The uneasy relationship between social inquiry and conceptions of science produced numerous and sophisticated dissenters to the proposition that social science could ever be accommodated to the methods believed necessary for scientific status; and to the usual answers to the question of whether it was desirable to imagine social inquiry could have this status. These issues were forcefully raised by F. A. Hayek ([1942–44] 1952) and Karl Popper ([1944–5] 1961) in a manner that now seems prescient, for they focused on economic theory, a topic that positivists had special difficulty assimilating.7

Winch’s Triad

Coeval with these discussions, which reached their peak of informativeness in the 1940s, and within “analytic” philosophy itself, there emerged another line of debate which further complicated the claims to scientific status made in the social
sciences. The problem of human action, and in particular the nature of agency, came to be defined in a debate on the relation between reasons and causes, between law explanations and intentional explanations, which had as its epicenter “ordinary language” philosophers (primarily at Oxford). As we shall see shortly, this prolonged and even obsessive debate had the unintended consequence of moving the problems of the philosophy of social science to an unaccustomed place at the center of general philosophical debate.8

Without question the galvanizing moment is to be found in two major writings by Peter Winch, his book *The Idea of a Social Science* (1958) and the essay “Understanding a primitive society” (1964). Winch offered arguments purporting to show why the core concerns of agency, rationality, and scientific methodology formed a logically inconsistent triad. The source of the inconsistency, Winch maintained, was clear. On his account, a science accommodating agency and the nature of human rationality, like a round square, could be shown to be an impossible object just by explication of the concepts involved. The concept of a science demanded a generalizability of relationships that the idea of a social science could not, in principle, provide. The “in principle” barrier turned out to be the notion of rationality itself, and Winch’s reasoning here transformed the problem by showing the relativistic implication of the appeal to “reason.” For in the case of human relations, Winch maintained, that which determines what counts as rationality is local and cultural. Things without thoughts move to universal rhythms; thinking things do not.9

Winch’s account of the socially variable nature of rationality made that issue decisive to the possibilities regarding the character of social inquiry. Debates about rationality and relativism came to dominate the philosophy of social science. The period 1964–80 marked philosophy of social science’s moment in the sun: its core issues mattered to other, more traditional central areas of philosophy. For a while, traditional problems of epistemology and metaphysics—problems of what we ought to believe and of what—converged around the puzzling anthropological data regarding an obscure African community of witchcraft believers, the Azande. What made the Azande so philosophically problematic was that they appeared to reason in ways that were “irrational” by our lights but which were nevertheless entirely functional and unproblematic within the context of their own form of life.

The significance of Winch’s “Wittgensteinian” position in the debates over rationality was that, like Quine’s earlier attack on and erasure of the analytic–synthetic distinction, Winch challenged the usual epistemic categories for identifying certainties. Categories thought to be certain *a priori* become, on Winch’s handling of rationality, only relatively so, that is, given their location within the intellectual motifs of a given society. He granted to Azande thought a kind of logical primitiveness of the sort hitherto reserved for the absolute presuppositions of metaphysics.

Winch’s reasoning is worth articulating in some detail. Edward Evans-Pritchard identified “contradictions” in Azande reasoning (e.g., their views on the heritability of witch-substance implied that almost everyone should be a witch, a view they
the nature of agency, reasons and causes, had as its epicenter, we shall see shortly, a consequence of an unaccustomed in two major writings (1958) and the essay purporting consistent methodology, Winch agency and the be shown to be an. The concept of the idea of a social turn out to transformed the al to "reason." For the determines what thoughts move to

nonetheless denied). Winch's view implies that Evans-Pritchard pushes Azande reasoning in directions it does not naturally go and therefore constitutes a misunderstanding of Azande reasoning. (Other problems included whether the Azande belief in witchcraft and oracles counted as prescientific or nonscientific, in the sense that they could not be countered by experience.)

The debate's deep significance lies in the fact that it seems to rule out the idea that, at least with respect to the basic inferential patterns of a culture, there could be any such thing as a mistake, or falsity, or irrationality. A "mistake" is a concept relative to the rules pertaining to concept-use in that society. The "rules" of concept-use in the culture determined whether or not an application is correct. The basic inferential patterns of the society define for that society what rationality is. But if "irrationality" too is relative to the rules, a clear implication would be to locate relativism at the fundamental level of the a priori conditions of reasoning.

There is nothing more fundamental. And this implicitly put an end to a certain conception of "analytic" philosophy in which analysis could provide a replacement for traditional epistemology and metaphysics by analyzing linguistic usage. Analyzing the language of the Azande led, not as G. E. Moore had expected, to the vindication of common sense, but straight to an epistemology of poison oracles and a metaphysics of witches.

The Legitimation of "Continental" Philosophy

The affinities between Winch's progressively more radical arguments and "continental" philosophy were apparent very early. Jürgen Habermas discussed them in On the Logic of the Social Sciences, originally published in 1967 (1988, esp.127–30, 135–7). One affinity arose between the idea that reasons were not causes and the idea, promoted by such turn of the century neo-Kantian figures as Wilhelm Dilthey and Heinrich Rickert, that the explanations of the Geisteswissenschaften were of a fundamentally different type from those of the Naturwissenschaften. But there was an even more powerful affinity to the neo-Kantian account of fundamental categories.

Consider the famous discussion at the origins of neo-Kantianism about the material and spiritual hypotheses in psychology (Fisher [1866] 1976:22), which is in some respects a simulacrum of the fundamental question of whether the social sciences can be sciences. One hypothesis holds that human psychology is purely a matter of material processes, the other hypothesizes a human soul. What could decide between these approaches?

The issue here, put in Quinean terms, is whether there is a fact of the matter between the two hypotheses. The neo-Kantians' response was that there is no fact of the matter, but they did not conclude so much the worse for the soul. Rather, the view was that there was no rational ground for deciding between the two. What constituted the factual or determined the facts of human psychology could
be determined in two different ways, consistent with each competing and incompatible hypothesis. The “facts” of psychology were theory-impregnated already, and therefore could not constitute evidence for the theories of which they were a part. It was simply an illusion to think that there were in some sense independent facts, or alternatively independent facts about the world on which reason operates directly.

This neo-Kantian line of argument posed the key problem around which “continental philosophy” developed. Edmund Husserl tried to solve it by attempting to return to a core basic level of “things themselves.” The failure of this project led to the recognition in, for example, Martin Heidegger and Karl Jaspers, that no fundamental project, no project of establishing absolute presuppositions, could succeed. The Frankfurt School tried a different, Hegelian, approach: to see the succession of foundations as part of a larger historical project, which was to provide, at the end, intelligibility and emancipation from the false consciousness that previous foundations represented. Another path from this failure was to treat the process of interpretation, in which presuppositions are made and then revised, as the fundamental basis of knowledge. In “hermeneutical” approaches the a priori remained as a condition of interpretation open to revision.

One cannot ignore here the extraordinary success and influence of Thomas Kuhn’s The Structure of Scientific Revolutions ([1962] 1996). By virtue of his much discussed notion of paradigms and how they operate within scientific communities, Kuhn recast the history of science itself as the succession of a priori assumptions guiding historically limited communities of scientists. The effect of this reasoning was to cancel the exemption of the internal development of natural science — tacitly accepted even by “continental” philosophy — from the problem of fundamental premises. And this in turn opened the door to a variety of relativisms based on the notion that differences of belief — which philosophy traditionally accounted for by rational considerations, such as new data, or the correction of erroneous beliefs — were the result of the different fundamental premises of different historical communities. In time, postmodernists argued that traditional notions of truth and meaning could only be an expression of a kind of tribal loyalty to one’s own community’s taken-for-granted standards (cf. Fish 1989, 1995); deconstructionists that these were conditions doomed to be concealed from their readers as a condition of understanding; and feminists that they were irremediably gendered and embodied, and traditional notions of truth and meaning were expressions of the limited understanding allowed by a particular standpoint within gendered society.

**Enter Davidson**

“Analytic” philosophy took a different turn. Through the debate over rationality and relativism in the 1970s and early 1980s, philosophy of social science had, as
we have suggested, posed a key challenge to mainstream analytic philosophy. Yet the challenge from the margins of philosophy to the core was eventually eclipsed by work on these very issues of rationality which occurred in the core of philosophy, notably Donald Davidson’s “The very idea of a conceptual scheme” ([1974] 1984). Richard Rorty recently described Davidson’s essay as “a paper which still strikes me as epoch-making. It will, I think, be ranked with ‘Two Dogmas of Empiricism’ and ‘Empiricism and the Philosophy of Mind’ as one of the turning-points in the history of analytic philosophy” (1999:575). Among other things, this essay normalized the notion that translating as rational is prior to judging of rationality:

seeing rationality in others is a matter of recognizing our own norms of rationality in their speech and behavior. These norms include the norms of logical consistency, of action in reasonable accord with essential or basic interests of the agent, and the acceptance of views that are sensible in the light of evidence. (Davidson 1999:600)

At the same time, it muted the radically relativistic implications of this idea by insisting that the beliefs of the “Others” we are interpreting must be largely the same as ours: “. . . we cannot take even a first step towards interpretation without knowing or assuming a great deal about the speaker’s beliefs. Since knowledge of beliefs comes only with the ability to interpret words, the only possibility at the start is to assume general agreement on beliefs” (Davidson 1984:196). A genuinely radical (but still comprehensible) alternative conceptual scheme was an impossibility, precisely because it would violate this precondition of comprehensibility. This led to the end of the dispute that had centered on the question of whether there was a single standard of rationality or a core of rationality common to all cultures. Davidson’s (holistic and Quinean) point was that neither was necessary, and that the same considerations that led to the problem – the primary translation as rational over evaluation of rationality – excluded the possibility of the kind of radical “living in a parallel universe” relativism embraced by some of the more exuberant interpreters of Kuhn.

Largely as a consequence of Davidson’s paper, issues that began the rationality dispute lost the sharp focus which debate on the anthropological cases had provided, and thus the close connection to cases peculiar to philosophy of social science. Philosophy of social science returned to a pastoral obscurity not unlike that of the Azande themselves. Yet the initial problems concerning the consistency of the Winchian triad did not go away. Two new sets of problems emerged: one with respect to relativism, the other with respect to intentional explanations of a particular kind, rational choice explanations, which we will consider in the next section.

Issues of relativism remain even if one accepts Davidson’s claim that one must accept most of the beliefs of those we interpret as true in order to interpret them at all. For it is far from evident that this constraint does not exclude, but might actually warrant, relativism with respect to much of social science or social theory,
or indeed of social standpoints. The problems became evident with the rise of feminist epistemology, which in some versions argued that there is a special standpoint that was epistemologically privileged by virtue of the fact that it is not bound up with the assumptions of dominant groups and could thus enable its possessors to see what the dominant groups could not.

This was a possibility wholly consistent with Davidson's formulations: it did not need to represent a "parallel universe" relativism of the sort his arguments excluded. To say that the master shared beliefs about maps and floors with the maid is to say nothing about the beliefs that form their basic perceptions of one another, the social relationship they have, and consequently the experiences that would underwrite their theories of the world. To the extent that these understandings of the social world are themselves "social theories," and the experiences in question are the basis of social knowledge, the problems that earlier philosophy of social science as "science" had failed to solve, problems about what counts as good reasoning, are still there to be solved. Moreover, they are there in the problematic form of potential relativistic circularity: are the criteria of evaluation themselves a matter of one's "standpoint"?

Rational Choice: The Scientization of the Intentional

In the early days of the reasons and causes debates, there was an unresolved puzzle about what sorts of things "reasons" explanations were. On the one hand, they were typically taken to be fully sufficient—and particularly not to be in need of further explanation, such as a causal explanation which connected the reason as a motivation with an action that was its effect. Indeed, much of the literature focused on the argument that the connections between reasons and causes was of a different kind, "conceptual," and therefore noncausal, or examples of what Aristotle called "practical syllogisms." But explaining what this meant raised insoluble problems of objectivity. The practical syllogisms taken from Aristotle depended on beliefs, such as "dry food conduces to good health," that were local, connected to long supplanted categories of Ancient Greek thought and cuisine. These local beliefs were "relative" in at least three senses: they explained action only for those who shared these beliefs, and counted as explanations only for those who shared them, and they were true and fully intelligible only to those who shared them. Winch had bitten the bullet and acknowledged this: the conceptual necessities that replaced the causal necessities of action explanation were local, and the job of the social scientist was to explicate these local concepts.

Yet only two decades after these problems over objectivity had apparently proven fatal, we find such claims as these: rational choice accounts not only offer a form of interpretation, but also a form "which lets us make objective yet interpretive sense of social life" (Hollis 1987:7). Such accounts were, on this
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What changed? In the first place, the subject changed. The endpoint of explanation was no longer a “reason” whose objective validity was in question, but a decision, whose rationality was taken for granted, but which needed to be explicated. Successful explication was successful explanation. The form of the explanation is still teleological and intentional. But the beliefs that go into the reasons are not, like beliefs about “dry food,” local and true only in a relative way. Explaining in terms of rational choice opens up the possibility (indeed requires) that the beliefs themselves be accounted for in the same manner, that is, as rational choices. If beliefs themselves can be accounted for in this way, then even the rationality of natural science finds explanation in these terms. Explaining the rational preferability of science, on this rather startling view, requires no essential appeal to a purely “scientific” method, and no appeal to epistemology, realism, and the rest of the traditional philosophical justification of science.10

But the more thoroughgoing the reduction to rational choice, the stranger the results, and the more perplexing the question of what sort of thing this endpoint is. How can there be an endpoint of explanation that is not itself grounded in nature — as traditional teleological arguments, however defective, were? Is rationality an unmoved mover? And if it is not a “mover” or disposition at all, can its role in explanation be other than purely formal — perhaps a formal redescription of events that have a “real” but different causal explanation at a different level of description, such as the cognitive or the evolutionary biological? If we think of the axioms of rational choice in a Quinean way, as particularly basic and therefore unlikely to be revised parts of our theory of the world, don’t they lose their objectivity, and become relative again, this time to the pragmatic purposes that our theory of the world satisfies, and doesn’t this suggest that rational choice is merely another methodological perspective?

Philosophy of Social Science Today

The question of whether rational choice analysis requires a further foundation points to the continued importance of the traditional problem of the scientific character of the social sciences. It arises today in novel forms. Winch’s concerns with the social embeddedness of notions of agency and rationality were directed at models of social science from the past — notably Mill’s in A System of Logic
(1843), and Max Weber interpreted in the light of Mill—which attempted to compete with or supplant ordinary reasons explanations. The models were of nomological explanations, or Humean causation. Both are largely irrelevant arguments, given the form of present social and natural scientific explanations.

Structural equation models, which constitute a goodly portion of empirical social science, and employs basically the same methods as those used in such “natural sciences” domains as biostatistics and on such topics as global warming, seem to stand on their own. They do not bring in their train old questions about the irreducibility of domains of explanation because they do not take the form of laws and do not depend on laws, and yet are capable of producing secure causal results with a minimum of background knowledge.

But the problems of compatibility do not disappear. Instead they proliferate. What is the relationship between these models and other theories and the data produced by other methods? Is there reason to think that biological and statistical models can be squared with deeply held views about agency and rationality, or with the results of cognitive science? Can the supposed accounts of microfoundations of behavior be squared with regularities noted at the macrolevel? These problems of compatibility are sufficiently complex and intractable to make one nostalgic for the older but better formed problem of reasons and causes.\textsuperscript{11} The \textit{Rationalitätstreffen} of the 1960s and 1970s, similarly, has transmuted rather than disappeared. Rational choice models replaced positivist characterizations of rationality, colonizing key areas of philosophy just as they have the social sciences. Ethics, epistemology, and philosophy of science all draw on their presumed explanatory power. Once again, a model of rationality originating outside of philosophy drives the discussion of issues at the center of the discipline, and it is a model that pervades and transforms the topics to which it can be applied.

Philosophy of social science today may best be thought of as concerned with novel issues of compatibility between arguments generated within its powerful problem traditions. The Winchian claims of logical incompatibility, of a conceptual inability to do intellectual justice simultaneously to agency, rationality, and scientific methodology, have not been resolved. But new issues regarding how to characterize what is rational, and how such characterizations affect the scientific study of the social, have become more pressing. Among these are: conflicts on what to call rational (see Harding’s chapter 13), efforts to incorporate the natural sciences with the purview of what social scientists may explain (see Fuller chapter 9), uses of generic notions of rationality to explain decisions in all areas of human activity (see Rawlings and Udelin chapters 5 and 6), subsuming apparently intentional actions to species of biological explanation (see Nelson’s chapter 11).

There is an underlying theme to these issues of compatibility. When Quine articulated his vision of a naturalized epistemology, he controversially urged that we understand this relation of rationality and science as one of “reciprocal containment,” though, he added, “containment in different senses: epistemology
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in natural science and natural science in epistemology" (1969:83). Epistemology contains science insofar as epistemology is a study of the logic by which we build our theories of the world from the data available to us. But science contains epistemology to the extent that our account of how creatures like us turn this trick is tied to studies of how we develop under the stimuli to which we are exposed. The line between philosophy and science blurs.

Philosophy of social science today is primarily concerned with the implications of this blurring. Social science represents, philosophically, a form of “real existing naturalism,” explaining or purporting to explain subjects that philosophers traditionally believed to be explicable within the province of reason alone. Today, the challenges typically relate to “normativity,” which is understood to be that which stands beyond the reach of naturalistic explanation. And behind many of the issues are questions that arise with naturalism generally – does it conflate or confuse the normative and descriptive enterprise? Is the social phenomenon of normativity something unrelated to what philosophers call “normative” and if so what does this imply for social explanation itself? Is there something normative beyond the naturalistic that interacts with the causal world? Or is normativity in the “philosophical” sense another ghost in the machine? With questions such as these, the issues at the core of philosophy of social science once again move to philosophical center stage. For it is in competing conceptions of social science – is what we value simply part of the explanans, or is it an explanandum – that such debates are played out. Perhaps history here can run in reverse, at least in the following way. Just as the issues raised by anthropological claims became lost in more general debates about rationality, perhaps debates on the sources of normativity can be given sharper form by looking more closely at competing explanations of their social origins and character. Or perhaps with normativity we reach an incompatibility as profound as mind and body.

Notes

1 See, for example, the chapters in this volume by Hankinson, Humphreys, Rawlings, and Udeshi for discussions of how explanatory models in the social sciences either stand free of concerns regarding what defines scientific method or how social scientists have worked to appropriate natural science (e.g., biological theories) for their own ends. Fuller and Harding in their chapters discuss challenges to the presumption that natural science is the royal road to explanation of social phenomena. Finally, although not discussed here, there is an interesting debate concerning the natural sciences as a “natural kind” (Rorty’s phrase). See Dupre (1993), Galison and Stump (1996), and Rosenberg (1994).

2 As is evident in the collection of essays on “methodology” before and after Comte, published by Larry Laudan as Science and Hypothesis (1981).

3 Ironically, the positivist in this debate, Comte, rejected statistics. Herschel said this of the statistician Quetelet:
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What astronomical records or meteorological registers are to a rational explanation of the movements of the planets or of the atmosphere, statistical returns are to social and political philosophy. They assign, at determinate intervals, the numerical values of the variables which form the subject matter of its reasonings, or at least of such “functions” of them as are accessible to direct observation; which it is the business of sound theory so to analyze or to combine as to educe from them those deeper-seated elements which enter into the expression of general laws. (Herschel 1850:22)

4 See especially in this regard the chapters by Roth and Turner in this volume.

5 For example, in his book Constructing the Subject: Historical Origins of Psychological Research (1990) Kurt Danziger treats the subject matter of academic psychology and the discipline itself as fundamentally the product of an erroneous conception of science and method.

6 The methodological manifestos of the logical positivists coincided with a time when the social sciences themselves found these questions particularly salient. Consequently, a number of philosophers and philosophically inclined social scientists found opportunity in this period for unusually close, intense, and fruitful interaction. “Positivist” social scientists such as George Lundberg were among the most supportive academic patrons of the Vienna circle as it re-established itself in the United States. Psychologists worked with Gustav Bergmann and revered Herbert Feigl.

Indeed, the history of the movement itself might have been quite different without some pre-existing affinities in the social sciences on which it was able to draw. For example, Karl Pearson’s The Grammar of Science ([1892] 1911) provided one of the crucial historical links between Comte and the logical positivists themselves. The tide of Nazism that swept logical positivists to the shores of the United States and Britain brought them in contact with its native social science positivism. Yet this tide also brought to Anglo-American attention the work of Popper and Hayek, and with them a strong stream of antiscientism originating in a certain conception of economics and economic life that opposed the idea of scientific planning and the scientifically organized reconstruction of social life. Thus, ingrained into some philosophy of science as it made itself felt in Anglo-American culture, was a skepticism about combining social inquiry and science. The admixture of the social and the scientific proved combustible everywhere they were found together, as they were in the 1940s, producing such texts as Lundberg’s Can Science Save Us? (1961), Hayek’s The Counter-Revolution of Science (1952), Morgenthau’s Scientific Man vs. Power Politics (1946), and, most influentially and importantly, Popper’s The Poverty of Historicism ([1944–5] 1961).

Philosophical issues of agency and rational choice disappeared when American social science, fueled by a tremendous infusion of foundation money in the postwar period, embarked on a massive attempt to create for the first time a genuine “behavioral science.” The stage was then set by the 1950s for searching discussions regarding the problem of constructing social scientific knowledge. The philosophical literature about the philosophy of social sciences that emerged in the 1960s drew on a literature from the 1940s and 1950s that was a product of the earlier interactions.

7 And continue to have difficulty with (cf. Rosenberg 1992).

8 On yet another front, much of the heady atmosphere of contention of the 1950s took on a striking and new political polarity within the student movement of the 1960s.
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For positivism, despite its development by thinkers of liberal and leftist views (Ayer, Schlick, Neurath, and even the young Wittgenstein), became typed as politically reactionary. This formulation reflected Popper’s vehement animus towards all notions of social engineering and the concomitant reluctance to assign to social science the ability to make policy prescriptions. This nonprescriptive view lay at the core of the Methodenskizzen, a much discussed and open conflict between Popper and his tribe and the by then elderly theorists of the first generation of the Frankfurt School, notably Max Horkheimer and Theodor Adorno. The exchanges functioned more as opportunities for mutual condemnation than a reasoned exploration of differences. But here too the philosophy of social science was the active front in the growing conflict between continental and analytic philosophy (but before even these characterizations became standard usage).

The problem followed, ironically, quite directly from the way that ordinary language philosophers had made their case against causal explanation. If, for example, explanations of action were to be construed as explanations of practical syllogisms rather than laws, it was evident from the standard examples (“Dry food suits any human; such-and-such food is dry; I am human; this is a bit of such-and-such food; yielding the conclusion: This food suits me” Anscombe [1957] 1963:58) were simply false – or at best “rational” in terms of the local criterion of rationality and the particular cosmology. And this posed the problem of relativism: if agents believed in witchcraft and “explained” their own actions in terms of practical syllogisms about witches, did this also count as explanation for us?


In retrospect, the intellectual shifts in the philosophy of social science problematic vindicate Weber. Weber was regarded in the 1960s by writers such as Winch and MacIntyre as an interpretivist entangled in a naively logical error. The error was rooted in his supposed attempts to apply the causal methods of Hume to the explanation of human action under the illusion that this would create a science free of the conceptual demands of generalizing natural science. Weber, however, had in fact grasped this problem, arguing that social science needed to rely on a probabilistic sense of causality appropriate to situations in which the causal categories are preconstituted, as occurs in the case of judgments about legal liability, and not to apply nonnomological or Humean concepts of causality. He also posed the crucial problems of the relationship between all three sides of the triangle of action descriptions and the culturally relative constitution of the subject matter of the social sciences, and provided an answer: to be intelligible to a given cultural audience, actions needed to be described in terms of ideal-typifications which were understood by that audience. Some typifications, such as that of instrumental rationality, that is, rational choice, were more readily applied transculturally than others, which were more local. But nothing about this type made it uniquely valid or applicable. It was only a contingent historical fact, which he thought could be explained, that it was particularly applicable in certain times, and there was no assurance that in the future it would not only cease to be applicable and even cease to be comprehensible to future audiences, just as former typifications are no longer intelligible to us.
References


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