3

The Philosophy of Social Science in the Twentieth Century: Analytic Traditions: Reflections on the Rationalitätstreit

Paul Roth

> My point is that if we are intelligibly to attribute attitudes and beliefs, or usefully to describe motions as behaviour, then we are committed to finding, in the pattern of behaviour, belief, and desire, a large degree of rationality and consistency … .The limit thus placed on the social sciences is set not by nature, but by us when we decide to view men as rational agents with goals and purposes, and as subject to moral evaluation.
>
> (Donald Davidson, 'Psychology as Philosophy')

> I wish to say: nothing shows our kinship to those savages better than the fact that Frazer has at hand a word as familiar to us a 'ghost' or 'shade' to describe the way these people look at things. … A whole mythology is deposited in our language.
>
> (Ludwig Wittgenstein (1979) *Remarks on Frazer's Golden Bough*)

Broadly speaking, questions of how to explain human action can be answered in one of two conflicting ways, each of which argues for its priority over the other. On the one hand, the epigraphs by Davidson and Wittgenstein each incorporates, in its own way, a common and powerful argument that maintains that any accounting of purposive behavior requires rationalizing it. Rationalizing explanations must in turn proceed by domesticating actions via translation into an intentional or evaluative idiom familiar to the "translator"/ explainer. Rationalizing actions just means *inter alia* providing *good* reasons that is, reasons that motivate individuals to act as observed. What makes an explanation of action *social*, moreover, will involve how people's reasons take into account the doings of others. This argument forges together the notions of action explanation and of agent rationality. The former cannot be had without the latter.

Yet the grip exercised by a requirement that a normatively inflected vocabulary must be employed when explaining social actions only serves to anchor one part of a fundamental philosophical dilemma. For, on the other hand, insofar as the notion of a social *science* exerts a hold, explanations will be bound to a requirement to discern law-like regularities that can be exploited for purposes of prediction and control. But statements incorporating normative idioms have not proven amenable to being regimented in this fashion.

The relevant notion of scientific rationality thus proves antithetical to what makes ordinary action explanations rational – providing good reasons in an agent's sense. This creates the dilemma: if of the social, then explanations cannot satisfy the standard that scientific rationality requires; and if of a science, then explanations cannot incorporate what the rationalizing of social action requires – the use of an intentional or evaluative vocabulary relevant to agency. The *Rationalitätstreit* arises from this dilemma. What counts as rational by way of ordinary explanation of social action cannot satisfy what counts as rational by way of scientific explanation.

This dilemma continues to haunt philosophical discussions of the social sciences. Yet, surprisingly little discussion tracks how an altered philosophical understanding of science comports with the dilemma as formulated. And especially within the analytic tradition, much has changed with regard to how to understand the term "science," and so what this implies with regard to related explanatory endeavors.

Does the dilemma still obtain? By exposing and examining those philosophical premises used to legitimate and so perpetuate the conceptual divide, reasons for rejecting these assumptions can be recalled. This will allow for dissolution of the dilemma. For as formulated, the dilemma in the context of the philosophy of social science imposes a forced choice of explanatory strategies or vocabularies. But what anchors each end of the dilemma depends, or so I shall argue, on a particular metaphysics about the objects of knowledge and notions of rationality and explanation tailored to the respective metaphysics. Put another way, once the underlying metaphysics goes, so goes any claim for there being a preferred meta-method or explanatory strategy. Philosophical reasons for mandating a forced choice of explanatory strategies disappear.

The core dilemma noted above has bedeviled analytic philosophy of social sciences from its inception. A review of the history and origins of this debate, at least as it has developed over the last century, suggests a type of rational reconstruction of analytic philosophy of social science in terms of this particular issue. Seeing it from the perspective of how the debate originates in the late-nineteenth century reveals that differing explications of rationality map onto incongruent conceptions of what I shall term "the objects of understanding." (Roth, 2000) Assumptions about these objects of understanding constitute the traditional metaphysics of knowledge that weighs like a nightmare on the philosophy of social science.

A distinction emerges late in the nineteenth century between nomothetic as opposed to idiographic accounts of human behavior. A nomothetic view presumes that laws characteristic of explanation in the natural sciences also must serve as the engines of explanations in the social sciences as well. (As discussed below, logical positivism provides what becomes for all intents and purposes the canonical account of the logic of explanation. See Salmon (1989) for an excellent overview.) An idiographic account of human actions insists that the human sciences account for meaningful behavior in terms of what humans could recognize as good reasons for that behavior. What determines the goodness of reasons depends, in turn, on citing factors specific to the time and place as explanatory of that action. But such contextualized reasons preclude generalization because they are context specific.

The appeal to "good reasons" or motivations connects idiographic accounts, moreover, directly to a precursor of the dilemma that concerns us, viz., the venerable Humean dichotomy between descriptive and evaluative statements. This dichotomy configures the problem as the analytic tradition inherits it. As enshrined in the philosophical literature, the Humean is/ought distinction maps onto the view that scientific regularities can only traffic in relations of fact. But any evaluative statement transcends what a description of mere facts provides. Since no evaluative statement can be derived from descriptive ones alone, *a fortiori* no account of "good reasons" can be derived from any bare recounting of facts.

Put another way, any sense of cause that "good reasons" supply people for their actions cannot be a type of cause that a science could harness. For reason–action connections so described cannot translate *under that description* into some parallel regularity between successive physical states of an object. Thus the hoary distinction between the nomothetic and the idiographic lingers on, seemingly intrinsic (as the opening quote from Davidson insists) to any effort to explain human action.

Interestingly and importantly (since the analytic tradition reproduces this part of the original debate as well), contextualization was not for historicists such as Dilthey any bar to objective knowledge of historically specific situations (see Apel, 1984: 3–6). When desiring to learn how those not like them understood their natural environment and social relations, historicists and their heirs also assumed that there exists a determinate object of understanding at which their inquiry aims. This object consists of a stable, shared set of meanings about that social sphere, a "Rankean reality" knowable *wie es eigentlich gewesen* (Iggers, 1983: 133–40). The fact that people successfully communicate seemingly establishes the *prima facie* legitimacy of assumptions regarding shared and stable meanings. Because languages alter and vary across times and cultures, there exists as well a need to contextualize assessments of such shared meanings.

Each approach – the nomothetic and the idiographic – claims to be a science. Each styles itself as a source of objective knowledge and as possessing its own distinct associated method of systematic inquiry. Each has its own specific object in view, and a method tailored to investigating and so learning truths about its object. The "objects of understanding" for each realm (the natural and the social) rationalize the methods of inquiry specific to that realm. The fit of method to object and the claim to deliver truths about the realm under investigation entitles each to claim the title of a science, overt differences of form notwithstanding.

The analytic tradition puts its own mark going forward, however, on the form that the older debate takes. On the one hand, the Humean line just rehearsed incorporates a view that runs from Bertrand Russell through logical positivism and out to contemporary philosophies of science and epistemologies that claim to be forms of naturalism. For in this tradition, natural science sets the standard for explanation, and intentional idioms receive respect only insofar as they abet or conform to programs of regimenting experience to scientific standards. Programs here run the gamut from reductionist approaches to evaluative idioms through to those that hold that such seemingly intractable idioms have their own (tractable) theoretical structure.

On the other hand, the analytic tradition also remains heir to thinkers such as G.E. Moore, J.L. Austin, and various thinkers who claim the legacy of the later Wittgenstein. This second strain within the analytic tradition takes seriously accounts of action that invest with *prima facie* significance ordinary language talk of reasons as causes. For thinkers in this branch of the analytic tradition, ordinary intentional talk must be respected, not purged, when it comes to action explanations. This carries forward to contemporary debates concerning, for example, collective intentionality, and its role in explanations of social behavior (influential works here include Gilbert, 1989; Searle, 1995). The competing camps within the analytic tradition – friends of naturalism versus friends of ordinary language – only recapitulate the traditional divide in somewhat different dress.

These dual lines of descent further help perpetuate seemingly irresolvable disputes regarding what an explanation of the social requires. Put another way, debates in analytic philosophy of social science regarding rationality tie to questions of how a posited object of inquiry rationalizes and so legitimizes particular forms or vocabularies of explanations. In these respects at least – the differing logics of scientific explanation and of purposive explanation as well as the associated metaphysics of objects of knowledge for each – the analytic tradition in the philosophy of social science inherits nineteenth century philosophical presuppositions. These underlie distinctions made between the *Geisteswissenschaften* and the *Naturwissenschaften*. Each engenders competing and incompatible explications of terms such as "rationality" and "explanation." They also continue to fuel the *Rationalitätstreit* into the twenty-first century. With the advantages of hindsight, competing views about rationality can be reconstructed as competing views regarding the object of explanation.

The rise of logical positivism and their calls for a "unity of method" made a profound mark on the social sciences through the early 1960s in part because of the absence of any methodological consensus on the interpretative side (Novick, 1988). Problems that attended Dilthey's neo-Kantian effort to mount a "critique of historical reason" have been told well by others (see Apel, 1984; Iggers, 1983). For present purposes, it suffices to note that a core problem remains through many iterations and variations on the interpretative position. In order to preserve their distinctive "space of reasons," the space so imagined must not exist as any proper part within some realm of physical facts. For then such facts would simply be ones to which some natural scientific method applies. But by removing the space of reasons to a realm of non-physical facts, a slippery slope to radical relativism or historicism invariably ensues. For protestations of determinacy notwithstanding, nothing emerges in the process of inquiry on which to anchor attributions of meaning in a space so imagined.

Ultimately, historicism preserves the sphere of understanding at the price of sacrificing claims to being a science. When confronted with a philosophical theory such as logical positivism that spoke with authority regarding what "real" science requires, interpretivists were not convinced. But they did not possess any plausible alternative account of their own method that had the precision and the clarity of the criteria promoted by logical positivism.

Especially in the context of post-World War II social science in the US, an image of logical positivism could be identified as influential and important because it provided a characterization of the fundamental desiderata of scientific theorizing. "[First,] its deductive-nomological account of explanation and concomitant modified Humean interpretation of 'cause'; second, its belief in a neutral observation language as the proper foundation of knowledge; third, its value-free ideal of scientific knowledge; and fourth, its belief in the methodological unity of the sciences" (Fay, 1975: 13). The unity of method thesis counted against, for example, incorporating terms or concepts into a science that could not ultimately be reconciled with the framework of physics.

Hempel notes that Otto Neurath, the member of the Vienna Circle to write most extensively on the social sciences, "put mentalistic terms such as 'mind' and 'motive' on his *Index* [of proscribed terms] on the grounds that they tended to be construed as standing for immaterial agencies and that this kind of reification gave rise to much stultifying perplexity concerning the relation of those mental agencies to the physical world" (Hempel, 1969: 169). Hempel goes on to remark that, in this respect at least, Neurath's position prefigures later philosophical efforts such as Gilbert Ryle's (1949) influential *The Concept of Mind*. Indeed, Hempel himself insists that

> all branches of empirical science test and support their statements in basically the same manner, namely by deriving from them implications that can be checked intersubjectively and by performing for those implications the appropriate experimental or observational tests. This, the unity of

> method thesis holds, is true also of psychology and the social and historical sciences.
>
> (Hempel, 1969: 191)

Hempel's own reflection on the methodological unity thesis thus too explicitly rejects, even through this late formulation of the position, all appeals to introspection or empathy. Yet as also Hempel observes, Neurath softened over time regarding how to interpret the demand for "unity of method," moving from a doctrinaire physicalism to a more pragmatic and instrumentalist view. This easing did not signal some new found appreciation of the scientific legitimacy of the intentional. Rather, as difficulties arose from strict demands for a reduction to some form of observational base for concepts even in the natural sciences, other bases were recognized for legitimating theoretical entities whose foundation in observation could not be readily established.

As American social science imbibes the unity of method thesis, this results in what Bernstein terms a form of naturalism, one most notable for proscribing any evaluative conclusions from its scientific work. Contrary to Marx's famous injunction, a social scientist's job *qua* social scientist "is to interpret the world not to change it; he interprets it by offering and testing theoretical explanations. He knows … that if one is seriously interested in 'changing the world,' this can best be accomplished through scientific knowledge" (Bernstein, 1978: 44).

In this context where the political and methodological agendas of social science meet, the import and influence of Sir Karl Popper's work demands mention (particularly Popper, 1957). An interesting, influential, and certainly at the time important controversy closely related to topics discussed in this essay can be found in (Adorno, 1976). For unlike the discussions sketched below that emanate from the work of Peter Winch, the so-called *Methodenstreit* explicitly features both methodological and political agendas. On the one hand, Popper's falsificationist approach made an important and enduring impact on conceptions of scientific method contained in the social scientific literature. On the other hand, however, Popper's key methodological precept of falsificationism fades in significance in the philosophy of science literature. It met the same fate that verificationism did at the hands of (Kuhn, 1996). Neither provides the desired key by which to rationalize actual processes of theory acceptance and theory change.

Upon examination, debates surrounding Popper's works turned out to have less to do with disputes about scientific rationality *per se* and instead become connected to controversies regarding the types of social arrangements that promote open or free inquiry (exemplary here is Horton, 1970). In particular, Popper opposes social scientific approaches that would license large-scale social engineering. Certain philosophers who drew initial inspiration from Popper such as Paul Feyerabend greatly radicalize this thesis by worrying the issue that a petrified notion of scientific method itself will hamper criticism and the growth of knowledge.

Neither the logical positivists nor those inspired to a science of the social under their banner were wedded to any doctrines of political quiescence. The value neutrality of science would be rather exactly what allowed social science, in the view of Carnap, Neurath, and others, to serve political purposes. For, once the facts were in, policy makers could then debate with some objective assurance.

> [Carnap and Morris] also shared, with Dewey and Neurath, a progressive political agenda. They saw a political role for a scientific philosophy that helped with the internationalist and progressive project of unifying scientific knowledge in support of social needs. Indeed, Morris and Carnap both felt that their scientific attitude was inseparable from a politically responsible philosophy.
>
> (Richardson, 2003: 11)

Popper maintains that the demands of scientific rationality mandate a limited policy role for social science; Carnap et al. hoped that the scientific status of social inquiry would prove an aid to designing progressive policies. (See discussion in Richardson 2003) But the critical point that emerges in these discussion concerns not the political agendas of positivists or others, but the inability of social science to reasonably approximate in its research and results the level of logical rigor or production of generalizations demanded by the unity of method thesis.

Although this story of positivism's rise and demise has been told well (Bernstein, 1976: especially Part I; Hempel, 1969), certain aspects related to this story bear emphasis and repeating. Particular note needs to be made with regard to the ways in which Kuhn's work eclipses that of logical positivism as an accepted account of theory acceptance or rejection. Pre-Kuhn, philosophical tradition had it that when taking the object of understanding to be the natural world, scientific method constitutes the rational method of inquiry. Following the logic of scientific inquiry provides a prescriptive procedure by which to obtain knowledge (truths) about the objects in that realm. Post-Kuhn, the philosophical community came to accept the implied irony of Kuhn's title; scientific revolutions had no logical structure.

On the philosophical side, the most decisive criticisms emanate from the positivists themselves. In this regard, Hempel's "The empiricist criteria of cognitive significance: problems and changes" (Hempel, 1965) still proves instructive as to how positivism fails to succeed on its own terms. Hempel details with clarity and precision the logical shortcomings of proposed criteria of meaningfulness in terms of empirical verification. In particular, formulations of the criterion of cognitive significance either regularly exclude statements that would be scientifically acceptable or can be teased into admitting statements that lack empirical significance. Ironically, the very clarity of the logical positivist criteria ultimately made possible the counter-examples that defeat them.

Further, reasons emerge for believing that such a criterion of meaningfulness cannot in principle be formulated. A key point made by (Hempel, 1965), publishing on this matter only a year before Quine's (1953) "Two Dogmas of Empiricism" appears, concerns the fact that statements of a theory cannot be evaluated individually, in isolation from one another. In order to test the truth of any single statement, many statements in that language/theory must be held or assumed true. Within the next few years as well, Sellars develops his critique of the "myth of the given," (Sellars, 1956) arguing in parallel fashion to Quine that the supposed sensory substrata on which the most basic attributions of knowledge supposedly rely also presuppose prior theoretical framing. Thus, the twin pillars on which the autonomy of scientific knowledge supposedly rested – the logical structure that made plain the truth conditions of individual statements and the unvarnished news of the senses (Quine's phrase) that provides the incorrigible evidence – proved to be unable to bear the weight of these claims.

Yet another important underlying assumption here concerns a belief in the cumulative view of scientific knowledge, that is, that changes in theory preserve or add to truths about the *same* objects. However, the arguments that challenged the rationality of theory acceptance and change also challenged the growth of knowledge assumption as well. For as theories change, so do its posits. But with different posits, there turns out to be no way to connect successive theories as theories about the same objects, and so as part of some narrative of accumulation of truths. Absent an account of how truths accumulate despite changes in theory, nothing rationalizes a belief in a generic scientific method. Instead, methods of inquiry and norms of explanation must be tailored to the specifics of the theory they serve (on Kuhn's impact, see Ball, 1976; Stephens, 1973). This defeats expectations that philosophers could hope to identify and explicate generic notions that constitute some core account of scientific rationality, an expectation that motivates much of the attention that logical positivism gives to the workings of scientific practice. Indeed, absent a stable object of inquiry, nothing remains to legitimate a belief even in a generic characterization of the notion of a science (Shapere, 1969: 122–3).

The disappointment on the social science side of this account of scientific rationality proves no less fundamental. Most importantly, what most fueled the sense that positivism constituted a failed program were that attempts to corral the social by means of what were taken to be the methods of science never yield the results expected in terms of either laws of social behavior or predictable results. First and foremost, in short, was "the tremendous disparity between the insistence on what theory is and the failure to actually produce it" (Bernstein, 1978: 52). For example, various theoretical failures to establish the reducibility of non-observables to observables undercut commitments to any strict form of behaviorism or physicalism. The compelling influence of a model of scientific rationality such as logical positivism formulates resided in the thought that adherence to science so outlined explains both the instrumental success and apparent growth of scientific knowledge (Bernstein, 1978: 24). Yet the repeated failures of empirical theory in the positivist mode to deliver on the theoretical promises finally only reinforced arguments in the philosophy of social science to the effect that the study of what made behavior social could not, in principle, be characterized by an empirical science.

But it would be a mistake to conclude on the basis of such disagreements that debates regarding the role of rationality in the social sciences reduces to tired aporias between a hermeneutically grounded *Geisteswissenshaften* and nomologically oriented sciences of the social (see Apel, 1984: Habermas, 1988). What needs fuller appreciation concerns how just these changes regarding a generic logic of science alter the possibility space for a philosophy of social science. This returns us again to considering the parallels between the nineteenth century debates and debates post-positivism on the notion of rationality in analytic philosophy of social science.

The important question to ask, I suggest, is this: once logical positivism falls from philosophical favor as a unifying account of scientific rationality, why does *any* analog to the nineteenth century debate remain? In tracking how the nineteenth century debate comes to be perpetuated post-positivism, Peter Winch's classic work (1958, 1964) represents the most significant guise that it assumes. Winch initiates debate on the implications that understanding the social as "rule governed behavior" has not only for the possibility of a science of the social, but also for how the notion of rationality can plausibly be construed. Ironically, from a study of Winch's work the answer that emerges indicates that it is interpretivists who insist upon and perpetuate the old dichotomy. They do so in order to preserve a particular metaphysical entity, their own unique object of understanding.

With the waning of logical empiricism and the rise of post-Kuhnian philosophy of science, in other words, it becomes too easy to miss a key motivating factor in the earlier debates. Debates about rationality were fueled by thought that some special link existed between, on the one hand, the method of science and, other the other hand, the growth or success of scientific knowledge (see for example Suppe, 1977: especially 659–705). This presumed attunement of objects and methods that can access them highlights an important assumption shared by those who believe, whether in the manner of Hempel or of Winch, that knowledge of the social is possible. For with respect to the terms in which Winch frames the debates and in which those who support or oppose him continue it, no question exists regarding the object of understanding. A methodology of inquiry attuned to that object should yield an accumulation of truths about it.

It comes as no surprise to note that logical positivism presumes its favored notion of rationality to be grounded in and validated by its established utility in fostering the growth of knowledge. More surprising is that this assumption underlies positions that otherwise appear adamantly opposed to the positivist account of scientific rationality as appropriate to the human sciences. But both the methodological unity thesis and the methodological duality thesis emanate from this assumption. For just as Hempel assumes the stability of the objects of explanation in formulating his logic of explanation, so too Winch assumes the stability of the species of internal relations as the objects of social inquiry. Winch explicitly holds that "social relations are expressions of ideas about reality" (Winch, 1958: 23). The study of social behavior, according to Winch, consists in learning the rules by which people constitute social reality. Since rules constitute the social, they must be shared *qua* rules. But since rules do the work of making the social "appear" to people, the study of the social can only be the study of the idea of the social—the concepts applied so that the social has being at all.

A remark by Habermas (on Weber) succinctly voices a still common view and gives good evidence of the persistence of the traditional view noted at the outset. "Social action belongs to the class of intentional actions, which we grasp by reconstructing their meaning. Social facts can be understood in terms of motivations. … Through understanding, I may interpolate a rationally pursued goal as sufficient motivation for an observed behavior." (Habermas, 1988: 11). Philosophers outside the analytic tradition writing on the philosophy of social science, for example, Charles Taylor, also simply do not challenge the alleged adequacy of a logical positivist model of rationality as appropriate to the *natural* sciences. Taylor just insists on inapplicability of these models to the social sciences. And when writing on themes from Winch more than two decades after Winch reinitiates this debate in the analytic tradition, Taylor approvingly asserts, "In the end, there is no way to finesse understanding if we are to give a convincing account of the explanatory significance of our theory." (Taylor, 1981: 196–7). Investing explanations of the social with meaning requires positing some systematic link between what motivates social actors and a method for determining these motivations and so use of the intentional idiom. The "realm of understanding" constitutes a space in which facts of meaning can be discovered. (On this point, see the exchange between Rorty, Dreyfus, and Taylor (Rorty, 1980a, 1980b). For an important work that anticipates the need to move beyond the dilemma, see Turner (1980).)

Winch's writings also prove important to extending the older debate because they contain significant even if unintended parallels to more mainstream philosophical criticisms of logical positivism. For example, although he nowhere cites Hempel's worries as broached in "The empiricist criteria of cognitive significance," Winch makes closely related points. What people take science to be and what they take to be reality cannot be prised apart. The conceptions prove mutually defining. Beliefs cannot be examined one at a time for their scientific goodness, for science does not exists as some neutral criteria for the goodness of belief. Rather it helps shape what people in the first instance take as candidates for belief. The two notions prove mutually influencing and influenced.

Moreover, beliefs do not (as Hempel ruefully acknowledges) stand or fall individually, but their very intelligibility relies on holding true many background beliefs as well. As noted above, Hempel's essay appears in the same time frame as the more critical essays by Quine and Sellars. Their work, of course, turns out to be much more generally influential because each provides an important philosophical critique of foundational accounts of rationality and knowledge within the analytic tradition. Winch's work easily assimilates into debates in "analytic philosophy" because it connects readily to closely related criticisms from within the analytic mainstream.

Importantly, Winch never challenges logical positivism as a model of natural scientific reasoning. And like the historicist tradition linked to Dilthey, he defends a *sui generis* notion of social knowledge. Each sphere – the natural and the social – has its object and its associated method. "On my view then, the philosophy of science will be concerned with the kind of understanding sought and conveyed by the scientist; the philosophy of religion will be concerned with the way in which religion attempts to present an intelligible picture of the world; and so on." (Winch, 1958: 19) Winch offers no hints with regard to how to identify to which sphere any bit of discourse belongs. But that each bit clearly belongs to some sphere he never questions.

For Winch, what disqualifies the social from being studied in the manner of the natural world concerns not the lack of an object, but that the object of social inquiry just has a different ontological status than those studied by the natural sciences. But this ontological difference does not make them less real or preclude having knowledge of about them. In this respect, to learn how rules apply is to learn what normative standards people in a particular society employ.

The Humean dichotomy once again intrudes to hive off the study of the social from the study of the natural. That is, Winch extends the conceptual account of the social developed in (Winch, 1958) to argue in (Winch, 1964) that different societies possess different ways of constituting social reality as well as different ways of licensing inference from within their shared categories. In his influential paper, "Understanding a primitive society," (Winch, 1964), Winch makes his primary target the view that there exists a generic logic of science, and that this logic equates to what it means to be rational. He challenges this generic conception because he maintains that it distorts action explanations. A generic notion of scientific rationality distorts such explanations because it misses (or wrongly dismisses) perfectly good rationales people give for their actions. That is, Winch worries that what people take as good reasons for actions will be rejected as explanations of their own actions when viewed from the standpoint of scientific rationality. Since irrational reasoning cannot explain, this seemingly precludes the possibility that people whose beliefs do not measure to up to our standard of logical consistency must be incapable of rationally accounting for their own actions. Winch took this conclusion to be unacceptable, and argues instead for taking each people's reasons for action as rational by construing standards of rationality as a contextual matter. Winch's position was taken to restrict judgments regarding the rationality of actions to a perspective internal to each group or society.

"Understanding a primitive society" ties in neatly with other criticisms of positivism that precede it by just a few years. Its arguments also participate in key aspects some key considerations broached in *The Structure of Scientific Revolutions*. Although Winch never mentions Kuhn's epochal work (the first edition of *Structure* appears in 1962), Kuhn's discussion of paradigm shifts and revolutions makes common issues with (Winch, 1964), especially with regard to its attribution of incommensurable (a term that Winch never uses) standards of rationality to different groups. Kuhn too has an interest in preserving the rationality of pre-modern scientific reasoning.

Winch attempts to garb a type of cultural relativism in Wittgenstein's authority. But his argument also redoes the historicist debate from the late-nineteenth century – rationality of actions construed as a culturally specific matter. Recovering reasons required recovering ways of thinking either lost or alien to us (or both). Because historically specific, such reasoning did not allow of generalizing in the manner of scientific laws. But since it was putatively based on shared internalized rules, it constituted an object that could be systematically studied, a something with its own reality. (For an interesting and informed updating of some of these matters, see Lloyd (2007).)

The debate plays out in the terms that Winch casts it for several decades. A classic manifestation of its earliest form is Wilson (1970). However, most of the essays in

Wilson (1970) still reflect a lingering philosophical faith in the belief that the requisite notion of scientific rationality could be cashed out in a way that transcended the vagaries of time and place. Debate at least through the 1980s rehashes the rationality issue in the local v. universal terms in which Winch casts it. (See Hollis and Lukes, 1982 for a good representative sample of the post-Winchian debate.)

The rise of post-Mertonian sociology of science in the late 1970s played upon this inferential gap that Kuhn brings into view between when scientists shift theoretical allegiance and the absence of logically compelling reasons for doing so. The self-described "relativism" of some sociologists of science (see for example the essay by Barnes and Bloor 1982) consists of no more than the insistence that theory change in science, if it's to be explained, it must be explained in terms of contextual factors. Since purely logical factors (abstractly characterized) cannot suffice to account for the historical timing of shifts in theoretical allegiances in the sciences, their argument goes, contextual and contingent factors must count into the decision-making of scientists. As Rorty aptly put it,

> Reflection on the method of science has become increasingly thinner since Kant. If there's any upshot of that part of modern philosophy, it's that the scientists didn't have a secret. There isn't something there that's either effable or ineffable. To understand how they do what they do is pretty much like understanding how any other bunch of skilled craftsmen do what they do. Kuhn's reduction of philosophy of science to sociology of science doesn't point to an ineffable secret of success; it leaves us without the notion of the secret of success. (Rorty, 1980b: 55)

Scientists, like the Azande, thus come to be studied not because they have insight into some special form of rationality that transcends cultural bounds, but rather because of their role as gatekeepers to what receives designation as "knowledge." The gatekeepers too turn out to be creatures of their time and place, reasoning from parochial concerns.

Yet Kuhn's way of telling the history and the philosophical work on which Kuhn explicitly draws from Quine essentially changed the terms in which this rationality debate could be cast. For as became evident from reactions even to the first edition of Kuhn's book, the work was understood to pose a direct and not obviously answerable challenge to any hope for some meta-justification of a generic notion of rational inquiry (see, for example, Shapere, 1969: 122–3). Philosophical critics of inductive inference from Hume to Goodman emphasize that reasoning from evidence rests for all intents and purposes on non-logical notions such as "custom and habit" or "entrenchment." Quine and Kuhn add two key considerations to this mix. First, they argue that categorization and inferential connections do not undergo evaluation in an isolated, piecemeal fashion, but always against and embedded in a more general set of assumptions about the nature of things. Second, even the best accounts of how the world works – those represented by received theories in the natural sciences – change historically in ways that make it impossible to demonstrate that different theories represent logical successors to their predecessors.

The notion of scientific rationality thus breaks down as a result in at least two decisive ways. But one has been better noticed than the other. Better noticed has been how any ahistorical account of scientific rationality fails to prove plausible when viewing a history of science through Kuhnian lenses. For methods, on Kuhn's telling of the tale, stay tied to specific theories or their related experimental paradigms. Shifts in theories, typically as driven by shifts in experimental paradigms, alter the conception of accepted method as well. What was rational by way of inquiry under one theory is not so for its successors. (See Zammito, 2004 for an excellent and philosophically well-informed intellectual history of debate on this topic.)

Less noticed, I maintain, has been the extent to which previous aporias in this area made certain metaphysical assumptions about the object of understanding. So, for example, under pressure from post-Kuhnian, historicized accounts of natural science, philosophers have surrendered any notion of a generic scientific method that can be applied to carve the natural world at its joints (Dupré, 1993; Galison and Stump, 1996). Moreover, the case for there being a stable object of knowledge – natural or social – interconnects with some conception of the growth of knowledge. For only on the assumption that theories theorize the same world of objects can knowledge be quantified across changes of theories.

Quine's *Word and Object* (1960) appears two years before Kuhn's *Structure*. Quine rejects as philosophically untenable a conception of truth that Kuhn was to show historically untenable. He forcefully rejects not only the idea that appeal to a method of inquiry or the facts suffices to settle questions of truth, but also denies that some single, stable notion of truth can be salvaged.

Peirce has attempted to define truth outright in terms of scientific method, as the ideal theory which is approached as a limit when the (supposed) canons of scientific method are used unceasingly on continuing experience. But there is a lot wrong with Peirce's notion, besides its assumption of a final organon of scientific method and its appeal to an infinite process.

> For … we have no reason to suppose that man's surface irritations even unto eternity admit of any one systematization that is scientifically better or simpler than all possible others. … Scientific method is the way to truth, but it affords even in principle no unique definition of truth. Any so-called pragmatic definition of truth is doomed to failure equally. (Quine, 1960: 23)

Putting the points together, even if there were a viable notion of scientific method, it would not yield a unique characterization of truth. But there does not exist, in any case, any such organon of science, and post-Kuhn no reason to believe that there will be one. Finally, absent any unique account of truth, then the assumption that there exists some stable object of knowledge about which to collects truths cannot be justified either. Accepted theories license certain statements, and these we call its truths. Nothing more metaphysically robust can be warranted.

That scientific knowledge had increased over time was, pre-Kuhn, an unchallenged dogma of the history and philosophy of science. But once Kuhn raises the specter of incommensurability between paradigms, no cogent account of the growth of knowledge could then be formulated. Kuhn's history ruptures progressive narratives regarding the growth of scientific knowledge. This removes as well another philosophical prop relied upon to support a notion of scientific method as representing rational inquiry *überhaupt*. The objects of science do not remain stable as theories change. Consequently, trans-theoretical claims to the accumulation of knowledge cannot then be warranted. And, lest one forget, Kuhn stigmatizes the social sciences as pre-paradigmatic: "it remains an open question what parts of social science have yet acquired such paradigms at all." (Kuhn, 1996: 15). Without a paradigmatic frame, not even progress in normal scientific terms can be charted.

But the primary point to now emphasize concerns how this break in the growth of knowledge narrative negatively impacts the Winchian side of the problematic as well, that is, the alleged connection between a method for studying the idea of the social and its putative object. Kuhn cites Quine's challenge to the analytic/synthetic distinction as motivating him to construe theory commitments in science as interrelated (Kuhn, 1996: vi). But this point applies, *mutatis mutandis*, to rules and their putative interrelationships as well. That is, just as scientific theorizing supposedly proves constitutive of beliefs about what there is, so too rules supposedly constitute shared conceptions of the social.

What does not get taken seriously enough, I am suggesting, finds early expression in Wittgenstein's remark on Frazer. For this intimates that any attempt at such a "translation" of actions – finding our words for another's reasons – brings with it willy-nilly a translator's heritage of myth-making. On this view, translators never escape from a "hermeneutic circle." Translation requires initial choices, including choices about what even to count as evidence for meaning. Every choice in turn patterns and limits subsequent efforts in this regard. The Humean dichotomy links with those of the hermeneutic circle to create what Quine calls the "indeterminacy of translation."

> The problem is not one of hidden facts, such as might be uncovered by learning more about the brain physiology of thought processes. To expect a distinctive physical mechanism behind every genuinely distinct mental state is one thing; to expect a distinctive mechanism for every purported distinction that can be phrased in traditional mentalistic language is another. The question whether … the foreigner really believes A or believes rather B, is a question whose very significance I would put in doubt. This is what I am getting at in arguing the indeterminacy of translation. (Quine, 1970: 180–1)

There does not appear to be, in Quine's (much disputed) formulation, any fact of the matter by which to arbitrate between imputations of incompatible interpretations (Roth, 2003). And incompatible attributions can be too easily found.

Fixing a realm of facts for the natural sciences will not (indeed, cannot) fix the translational options. For the latter necessarily incorporate intentional and evaluative statements that cannot be caught in any scientific image. Any realm of meaning still remains unsettled even after the realm of physical facts has been fixed. Thus, Quine's remarks about indeterminacy indicate that interpretivists still assume the burden of establishing that there exists their putative realm of facts, their alleged particular "object of understanding." If a realm of facts about meaning exists, the nature of facts in this realm remains quite mysterious (for an important critique, see Turner (2010)).

As Kripke (1982) has made the philosophical community appreciate, specifying what rules a person internalizes proves to be an impossible task. Thus the underlying legitimating assumption that mere communication among people suffices to establish the existence of a shared and stable meaning structures cannot, it now turns out, license that belief after all. More plausibly, to use Quine's topiary metaphor, one has individuals trimmed into outward conformity, but the underlying structures turn out, on examination, to be radically different. This proves true whether one studies the physiology of human nerve endings, and tries to account for "sameness" in that way, or if one empirically investigates the associations that individuals have to specific terms. The case for uniformity cannot be made by empirical investigation, and it cannot be validated by conceptual inquiry.

In sum, under pressure from Quine, Kuhn, and Kripke, neither principled nor empirical arguments can be mounted that there even exists anything that answers to the objects of knowledge that interpretivists seek. This consideration proves *additional* to those considerations that argue for the severing of any link between a method of inquiry and the growth of knowledge.

What Davidson adds to this story, although he sometimes puts the point as if it were anti-Kuhnian, consists of a demonstration that meaning-making involves how rationalizing others consists in part of the *imposition* of a structure of belief on others. If translatable at all, others will in key respects turn out to believe much of what their translators do. If they did not turn out this way, Davidson argues, their language would be literally untranslatable. Indeed, the question would arise as to whether or not they spoke a language at all.

It would be wrong to summarize by saying we have shown how communication is possible between people who have different schemes, a way that works without need of what there cannot be, namely a neutral ground, or a common coordinate system. For we have found no intelligible basis on which it can be said that schemes are different.

It would be equally wrong to announce the glorious news that all mankind – all speakers of language, at least – share a common scheme and ontology. For if we cannot intelligibly say that schemes are different, neither can we intelligibly say that they are one (Davidson, 1973–4: 20).

All that can reasonably be asserted, Davidson maintains, is the success of translation. But success comes at the price of making others into believers like us.

To return one last time to the epigraph from Wittgenstein with which the essay begins, note how it can be read as anticipating Davidson's now famous view on conceptual schemes. Wittgenstein's remark suggests that insofar as attribution of meaning consists of a form of interpretation of what people say or do, interpreters have no choice but to make others like themselves. No interpretation without conceptual assimilation.

The thesis that reasons ought to be part of any explanation of actions remains extremely tendentious. On the one hand, it appears to rule out without argument explanations of actions that require no appeal to reasons as causes, for example, biological explanations of behavior. (For a polemical statement of this view, see Rosenberg (1980).) More recent and sophisticated attempts to incorporate frameworks native to the natural science as part of a scheme to explain macro-social phenomena also make no reference to reasons in the explanations offered of behavior (see, for example, Diamond, 1999). In such cases, it need not be the case that causes reduce to blind biological imperatives. Rather, reason operates "behind the back" of the individuals involved, as can be found in rational choice explanations of, for example, the location of trading centers.

On the other hand, the notion of rationality to which reason explanations appeal has proven notoriously elusive and difficult to specify, and runs a gamut from extremely contextualized historicist notions to a historical formalizations of principles of rational decision making. (For an array of current views on this matter, see Little (1990) as well as the exchange between Henderson (2005), Risjord (2005), Stueber (2005), and Roth (2005).) If accounts of rationality can be "thin" (require little specific context, as in game theoretic models), then formal models and their mathematical expressions of principles of rationality should do the needed work. This would preserve attributions of rationality without any loss of generality. If accounts of the social require "thick" conceptions of rationality (i.e. ones rich in specifics of time and place), then a science of the social appears unlikely. The weight of contextual detail does not allow the account to rise to the level of useful generalizations about behavior. (See, for example, Obeyesekere, 1994 and Sahlins, 1995 for how these competing "thin" and "thick" views of rationality play out in a specific interpretive dispute.) But this debate too threatens only to replay the presumed divide between explanation (thin accounts) and understanding (thick accounts). Moreover, with regard to reasons specific to historical situations, no consensus can be identified regarding a method to access this or to provide an uncontroversial mark of correctness of an historical reconstruction (see Novick, 1988).

But once the respective objects have been thrown into question as determinate and stable, it deprives a basis for meta-debates about methods of rationality, at least in those terms that the debate has traditionally been cast. Likewise, but even less appreciated, taking to heart the implied consequences of interpretation as assimilation makes moot disputes about reconstructing how any individual or group "really" thinks about things. But once metaphysical assumptions regarding perduring stable objects of inquiry go – the "real" world or "real" meanings – so goes notions of fixed methods for accessing these objects and determinate ways of rationalizing alleged findings. In this context, a more pluralistic, purpose-tied account of how inquiry proceeds emerges.

Barry Barnes proposes an account of what he terms "natural rationality," which he stipulates as "cognitive propensities" that make inductive inferences happen more or less automatically (Barnes, 1976: 115). On this account, to treat rationality, understood as the process of inferring inductively in ways that one's social group finds licit and acceptable (hence "rational") naturalistically just means to treat it as itself an *empirical* phenomenon, that is, one learned by studying what people actually do in specific situations. In Barnes's pejorative use of "philosophers," philosophers insist on characterizing the notion of rationality in an a priori fashion. Barnes quite plausibly maintains that this has proven to be a failed project. (Barnes betrays no evidence of having read in 1976 either Davidson on conceptual schemes or Quine on naturalism.) But that point should not be taken as critical. Rather, whatever people accept, from poison oracles to theorem proving on Barnes's view, constitutes grist for a naturalist's mill. No matter what a group accepts as knowledge "does not mean that its emergence, acceptance, and persistence are not empirical phenomena. Acts of validation and assertions of validity are themselves empirical phenomena, and as such are available for sociological investigation." (Barnes, 1991: 321). Barnes ultimately comes to recognize and acknowledge Quine's account of "epistemology naturalized" and Kripke's account of rules as intellectually akin (Barnes, 1991: 334). Barnes's discussion of how to study and understand the notion of rationality thus has many important features in common with Quine and Davidson.

We began with the question of whether or not rationalizing behavior can be made compatible with explaining that behavior, where the term "explanation" implies placing that behavior in a scheme of causes and related causal laws. This led to a dilemma, a dilemma fostered I have argued by a number of problematic philosophical assumptions, all tied in one way or another on the account given to a particular metaphysics of knowledge. Debates about rationality assume this metaphysics of objects and a related epistemology. Yet neither the metaphysics nor the epistemology has proven philosophically viable. The conclusion urged has been that the nineteenth century debate no longer should be credible. Nothing remains by which to engineer its philosophical divide.

Thus, despite the continuation of a nineteenth century debate about rationality through the present day, by the late 1970s the philosophical tide had turned. The notion of rationality with regard to inferences about matters of fact long ago lost any claim to *a priori* status. In addition, accounts of scientific rationality could not vindicate a specific method as linking the process of inquiry and the growth of knowledge. But once the notion of how our beliefs themselves faced the "tribunal of experience" changed from having them adjudged one-by-one to having them operate in concert with one another, the question of how to validate beliefs took on a radically different philosophical aspect. That process comes to be understood as internal to the beliefs that support particular practices of inference making. Finally, with the work of Kripke and Davidson in hand, the study of the constitutive rules by which shared meanings become possible, and so the social, itself stands revealed as an artifact of being imposed on the subjects of study (Bernstein, 1978: 92–3).

Ironically, disconnecting discussions of rationality from an implausible metaphysics of knowledge proves enabling rather than paralyzing. For it allows for what might be termed a methodological pluralism in the social sciences at least. (Whether, in Paul Feyerabend's infamous phrase, one should also adopt a stance of being "against method" in the natural sciences is left for others to debate.) Absent assumptions about a determinate logic of science or a specific theory of meaning, debate in the analytic tradition in philosophy of social science permanently transmutes. It no longer needs to obsess about what counts as a *science* of the social because there now exists no fixed or determinate meta-notions of science, explanation, rationality, or understanding.

Methodological pluralism, in this case, does not deny systematic processes of inquiry that ties a notion of rationality to the ends of inquiry. (This is a guiding theme of Turner and Roth (2003), both with regard to their joint essay and the volume as a whole.) This helps fill out what sense to make of appeals to "methodological pluralism" and "pragmatism." (See Rorty (1980a) for prescient remarks on this matter.) The issue of rationality becomes pragmatic at least in the sense of 'pragmatic' that ties to the ends of inquiry, and what methods abet attainment of those ends. The ghost of a nineteenth century philosophical dispute about rationality should at last finally be exorcised, and the dead hand of tradition made to release its grip on the philosophy of social science.

ACKNOWLEDGEMENTS

I wish to thank Jay Peters, Mark Risjord, and Stephen Turner for their comments on an earlier draft of this essay. Responsibility for what appears remains mine.

REFERENCES

Adorno, T., H. Albert, R. Dahrendorf, J. Habermas, H. Polot and K. Popper (1976) *The Positivist Dispute in German Sociology.* New York : Harper Torchbooks

Apel, K.-O (1984) *Understanding and Explanation.* Cambridge, MA: MIT Press.

Ball, T. (1976) 'From paradigms to research programs: Toward a post-Kuhnian political science', *American Journal of Political Science*, 20: 151–177.

Barnes, S.B. (1976) 'Natural rationality: A neglected concept in the social sciences', *Philosophy of the Social Sciences*, 6: 115–126.

Barnes, S.B. (1991) 'How not to do the sociology of knowledge', *Annals of Scholarship*, 8: 321–335.

Barnes, B. and D. Bloor (1982) 'Relativism, rationalism and the sociology of knowledge'.(Hollis and Lukes 1982), 21-47.

Bernstein, R.J. (1978) *The Restructuring of Social and Political Theory.* Philadelphia: The University of Pennsylvania Press.

Davidson, D. (1973–1974) 'On the very idea of a conceptual scheme', *Proceedings and Addresses of the American Philosophical Association*, 47: 5–20.

Davidson, D. (1980) 'Psychology as Philosophy'. *Essays on Actions and Events.* New York: Oxford University Press.

Diamond, J. (1999) *Guns, Germs, and Steel: The Fates of Human Societies.* New York: Norton.

Dupré, J. (1993) *The Disorder of Things.* Cambridge, MA: Harvard University Press.

Galison, P. and D. Stump (ed.) (1996) *The Disunity of Science.* Stanford, CA: Stanford University Press.

Fay, B. (1975) *Social Theory and Political Practice.* London: Allan & Unwin.

Gilbert, M. (1989) *On Social Facts.* New York: Routledge.

Habermas, J. (1988) *On the Logic of the Social Sciences.* Cambridge, MA: MIT Press.

Hempel, C.G. (1965) *Aspects of Scientific Explanation.* New York: The Free Press.

Hempel, C.G. (1969) 'Logical positivism and the social sciences', in P. Achinstein and S. Barker (eds) *The Legacy of Logical Positivism.* Baltimore: The Johns Hopkins Press. pp. 163–194.

Henderson, D. (2005) 'Norms, invariance, and explanatory relevance', *Philosophy of the Social Sciences*, 35: 324–338.

Hollis, M. and S. Lukes (eds) (1982) *Rationality and Relativism.* Cambridge, MA: MIT Press.

Horton, R. (1970) 'African traditional thought and Western science', in B. Wilson (ed.) *Rationality.* Oxford: Blackwell. pp. 131–171.

Iggers, G. ([1968]1983) *The German Conception of History.* Revised edition. Middletown, CT: Wesleyan University Press.

Kitcher, P. (1985) *Vaulting Ambition: Sociobiology and the Quest for Human Nature.* Cambridge, MA: MIT Press.

Kripke, S. (1982) *Wittgenstein on Rules and Private Language.* Cambridge, MA: Harvard University Press.

Kuhn, T. ([1962] 1996) *The Structure of Scientific Revolutions*, 3rd edn. Chicago: University of Chicago Press.

Little, D. (1990) *Varieties of Social Explanation.* Boulder, CO: Westview Press.

Lloyd, G.E.R. (2007) *Cognitive Variations: Reflections on the Unity and Diversity of the Human Mind.* New York: Oxford University Press.

Novick, P. (1988) *That Noble Dream: The 'Objectivity Question' and the American Historical Profession.* Cambridge: Cambridge University Press.

Obeyesekere, G. (1992) *The Apotheosis of Captain Cook.* Princeton, NJ: Princeton University Press.

Popper, K. (1957) *The Poverty of Historicism.* Boston: Beacon Press.

Quine, W.V.O. (1953) 'Two dogmas of empiricism', in W.V. Quine (ed.) *From a Logical Point of View.* Cambridge, MA: Harvard University Press. pp. 20–46.

Quine, W.V.O. (1960) *Word and Object.* Cambridge, MA: MIT Press.

Quine, W.V.O. (1969) 'Epistemology naturalized', in W.V. Quine (ed.) *Ontological Relativity and Other Essays.* New York: Columbia University Press. pp. 69–90.

Quine, W.V.O. (1970) 'On the reasons for the indeterminacy of translation', *The Journal of Philosophy*, 67: 178–183.

Richardson, A.W. (2003) 'Logical empiricism, American pragmatism, and the fate of scientific philosophy in North America', in G. Hardcastle and A.W. Richardson (eds) *Logical Empiricism in North America.* Minneapolis, MN: University of Minnesota Press. pp. 1–24.

Risjord, M. (2005) 'Reasons, causes, and action explanations', *Philosophy of the Social Sciences*, 35: 294–306.

Rorty, R. (1980a) 'A reply to Dreyfus and Taylor', *Review of Metaphysics*, 34: 39–46.

Rorty, R. (1980b) 'A discussion', *Review of Metaphysics*, 34: 47–55.

Rosenberg, A. (1980) *Sociobiology and The Preemption of Social Science.* Baltimore: Johns Hopkins University Press.

Roth, P.A. (2000) 'The object of understanding', in B. Kögler, (ed.) *Empathy and Agency.* K. Stueber & Boulder, CO: Westview Press. pp. 243–269.

Roth, P.A. (2003) 'Why there is nothing rather than something: Quine on behaviorism, meaning, and indeterminacy', in D. Jacquette (ed.) *Philosophy, Psychology, and Psychologism: Critical and Historical Readings on the Psychological Turn in Philosophy*. Kluwer Academic. pp. 263–287.

Roth, P.A. (2005) 'Three grades of normative involvement', *Philosophy of the Social Sciences*, 35: 339–352.

Ryle, G. (1949) *The Concept of Mind* New York: Barnes & Noble, Inc.

Sahlins, M. (1995) *How 'Natives' Think: About Captain Cook*, *For Example.* Chicago: University of Chicago Press.

Salmon, W.C. (1989) *Four Decades of Scientific Explanation.* Minneapolis, MN: University of Minnesota Press.

Searle, J. (1995) *The Construction of Social Reality.* New York: Free Press.

Sellars, W. (1956) 'Empiricism and the philosophy of mind,' in H. Feigl and M. Scriven (eds) *Minnesota Studies in the Philosophy of Science, Volume I.* Minneapolis, MN: University of Minnesota Press. pp. 253–329.

Shapere, D. (1969) 'Towards a post-positivist interpretation of science', in P. Achinstein and S. Barker (eds) *The Legacy of Logical Positivism.* Baltimore: The Johns Hopkins Press. pp. 115–160.

Stephens, J. (1973) 'The Kuhnian paradigm and political inquiry', *American Journal of Political Science*, 17: 467–488.

Stueber, K.R. (2005) 'How to think about rules and rule following', *Philosophy of the Social Sciences*, 35: 307–323.

Suppe, F. (ed.) (1977) *The Structure of Scientific Theories*, 2nd ed. Urbana, IL: University of Illinois Press.

Taylor, C. (1981) 'Understanding and explanation in the *Geisteswissenschaften*' in S. Holtzman and C. Leich (eds) (1981) *Wittgenstein: To Follow a Rule.* New York: Routledge.

Turner, S. (1980) *Sociological Explanation as Translation.* Cambridge: Cambridge University Press.

Turner, S. (1994) *The Social Theory of Practices: Tradition, Tacit Knowledge, and Presuppositions.* Chicago: University of Chicago Press.

Turner, S. (2010) *Explaining the Normative*. Malden, MA: Polity

Turner, S. and P. Roth (2003) 'Ghosts and the machine: Philosophy of social science in contemporary perspective', in S. Turner and P. Roth (eds) *The Blackwell Guide to the Philosophy of the Social Sciences.* Malden, MA: Blackwell. pp. 1–17.

Wilson, B. (ed.) (1970) *Rationality*. Oxford: Blackwell.

Winch, P. (1958) *The Idea of a Social Science and its Relation to Philosophy*, Second edition. Atlantic Highlands, NJ: Humanities Press.

Winch, P. (1964) 'Understanding a Primitive Society,' reprinted in (Wilson, 1970).

Wittgenstein, L. (1979) *Remarks on Frazer's Golden Bough.* Atlantic Highlands, NJ: Humanities Press.

Zammito, J.H. (2004) *A Nice Derangement of Epistemes: Post-Positivism in The Study of Science From Quine to Latour.* Chicago: University of Chicago Press.