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Author(s): Dick Pels

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Karl Mannheim and the Sociology of Scientific Knowledge: Toward a New Agenda*

DICK PELS

University of Groningen and University of Amsterdam, The Netherlands

In previous decades, a regrettable divorce has arisen between two currents of theorizing and research about knowledge and science: the Mannheimian and Wittgensteinian traditions. The radical impulse of the new social studies of science in the early 1970s was initiated not by followers of Mannheim, but by Wittgensteinians such as Kuhn, Bloor, and Collins. This paper inquires whether this Wittgensteinian program is not presently running into difficulties that might be resolved to some extent by reverting to a more traditional and broader agenda of research. A social theory of knowledge (or social epistemology) along Mannheimian lines would not only reinstate the "magic triangle" of epistemology, sociology, and ethics, and hence revive the vexed problem of "ideology critique," but would also need to reincorporate the social analysis of science into a broader macrosocial theory about the "knowledge society."

TWO TRADITIONS IN THE SOCIAL THEORY OF KNOWLEDGE

Signalling a regrettable fact and advancing an appropriate remedy are intellectual operations that presuppose and codetermine one another in a circular manner. The unfortunate fact concerns the relative divorce and mutual indifference between two contemporary currents of theorizing and research about knowledge and science, which I call the "Mannheimian" and the "Wittgensteinian" traditions. I use these appellations in somewhat ambiguous homage to David Bloor, who, in one of the first statements in print of the Edinburgh Strong Programme, compared the two thinkers with regard to the strategic possibility of a sociological explanation of logic, mathematics, and natural science (Bloor 1973). The Mannheimian program for the sociology of knowledge was considered "weak" precisely for its refusal to explain cultural and natural science symmetrically, and hence to extend causal sociological analysis to the "hard case" of the natural sciences; and for its coincident failure to demand an equally radical symmetry between the sociological explanation of true and false beliefs, thus confining the sociology of knowledge to a mere "sociology of error." In both respects, Wittgenstein was celebrated as offering a more attractive starting point: "Wittgenstein solves Mannheim's problem" (Bloor 1973:173; cf. Bloor 1983).¹

Accordingly, the spurt of intellectual initiative that awoke the slumbering sociology of knowledge to the radical impulse of the new social studies of science in the early 1970s was not initiated by Mannheimians, but largely developed without Mannheim, if not in conscious opposition to his work. Although in the discursive ferment of the 1960s and '70s the Mannheimian heritage was kept alive by sociologists of knowledge such as Mills,

* Amsterdam School for Social Science Research, University of Amsterdam, Oude Hoogstraat 24, 1012 CE Amsterdam, The Netherlands; e-mail: dick.pels@philos.rug.nl. Previous versions were read at the Congrès du centenaire of the Institut International de Sociologie, June 1993, at the Sorbonne, Paris, and at the XIIIth World Congress of Sociology, July 1994, in Bielefeld. I am grateful to Werner Callebaut, David Kettler, Volker Meja, Alan Sica, Irving Velody, Anna Wessely, Rein de Wilde, and three anonymous reviewers for helpful comments.

¹ Cf. Bloor's similar approach to the rift between Wittgenstein and Durkheim. While Durkheim still opted to except Western scientific culture from the social explanations he applied to primitive systems of classification, Wittgenstein did not "lose his nerve or betray himself in this way" (Bloor 1983:3).

Gouldner, Coser, Shils, and Wolff, their efforts did not provoke a distinctly Mannheimian research tradition in the 1980s—with the significant exception of the grand editorial project carried through by Kettler, Meja, and Stehr (cf. Goldman 1994; Kettler and Meja 1994). For various reasons, interesting in themselves, contemporary social theorists such as Elias, Bourdieu, Foucault, Habermas, and Giddens have found only limited use for Mannheim. Insofar as they have developed distinct sociologies of knowledge, they have also operated in virtual isolation from radical Wittgensteinian science studies.

The real action and excitement in the sociology of knowledge, on the other hand, was not generated by mainstream sociology but emerged from the new philosophy and historiography of (natural) science. The seminal work of Kuhn, insofar as philosophical sources entered into it, took its inspiration not from the sociology of knowledge tradition but from Wittgenstein and Fleck, and initially concentrated not on “soft” sociological, political, or historical thought but on the “harder” sciences of nature and medicine.² Bloor and Barnes, the progenitors of the Strong Programme, as well as Collins, Mulkay, and Lynch, followed a Wittgensteinian rather than a Mannheimian track, as did constructivists such as Knorr-Cetina, Woolgar, and Latour.³ Evidently, Bloor’s reproach about Mannheim’s “failure of nerve” concerning a symmetrical treatment of true knowledge and natural science was considered sufficiently damaging to turn his sociological project into a dead horse. Henceforth, Mannheim was cited solely as a token predecessor (cf. Knorr-Cetina 1983:115, 136; Law 1986:1).⁴

Let me at once enter some specifications that qualify my claim about a Wittgensteinian turn in science studies, to avoid the risk of forcefully homogenizing what are in fact quite diverse streams of theorizing and research (cf. Callebaut 1993). These provisos will simultaneously elaborate significant reservations about Bloor’s opposition of a “strong” Wittgensteinian to a “weak” Mannheimian program in the social theory of knowledge; in fact, the legacies of both Mannheim and Wittgenstein are much more interpretively flexible than is suggested by such sweeping categorical gestures. Bloor’s critical reading of Mannheim, indeed, has been plausibly described as actually more of a correction and expansion of the classical sociology of knowledge than an across-the-board attack on it

² Barnes (1982:9, 34, 65) cites Fleck, Piaget, and the later Wittgenstein as Kuhn’s primary extrahistorical intellectual sources. Bloor (1983) likewise suggests strong parallels between Kuhnian naturalism and Wittgenstein’s allegedly naturalistic “social theory of knowledge.”

³ E.g. Collins 1985:12ff., 24n, 152n; 1986:3, 8n; 1990:17, 20–21, 225n; Woolgar 1988:45–50. The pivotal significance of Wittgenstein is also exemplified by the recent exchange between Lynch and Bloor, shortly to be discussed, in the course of which the former has opposed an ethnomethodological and antiepistemological reading of Wittgenstein to the latter’s naturalistic interpretation (Bloor 1992; Lynch 1992a, 1992b, 1993). The lines of disagreement are anticipated in Lynch’s earlier objections against Phillips’s (1977) “appropriation” of Wittgenstein’s philosophy for a rather Bloorian sociology of science (Lynch 1985:179ff.). A parallel critical effort is found in De Vries (1992), who counters the “neo-Kantian” and “epistemological” Wittgenstein canvassed by Bloor and Collins with a more radical “anthropological” Wittgenstein, more congenial to Latourian actor-network theory. Lynch (1993) provides a rich account of the development of “strong” Sociology of Scientific Knowledge (SSK) and its many sibling rivalries.

⁴ The width and depth of the divorce of the traditions can also be fathomed by looking at the institutional and professional distribution of research interests and research personnel. The ISA Research Committee on the History of Sociology and the ASA Theory Section, which include a number of “regular” social theorists and Mannheim scholars, have so far hardly concerned themselves with the new social studies of science and technology, which are pursued in rivalling institutional forums such as the Society for the Social Study of Science (4S) and the European Association for the Study of Science and Technology (EASST). Established sociology journals, including relatively new and adventurous ones such as *Theory and Society* or *Theory, Culture, and Society*, hardly ever print contributions to science and technology studies, which tend to appear in *Social Studies of Science*, *Philosophy of the Social Sciences* or *Science, Technology, and Human Values*. In the Netherlands, as elsewhere, science and technology studies have mainly developed outside and in opposition to established social science departments and at new, often technically oriented universities. While the new social studies of science, in defiance of the Mannheimian proviso, have massively invaded natural and medical science and technology, the social and historical study of social science itself has somehow remained the preserve of regular sociology (cf. Wagner 1990; Heilbron 1995). Cf. also n. 8.

(Lynch 1993:42ff.; Kim 1994:391); whereas his recruitment of Wittgenstein in support of such a strengthened naturalistic sociology has equally come under severe attack (cf. Sharrock and Anderson 1984; Hacking 1984; Coulter 1989; Lynch 1992a, 1993). Appropriations of Mannheim have diverged as sharply as have those of the later Wittgenstein. While pleading an anticausalist, antiepistemological, and “praxeological” version of Wittgenstein, for example, Lynch uncritically goes along with the same naturalistic reading of Mannheim that is still taken for granted by Bloor. Other critics of the Strong Programme, however, such as Hekman (1986), have dismissed such a reading as a “serious misunderstanding,” and opposed it with an antiepistemological and antifoundationalist one which brings the Mannheimian project close to that of Gadamerian hermeneutics. (The curious intrigue here is that Hekman symmetrically reads Wittgenstein as still caught up in the epistemological concerns that dominated Enlightenment thought, and as failing to make the crucial move from epistemology to [Gadamerian] ontology [1986:120, 128].)

If neither entity, Mannheim nor Wittgenstein, identifies a stable corpus of intellectual claims, and may easily take the epistemological role of the other, the tension and the rift may seem less between firmly established ancestral traditions than between contrasting naturalistic and antinaturalistic interpretations of *both* thinkers. Nevertheless, I still presume that talk about a “Wittgensteinian turn” plausibly draws together the many divergent strands in the science studies movement into a relatively coherent picture. I also presume that a more attentive reading of Mannheim, which more definitely acknowledges the productive indecisions and complexities that traverse his project, will help specify an intellectual position that may counter some of the deficiencies presently evolving within this Wittgensteinian project. The point of this paper is therefore to inquire whether, if perhaps “Wittgenstein” has to some extent solved “Mannheim’s” problem, the reverse might not also apply—in other words, whether the Wittgensteinian program in the social studies of science, which has so successfully reinvigorated science and technology studies over the past two decades, is not presently running into difficulties that might be resolved to some extent by reverting to a broader and more traditional Mannheimian agenda of research. The gap that has developed between the rival perspectives can and should be bridged, perhaps by a relative synthesis of Mannheimian inspiration. This requires, first, some preliminary recovery work. It has not been just the Wittgensteinian tradition of science studies that has forgotten about Mannheim; the modern sociological tradition itself has likewise found itself reinventing Mannheimian wheels. Hence this paper constitutes an effort to recover something like an original Mannheimian inspiration from beneath the brushwork of *both* the modern social studies of science *and* the modern sociology of knowledge.

VALUE-FREE RELATIVISM AND ITS DISCONTENTS

Before sketching a summary programmatic outline for such a relative synthesis between the two traditions, I will first attempt to identify the relative coherence of the Wittgensteinian project and locate its present impasse. I take it to indicate a basic style of thinking and research that may be succinctly characterized as “value-free relativism” (cf. Pels 1991). This project integrates a strong antiepistemological and antinormativistic temper with a radically contextualist and descriptivist methodology. Suspicious of all philosophical generalization and normative “legislation,” it advocates the close descriptive analysis (ethnography) of concrete examples of everyday and scientific language use and practices (case studies method). It conceives language as (speech) *action*, challenging traditional views that emphasize reference, correspondence, and mimetic representation. It thus stresses the *performative* nature of language-in-use: Words are also deeds. The meaning of words resides in their practical usage, and remains limited to the “language games” or “forms of

life" in which they are put to practical use (cf. Pitkin 1972:39, 289; Phillips 1977:27–30). The relativity of scientific truth claims is thus only a particular case of a more widespread skepticism concerning the rule-bound character of practical activity. Truth, reason, and logic, far from compelling particular courses of action, form post hoc rationalizations for orderly practices and shared conventions (Woolgar 1988:46–47, 50; Collins 1985:12ff.; Lynch 1993:71ff., 162ff.).

Wittgensteinian value-free relativism thus strongly insists on the replacement of a normativistic by a naturalistic conception of rationality, according to which discrepancies in belief become "simply a matter of cultural variation, so that all beliefs in all cultures become equivalent for the sociologist" (Barnes 1976:125; Bloor [1976] 1991). Naturalism and relativism in the theory of knowledge hence imply *symmetry* of explanation between truth and error, and *impartiality* or moral indifference towards nature and society—the two core tenets of the Strong Programme that have been progressively extended and radicalized in subsequent waves of constructivist science studies (cf. Pels 1996). Constructivist naturalism neutralizes the question of legitimacy or illegitimacy of knowledge in favor of ethnographic *redescriptions* of scientific discourses and practices (cf. Knorr-Cetina 1983) which, in line with Wittgenstein's own analyses of ordinary language use, nearly "leave everything as it is" (cf. Sharrock and Anderson 1984:377; Collins and Yearley 1992:308–309). *Nearly*, since the critical edge of such dispassionate investigations (conducted from Wittgenstein's "Martian point of view"), is not to harvest "denunciations" of erroneous native beliefs or irrational native actions but to "display" the historicity and pragmatic contingency of scientific practices and claims (cf. Latour 1993:43–46).⁵ On this interpretation, the label "value-free relativism" appears sufficiently comprehensive to include the drift from the causalist "politics of explanation" of Bloor (who is more hesitant to follow Wittgenstein on this issue) toward the "politics of description" deployed more recently by Knorr-Cetina, Callon, or Latour; it also encompasses sibling rivalries such as lately conducted between Collins and Latour and between Bloor and Lynch, all of whom operate a more or less radical methodology of disinterested analysis.⁶

This Wittgensteinian agenda has two major weaknesses, which predictably mirror its major strengths (cf. Fuller 1988; Radder 1992; Fuchs 1992; Lynch 1993). First, its ethnographic descriptivism easily degenerates into a type of empiricism or positivism that conflicts with its own precept of reflexivity, because it ignores the normative and political constitution of its own knowledge claims. While undermining quite a number of entrenched epistemological binaries (cognitive vs. social explanations, science vs. politics, culture vs. nature), value-free relativism remains quagmired in the dualism of facts vs. values, and is unable to deal successfully with the problem of critique (Pels 1990, 1991; Proctor 1991:244ff.; Radder 1992). Second, its predominant interest in the microdynamics of laboratory settings and scientific controversies has prejudiced it in favor of an actor-centered

⁵ Note how close this Wittgensteinian methodology is to Foucault's suspicions about ideology critique and his alternative precept to investigate the empirical linkages between truth contents and power effects. Foucault likewise neutralizes the question of legitimacy/illegitimacy, which must be exchanged for the concrete study of the acceptability of knowledge claims in terms of a "reconstruction of their 'positivity'—which is simultaneously an uncovering of their fundamental *arbitrariness*, their contingency, their 'violence'" (e.g., 1994:74–76; cf. Pels 1995b for a critical perspective). The critical issue is also (nicely but involuntarily) captured by Sharrock and Anderson's opinion that Wittgenstein's injunctions "to describe, not explain," and "to look, not think" were "not a call to create a programme of empirical research, but for us to take notice of things that are staring us in the face" (1984:386). Cf. also n. 23.

⁶ Cf. Lynch on Woolgar's and Ashmore's reflexivism as "perhaps a more consistent application of Bloor's impartiality postulate than Bloor had in mind," and as "an extremely strong injunction to act in accordance with the Mertonian norm of 'disinterestedness'" which in fact takes Mannheim's "nonevaluative" conception of ideology to its ultimate limit (Lynch 1993:106–107). But neither does Lynch himself abandon the search for a neutral or nonevaluative observation language, in line with the ethnomethodological policy of "indifference," despite his critique of current SSK empiricism and his advocacy of a "praxeological turn" (115, 141ff., 303).

bias, which has tended to occlude broader macroinstitutional settings that constrain the economic, social, and political conditions of scientific production. Despite the Strong Programme's lingering macrosociological sensitivity, and despite major recent efforts to reincorporate the larger societal context in the study of local scientific work (Haraway 1991; Knorr-Cetina 1982; Shapin and Schaffer 1985; Cozzens and Gieryn 1990; Fuchs 1992; Latour 1993), constructivist studies after the "ethnographic turn" have preferred the micro-side of the micro-macro dualism as well as the factual side of the fact-value dichotomy. The point is now to "get constructivism out of the lab" (Gieryn 1995:440).

My own gamble is that such programmatic shortcomings may be alleviated by promoting a (partial and guarded) shift towards a more comprehensive and normatively sensitive Mannheimian research agenda. This will require a return from a doubly restricted *sociology of science* to a more broadly conceived *social theory of knowledge* or *social epistemology* (Fuller 1988, 1992, 1993; Harding 1991; Pels 1991; Fuchs 1992; Roth 1994).⁷ It implies a shift from a micro-oriented ethnography of laboratory life and scientific controversies toward a macrosocial theory of knowledge as classically outlined in the works of Marx, Durkheim, Mannheim, and Berger and Luckmann, which reinserts science studies in the more general concerns of cultural studies and social and political theory. Such a comprehensive sociopolitical theory of knowledge and culture (cf. also Bourdieu and Wacquant 1992; Beck 1992; Beck, Giddens, and Lash 1994) extends its scope of analysis more emphatically from academic-scientific beliefs toward *nonacademic* (ideological, political, journalistic, everyday) ones and their multiform interrelationships. This also marks the recuperation of the problem of *ideology* and *ideology critique* (the Mannheimian problem of "ideology and utopia"), which, as I have indicated, has been unjustly cashiered from the roster of "symmetrical" science studies (and from that of postmodern philosophy and antifoundationalist social theory more generally), but which appears to prepare for a major comeback (cf. Simons and Billig 1994; Lynch 1994; Žižek 1994). It also includes an attempt to reconnect the social theory of knowledge to a *social theory of intellectuals, experts, and professionals* or, more broadly, to a macrotheory of the "knowledge society" and its emerging strata of epistemocrats or cultural capitalists (e.g., Bauman 1987, 1992; Eyerman and Jamison 1991; Stehr 1994).⁸ Last but not least, it includes an attempt to restore the breadth and depth of what Mannheimians (such as Stehr 1981; Stehr and Meja 1982) have called the "magic triangle" of symmetrical interdependency among *epistemology, sociology, and ethics*, thus counteracting the tendency of an imperialistic sociology (and of postmodernist philosophy in general) to abrogate epistemology and ethics altogether (cf. Radder 1992; Winner 1993; Squires 1993; Bauman 1993).

All this, however, should be undertaken while preserving the crucial epistemological profits gathered by two decades of research and reflection in the radical microstudies of scientific knowledge: their ethnographic precision; their symmetrical inclusion of natural, practical, and technological knowledge as objects of analysis; their antirealist and antiuniversalist approach to issues of representation; their radical reflexivity; and the strong thesis

⁷ The term *social epistemology* already occurs in Merton (1973:107, 113, 123), whose register entry of the term retrospectively suggests that it was present since 1941. However, the literal expression appears not to be used before 1972. The term was also employed by social constructionist Gergen (e.g., 1988), before being popularized by Fuller (1988).

⁸ Stehr's recent book provides one of the sharpest illustrations to date of the gap between more traditional sociology of knowledge concerns and radical constructivist science studies; the latter are virtually absent from his macrosocial narrative. Bauman's prolific writings on intellectuals and the state remain likewise uninformed by constructivist studies of science, even though Bauman shares many of their postmodernist concerns. On the other side of the looking-glass, the voluminous authoritative *Handbook of Science and Technology Studies* (Jasanoff et al. 1995) is astonishingly weak on contributions from the sociological mainstream (except Restivo 1995 and Gieryn 1995). For example, while Bourdieu's work receives only scant and inadequate discussion (cf. Callon 1995:37–41), neither Mannheim nor Elias are considered worthy of being indexed.

about the essential inseparability of cognitive and social dimensions in knowledge formation, which is also echoed by Foucault's slogan of *pouvoir/savoir* (cf. Pels 1995b). Precisely where this synthetic effort will take us is at present difficult to predict.⁹ Nevertheless, the primary task of a social epistemology, as here proposed, would be an attempt to restyle or reinvent some ideas from the classical tradition by reimporting the gains of contextualism, constructivism, and reflexivity, and thus to reapply the critical results of science and technology studies to social and political theory itself (cf. Barnes 1988; Latour 1991, 1993; Law 1991).

REINVENTING THE WHEEL

I have already noted the curious fact that, since the early 1970s, the mainstream sociological tradition has been as silent about Mannheim as has the alternative tradition inspired by Wittgenstein, Fleck, and Kuhn. Whereas one can still trace conscious lines of descent connecting Mannheim to American sociologists of knowledge such as Merton, Mills, Coser, Shils, Berger, and Gouldner, major contemporary European thinkers such as Elias and Bourdieu, who found themselves much closer to the sources of the sociology of knowledge tradition both geographically and intellectually, have somehow managed to skip over Mannheim and reinvent many of his crucial insights without due awareness or acknowledgment. Norbert Elias's sociogenetic and processual theory of knowledge, despite his sometimes condescending tone about the work of his former principal, is substantively informed by unacknowledged terminological and substantive borrowings from Mannheim's early work (cf., e.g., Mannheim [1922] 1982; for different views cf. Mennell 1989; Kilminster 1993). Pierre Bourdieu's investigation of intellectual, scientific, and cultural fields, which until recently has developed in remarkable isolation from both German and Anglo-Saxon sociological lineages, is likewise marked by a virtual absence of references to Mannheim's work, although core ideas of his field theory of science are clearly prefigured there. The celebrated 1928 lecture on cultural competition, which Elias at the time still enthusiastically hailed as a "spiritual revolution" but subsequently ignored,¹⁰ also anticipates the basic lineaments of Bourdieu's quasi-economic model of science, including its perhaps central idea of a selection (*Auslese*) of "truth" as a product of the criss-crossing censure induced by interested intellectual competition (Bourdieu [1975] 1981; Mannheim 1952:196–97; in Meja and Stehr 1982:326).¹¹

This abridgment of historical consciousness may also explain why Karin Knorr-Cetina's early work (1977, 1981), while sympathetically building upon and engaging with Bourdieu's quasi-economic theory of scientific competition, largely credits Bourdieu with a model that can already be found in outline in Mannheim. A similar substitution is evident in some early "economistic" articles by Latour (e.g., Latour and Fabbri 1977; Latour 1994) and in Latour and Woolgar's benchmark study *Laboratory Life* (1979).¹² Knorr-Cetina's subsequent, more critical discussion of the economic model of science repeatedly identifies

⁹ For example, it raises the difficult issue of the conditions of possibility of an antirealist and antifoundationalist ideology critique (cf. recent efforts by Simons and Billig 1994; Lynch 1994; Žižek 1994).

¹⁰ Elias's criticism of Mannheim is most balanced in his 1990 work (138 ff.). However, his account of the Züricher Soziologentag subtly exaggerates his own intellectual independence and plays down his own partiality to Mannheim's views in the latter's dispute with Alfred Weber. Goudsblom (1987:69) fails to mention that the "spiritual revolution" supported by Elias against Alfred Weber's "idealism" and "individualism" was in fact Mannheim's (cf. Elias in Meja and Stehr 1982:388–89; also Mannheim [1922] 1982:62).

¹¹ And including its problems: cf. the residual universalism of Bourdieu's notion of rational progress and his residual holism (cf. Mannheim [1922] 1982:170; Pels 1995a).

¹² More accurately, these studies, like Bourdieu's exemplary 1975 work, mix the economic metaphor of credit and the political metaphor of authority rather liberally, while privileging somewhat the former vocabulary. Cf. also Latour (1994), which was first published in 1984 but apparently written much earlier (cf. Knorr-Cetina 1982:127).

Merton as one of its initiators, while once again failing to mention the obvious ancestorship of Mannheim (Knorr-Cetina 1982:104; 1983:129). Bourdieu confuses Merton and Mannheim in quite similar fashion. In critical homage to the former, and in defense of important features of the Mertonian approach against the “levelling” and “nihilistic” impulse of radical science studies, Bourdieu sympathetically cites him as follows, in order to embrace this position as entirely his own:

In the cognitive domain as in others, there is competition among groups or collectivities to capture what Heidegger called the “public interpretation of reality.” With varying degrees of intent, groups in conflict want to make their interpretation the prevailing one of how things were and are and will be.

Since it was Merton, Bourdieu continues, who established that science must be analyzed sociologically through and through, the Strong Programme did little else but crash through a wide-open door (1990:297–98). This Mertonian piece of wisdom, however, is a paraphrase of a crucial passage in Mannheim’s 1928 essay (1952:196–97). Merton himself provides the reference in a footnote, in explicit mention of Heidegger’s *Sein und Zeit*, “as cited and discussed by Karl Mannheim” in 1928 (Merton 1973:100–101).¹³

While the longitudinal relationship between Bourdieu and Mannheim thus raises a few questions, the lateral relationship between Bourdieu and radical constructivist science studies appears to have become one of sincere mutual annoyance. Seated a mile from each other at their respective Parisian desks (at the Collège de France and the Ecole des Mines), Bourdieu and Latour have succeeded in digging an ocean of intellectual distance between them—in true Parisian fashion (cf. Callebaut 1993:107, 473). Bourdieu has summarily thrown together the Strong Programme (which is not distinguished from Latourian actor-network theory or any other variant of science studies) with modern currents in reflexive anthropology and literary deconstruction as opening the door toward a narrowly disguised “nihilistic relativism,” which stands squarely opposed to a truly reflexive social science (Bourdieu and Wacquant 1992; Bourdieu 1993). The constructivist notion about a “seamless web” connecting science and society is said to introduce a monistic reductionism, a “short-circuiting” of explanations that fatally regresses behind Merton. If the Strong Programme took its own tenet of reflexivity seriously, Bourdieu contends, it would recognize that its grandiose and self-certain statements only perform a distinctive break, while serving little else but the initial accumulation of scientific capital.¹⁴

In his turn, Latour has increasingly toned down his early enthusiasm for Bourdieu’s field theory of science, while the latter now evidently feels that his ideas have been plagiarized. Since their early work, which is still heavily imprinted by the quasi-economic model, both Knorr-Cetina and Latour have veered increasingly toward a quasi-political or power model of science, and have grown increasingly critical of the Bourdieuan metaphors of credit, profit, and cultural capital (e.g., Latour 1986). In addition, Latour has summarily incorporated Bourdieu in his sweeping critique of the “poverty of sociology,” which allegedly remains stuck in the Kantian divorce between culture and nature, and is unable to account for the quasi-objects and the new hybrid powers which are created in laboratories (1993:5–6, 51, 54). Bourdieu is thus offered as a typical victim of what Latour calls the “Modern

¹³ Merton’s familiarity with Mannheim’s work can be traced back at least to his early article (1941).

¹⁴ “The ultraradicalism of a sacrilegious denunciation of the sacred character of science, which tends to cast discredit on all attempts to establish—even sociologically—the universal validity of scientific reason, naturally leads to a sort of nihilistic subjectivism. Thus the cause of radicalization which inspires Steve Woolgar and Bruno Latour drives them to push to the limit or, better, to extremes, the kinds of analysis, such as the ones that I proposed more than ten years ago, which endeavour to transcend the (false) antinomy of relativism and absolutism . . .” (Bourdieu 1990:299).

Constitution,” which continues to separate the representation of facts in nature from that of citizens in social world, while the “networks,” the “hybrids,” the “quasi-objects” are presently proliferating so massively as to undermine this divorce from all sides (1993:5). Knorr-Cetina, for her part, somewhat downplays this critique of Bourdieu’s “forgetfulness of the object,” but still ends up by pontifically agreeing with Latour that Bourdieu’s sociology of science “is orthogonal to, and beside the point of, the most interesting developments in recent sociology of science” (Callebaut 1993:473–74).

Even so, it is not Bourdieu or Latour but Mannheim who should be identified as the *Urheber* of the “agonistic” model of science in either of its currently popular quasi-economic and quasi-political formulations: *viz.* of the idea of intellectual competition and profit-seeking, and of the consonant idea of science as a “continuation of politics by other means.” If perhaps the Starnberger School actually coined the term *science dynamics*, it was Mannheim who at least came very close when referring to the “dynamics” of intellectual currents, their “inner rhythm,” or their “rhythm of development” (1952:329–30, 326). Gouldner and Friedrich did not invent the reflexive slogan of a “sociology of sociology” or even that of a “Marxism of Marxism”: Mannheim did, as early as 1922 (Mannheim [1922] 1982; [1936] 1968:111). Foucault’s *epistème*, Althusser’s *problématique*, and Kuhn’s concept of paradigm are all foreshadowed in Mannheim’s early work on *Weltanschauung* (1952:33ff.). Many ideas of the “Kuhnian turn,” which launched the success story of the modern social studies of science, were prefigured in Mannheim’s sociology of knowledge, among which are the critique of *a priori* rationalism, the tendential blurring of the divisions among facts, theories, and values, the idea about scientific revolution as a *Gestalt*-switch (e.g., Mannheim [1922] 1982:127–28), the rootedness of scientific viewpoints in underlying worldviews, and the constitutive role of scientific communities. Curiously, it took an iconoclastic historian of natural science, following a Wittgensteinian path, to reawaken the social sciences to much of their own Mannheimian heritage (cf. Phillips 1977:72–76).

STYLES OF THOUGHT

I will elaborate another example a little further, since it so deftly illustrates the lack of interaction between the two traditions that was already manifest in the 1960s. It concerns the concept of “style of thought” which social students of science routinely ascribe to Ludwik Fleck’s 1935 constructivist classic *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*, but which is found as early as 1921 in Mannheim. Fleck’s work was rediscovered through Kuhn, who acknowledged that the former had anticipated many of his ideas (1962:vi–vii, 1979). Although the concept of *Denkstil* makes its appearance in 1929 in Fleck’s rejoinder to a 1928 article by Kurt Ziegler (Schäfer and Schnelle 1980:xxii, 1983:16), Mannheim explicitly traces it to a 1910 Hungarian article by Lukács (Frisby [1983] 1991:124).¹⁵ Fleck himself, however, and his modern editors after him consistently fail to refer to Mannheim. This is quite remarkable in view of the fact that Fleck does mention sociologists of knowledge such as Durkheim, Lévy-Bruhl, Gumpłowicz, Jerusalem, Scheler, and Simmel, and hence comes extremely close to Mannheim’s contemporary work (Fleck [1935] 1980:62–70, 145). It is almost inconceivable that he could have missed Mannheim’s contributions.¹⁶

¹⁵ And even further back to a 1901 work by Alois Riegl (e.g., Mannheim [1922] 1982:127, 233). Wittgenstein’s *Philosophische Untersuchungen* (1945–49) do advance such concepts as “language game,” “life form,” and “family likeness,” but do not feature the concept of “thought style.”

¹⁶ Fleck appears to quarry his knowledge about sociology primarily from Jerusalem’s long contribution to Scheler’s collection, *Versuche zu einer Soziologie des Wissens* (1924; cf. Meja and Stehr 1982:27ff.). Jerusalem’s article refers neither to Mannheim nor to the concept of *Denkstil*.

Mannheim, as indicated, liberally employs the sociological concept of *Denkstil* at least from 1921 in his article on *Weltanschauung* (1952:34–35; also [1922] 1982:86–88, 124–28). The concept is reapplied in his study of “Historicism” (1924) and in his early programmatic article on the sociology of knowledge (1925). His Habilitationsschrift “Conservative Thought” (1927) begins with a more systematic exposition of the style concept, which exemplifies its strategically central position for Mannheim’s sociology of knowledge as a whole (1953:74–84). The widely acclaimed and discussed lecture on “Cultural Competition” of the following year gives it critical prominence, as is evidenced by Mannheim’s singular remark that “theories of knowledge are really only advance posts in the struggle between thought-styles” (1952:228, 210). In the course of the debate following Mannheim’s lecture, the concept is used by Alfred Weber as if it was already common currency (in Meja and Stehr 1982:374–75). In Mannheim’s *Ideologie und Utopie* (1929) the concept of *Denkstil* is once again omnipresent.

While Fleck’s version of the concept arises in the context of analyzing the construction of medical and natural-scientific facts, Mannheim’s idea of a “thought style” precisely captures the specific difference that is said to obtain between cultural or human studies and the natural sciences, and emphasizes the interpretative advantage that the former enjoy over the latter. In this capacity, it closely fraternizes with concepts such as *Weltanschauung*, *Gestalt*, or “totality” and has a similarly synoptic intent (e.g., [1922] 1968:111). A style of thought expresses a fundamental volitional impulse or “world postulate” (*Weltwollen*) residing beneath and structurally organizing more articulate cognitive contents. Ultimately rooted in pretheoretical, irrational motives, it provides the connecting link between discursive contents and prediscursive experiential group strivings and group rivalries. As such, it is merely a different name for the “existential attachment” of thinking itself (cf. 1952:210). Hence, much like the concept of *Weltanschauung*, *Denkstil* provides an antireductionist intermediary term that highlights the indirect connections between intellectual contents and social class interests which are often mistakenly correlated in direct fashion: “We cannot relate an intellectual standpoint directly to social class; what we can do is find out the correlation between the ‘style of thought’ underlying a given standpoint, and the ‘intellectual motivation’ of a certain social group” (1952:184; 1953:74–84).¹⁷

MANNHEIM’S “FAILURE OF NERVE”

As already noted, the first statements of the Edinburgh Strong Programme still include various critical discussions of Mannheim, whereas subsequent generations of sociologists of science are satisfied to treat him as a largely irrelevant predecessor. If only for this reason, it is important to reinspect the “failure of nerve” argument, which is encountered most curtly and polemically in Bloor ([1976] 1991), Barnes (1977), and Woolgar (1988:23, 48) and appears in a more sophisticated and quiet version in Bloor (1973), Barnes (1974), and Mulkay (1979:10–17, 60–62).¹⁸ As indicated before, this critical argument suggests that Mannheim failed to carry through the activist, relativist, and symmetrical impulses of his own program of a sociology of knowledge, and conceded too much to the contemplative model of a realist and rationalist epistemology. This led him to exclude mathematics, logic,

¹⁷ Cf. Bourdieu’s repeated (e.g., 1990, 1993) warnings against “short-circuit” explanations, which fail to notice the inevitable “refraction” of external social interests by the laws of the intellectual field. This accusation is also, and mistakenly, levelled at the Strong Programme and its radical offshoots (1990:298).

¹⁸ In terms of the “Epistemological Chicken” dispute about the radicalization of the symmetry principle, as conducted between Collins and Yearley and Callon and Latour (cf. Pickering 1992:301ff.; Pels 1996), Mannheim would no doubt be considered an early example of “chicken.” Woolgar capitalizes (on) “Mannheim’s Mistake” as general indicator of the tendency to exempt particular kinds of knowledge (maths, natural science, sociology, SSK itself) from the purview of sociological analysis (1988:23, 48).

and the natural sciences from a comprehensively social and contextual analysis. In 1973, to be sure, Bloor still generously admits that Mannheim's conception of the sociology of knowledge is a "close approximation" to the Strong Programme, although he "faltered" with respect to the all-important symmetry principle (1973:175). Somewhat later, however, he accuses sociologists such as Mannheim, who continue to privilege natural scientific knowledge as an exceptional case, of a betrayal of their essentially naturalistic disciplinary standpoint and a "lack of nerve and will": "Despite his determination to set up causal and symmetrical canons of explanation, his nerve failed him when it came to such apparently autonomous subjects as mathematics and natural science" ([1976] 1991:4–5, 11, 157).¹⁹

Now there is no question of refuting this type of critique or of showing its lack of textual foundation. The methodological demarcation between cultural or human studies and natural science is a well-documented position in Mannheim from his earliest writings onward (e.g., [1922] 1982:75–76, 98–99; 1952:35–36, 44, 101–102, 130, 135). An entirely representative example is found in the 1928 lecture on intellectual competition, where Mannheim emphasizes that the postulate about the *Seinsrelativität* or positional and perspectival determination of thought does not touch the exact natural sciences, but is confined to historical, political, cultural and social, and ordinary everyday thought. In the latter type of "existentially determined" knowledge, the results of thought processes are determined partly by the nature of the thinking subject. Whereas in the natural sciences thinking is carried on by an abstract "consciousness as such," in existentially determined thought it is the "whole man" who does the thinking (1952:193–94).

However, the attempt to bring this demarcation under the psychologically debunking category of a "failure of nerve" appears to miss and to misrepresent Mannheim's own critical epistemological intentions. First of all, the "failure of nerve" argument is distinctly *anachronistic*. The neo-Kantian distinction between culture and nature and between cultural and natural sciences, which Mannheim adopts as his starting point, is primarily intended to *liberate* or *emancipate* the sciences of culture and society from the epistemological primacy of natural-scientific method, and hence to relax and delegitimize the exemplary function of natural-scientific rationality (cf. Frisby [1983] 1991:194; Mannheim 1952:37, 67, 70–71). It is Mannheim's explicit aim to recover the autonomous logic of cultural and social knowledge that has been *discredited* by the unwarranted extension of natural scientific methodology (1952:76, 82). Both the positivistic and the Kantian conception of science mistakenly conceive of exact natural science as the only ideal prototype to which all sciences must surrender. However, since each sphere of reality has its own kind of "givenness," each type of thinking should be understood in "its own innermost nature" (1952:70, 126, 194). Natural scientific knowledge invokes only one of the capacities for creating knowledge possessed by the human spirit, but tends to hypostatize this one form of knowledge as knowledge *per se* ([1922] 1982:76, 185, 195). It would be altogether wrong, proceeding from such an overly narrow conception of knowledge, to deny existential determination to knowledge in general or to treat this condition as a defect to be eliminated (186).²⁰

In consequence, the knowledge-political motive that underlies Mannheim's *distinction* between cultural and natural science is largely identical to the one that subsequently informs the Strong Programme's attempt to *erase* it. Evidently, Mannheim did not suffer from a

¹⁹ Barnes locates a singular tension in Mannheim's sociology of knowledge between "relationism" and a remnant "epistemological" faith in the potency of reason. The idea of a *freischwebende Intelligenz* as granting access to really valid knowledge is judged "an unsatisfactory finale to a penetrating and courageous work" (1974:147–48). Similarly, although *Ideology and Utopia* opens with a commitment to an activist conception of knowledge, a great part of his argument is in fact predicated upon a "contemplative" model (Barnes 1977:3–4).

²⁰ Cf. Bloor ([1976] 1991:164) who considers the social-institutional character of our best scientific achievements "not a defect but part of their perfection."

failure of nerve, but courageously attempted to break the spell of dominant epistemological conceptions of truth and rationality, marching against the same “static philosophy of Reason” which subsequently constituted the point of attack of the more radical Wittgensteinians. Mannheim’s stated objective is to undermine scientism by freeing “half of the world” and half of the sciences from its suffocating embrace. Liberation from epistemological rationalism is precisely also what constitutes the “Kuhn effect,” which breaks the spell of unitary scientific methodology by offering a novel and disrespectful interpretation of the development of natural science itself. Barnes (1977:3) therefore entirely misreads Mannheim when he concludes that he took the disinterested knowledge of the natural sciences as *preferable* to sociology or history or political thought. On the contrary: Mannheim regularly insists upon the distinctive *superiority* of the cultural, historical, and social sciences against the natural sciences precisely because the former are existentially bound and hence share unique cognitive profits, offering a “deeper penetration into its object than is ever possible in the exact sciences” ([1922] 1982:252; [1936] 1968:150–52; cf. Hekman 1986:74).²¹

There are some additional ironies at hand. As suggested before, Bloor’s symmetrical extension of causal sociological method toward logic, mathematics, and natural science operates a form of sociological naturalism that imitates features of the same objectivistic scientism from which Mannheim took care to distance himself (cf. Hekman 1986:39ff.; Lynch 1993:50–52, 71). The Strong Programme, Bloor argues, simply extends and articulates the naturalistic, value-free stance of sociology, which is also “the basic standpoint of the social sciences.” Bloor thus cavalierly identifies his own naturalistic standpoint not only with that of social science in general, but also with that of “most contemporary science,” which is primarily “causal, theoretical, value-neutral, often reductionist, to an extent empiricist, and ultimately materialistic” ([1976] 1991:157). Mannheim’s intention to save value-bound “interpretation” and “understanding” from the regime of empiricist causal explanation (e.g., 1952:81–82) is thereby simply reversed. In addition, Mannheim’s productive hesitations with regard to the epistemological separability between facts and values are countered by a clear Weberian distinction between evaluation and explanation.²² Although Bloor admits that the Strong Programme is itself value-based, its core underlying value entails a conception of the natural world as “morally empty and neutral,” affirming the same moral neutrality that we have learned to associate with all the other sciences ([1976] 1991:13). Mannheim, on the other hand, discusses something close to Bloorian symmetry in his treatment of the “non-evaluative general total” conception of ideology ([1936] 1968:71–72), only in order to plead the inevitable transition toward a more evaluative point of view (see below).²³

In view of such misrepresentations and difficulties, it must not be reckoned a failure and weakness of Mannheim’s program that natural science was considered a special case and provisionally immunized from sociological scrutiny. In its own historical context, Mann-

²¹ In addition, there are intimations scattered across Mannheim’s work that the dualism between natural science and historical-cultural-social science is not a final methodological state. “Static” methodology must ultimately find a place within a more comprehensive “dynamic” conception of science, and historical method may provide a “point of unity” from which the dualism can be surmounted (e.g., 1952:130–33). Cf. his parallel remarks about the “partial” nature of traditional epistemology, which is exclusively oriented towards natural science (1968:261ff.). Cf. also Hekman (1986:78).

²² This distinction is also increasingly evident in Collins’s work. Cf. his (1991) rejoinder to Scott, Richards, and Martin (1990).

²³ Elsewhere, Mannheim emphasizes more strongly that the sociology of knowledge has “no moral or denunciatory intent,” and pleads avoidance of the term *ideology* because of its moral connotations. A few pages after this, however, he reaffirms that a “particularizing” sociology of knowledge inevitably arrives at a point where it becomes more than sociological description, and turns into a critique “by redefining the scope and the limits of the perspective implicit in given assertions” (Mannheim [1936] 1968:237, 256). Cf. also n. 5 on Wittgenstein and Foucault.

heimian asymmetry was as strong as Bloorian symmetry was in the context of the early 1970s, and there is a tinge of contextual unfairness in accusing Mannheim of having failed to reverse his perspectivist and relationist method upon the natural sciences themselves. If Mannheim could still write that while the development of natural science was propelled by an “immanent logic of things,” each phase of the social sciences was connected “in both content and method with the total process” ([1922] 1982:277), the Kuhnian turn in the historiography and sociology of natural science has convincingly proven otherwise. It has revealed the workings of a cultural and social logic that has softened the hard sciences beyond Mannheimian imagination, conclusively disproving Mannheim’s contention that the exact sciences make statements “into whose content the historical and local setting of the knowing subject and his value orientation do not enter” (1952:101). However, if *all* the sciences now stand revealed as in some sense “existentially determined,” strong symmetry is less a critique of the Mannheimian program than a contemporary extension of it (Lynch 1993:42). If *all* knowledge is presently seen as “a continuation of politics by other means” or as subject to a logic of interested competition, it should be acknowledged that it was Karl Mannheim who first “liberated” this idea for the cultural and social sciences.

PRODUCTIVE INDECISIONS

I commenced my argument by drawing attention to the intellectual rift between a Mannheimian and a Wittgensteinian tradition in the study of knowledge and science, and pleaded a partial return to Mannheimian macrosociological, epistemological, and normative concerns in order to help resolve some internal Wittgensteinian difficulties. This synthetic move toward a *social epistemology*, as it has been provisionally called, is expected to repair what some commentators deplore as the “unsplendid isolation” of radical science studies, which, it is said, have lost touch with the main body of sociological work and have so far failed to produce a truly general theory of knowledge and science (e.g., Fuchs 1992). But this move also bridges the divide from the opposite side, realigning some major recent macro-sociologies of knowledge, such as those of Elias and Bourdieu, more closely with the core concerns and insights of micro-oriented constructivist science studies. In this manner, the revitalization of social and political theory, which has been rather unilaterally proclaimed by actor-network theorists such as Latour (1991) and Woolgar (1994), may be reciprocated through a sociological and political broadening of the research agenda of science studies themselves.

Let me conclude the present review by specifying some of the themes of a social epistemology that is able to renegotiate some of the (in)differences between the two traditions. However paradoxically, the heritage of Mannheim is inspirational here precisely because his work is enlivened by the presence of a number of unresolved ambiguities.²⁴ As noted, Mannheim has repeatedly been accused of indecision on crucial points of epistemology, of hovering between the old and the new, and of lacking the courage of his convictions. Here I wish to contend that these hesitations are to some extent productive, insofar as they provide useful correctives of particular Wittgensteinian exaggerations, and hence offer a more ecumenical point of departure where analytic differences are not split but surpassed. More particularly, they help us avoid the logic of inversion that still constitutionally ties radical science studies to the *a priori*s of traditional epistemology that they have set out

²⁴ As Mannheim himself concedes in a well-known 1946 letter to Wolff, his work does not conceal inconsistencies (“covering up the wounds”), but intends to “show the sore spots in human thinking at its present stage.” Such contradictions are not mistakes but “the thorn in the flesh from which we have to start”; they are not due to shortsightedness but result from the fact that “I want to break through the old epistemology radically but have not succeeded yet fully” (quoted in Wolff 1959:571–72).

polemically to reject (cf. Darmon 1986). In this sense, we may generalize Kettler, Meja, and Stehr's observation that Mannheim's "special, productively unresolved" position (in their case, between Hegelian Marxism and Weberian sociology) makes his work "a timely, heuristically valuable starting point for fresh study" (1990:1470).

The following remarks do not focus upon the larger "social" in social epistemology—i.e., upon sociological macroprocesses of intellectualization, scientization, or professionalization (where a Mannheimian program builds upon acknowledged strengths)—so much as retrieve and clarify a few points of "socialized" epistemology itself. Fundamental to the project of a social epistemology, first of all, is a spirited resistance against the "eliminativist" tendency present in naturalistic science studies which, as we have seen, regularly dismiss normative epistemology in favor of a rigorously empirical study of "what actually happens" in science. Mannheim's first "productive indecision" is located precisely at the intersection of this classical disciplinary contest. His initial polemical separation between normative epistemology and empirical sociology appears to anticipate the abolitionist temper of modern science studies, but is subsequently revoked in favor of a sociological *reconstitution* of questions of truth, rationality, objectivity, and value (cf. 1952:192, 226–27; [1936] 1968:1ff., 256ff.). Apparently, Mannheim's purpose was "not so much to criticize epistemology as to rescue its project in the light of historical developments which rendered it redundant" (Scott 1987:42). It was to replace an academic epistemology, which remained tied to justificatory and foundationalist concerns and a static, individualistic polarity between subject and object, with a new, relational and positional epistemology which would effectively "reckon with the facts brought to light by the sociology of knowledge" (Mannheim [1936] 1968:70–71, 264)—specifically, the fact of "the essential perspectivism of human knowledge" (quoted in Wolff 1959:571). It was thus not to discard the problem of truth and objectivity altogether, but to bring it "one notch lower, closer to the level of concrete research" (1952:103).²⁵

Another closely related indecision on Mannheim's part engages the long-standing controversy about cognitive vs. social explanations of the development of knowledge; here, Mannheim's views are mobilizable as an antidote against remnants of both cognitivist and sociologistic reductionism. Even though (*pace* Bourdieu 1990:298) the conception of social determination professed by the Sociology of Scientific Knowledge (SSK) has never been of the unilateral "short-circuit" variety, the logic of inversion has regularly lured its theorists in this abolitionist direction, as is exemplified by Latour's Seventh Rule of Method, which advised a moratorium on cognitive explanations of science and technology originally extending for a ten-year period—thus shortly due to expire (1987:247, 258). Mannheim's path-breaking analysis of the role of competition in cultural and intellectual life, while departing from the radical assertion that this "external" social factor "enters as a constituent element into the form and content of every cultural product or movement," is carefully demarcated against both cognitive internalism and unbridled sociologism (1952:191–92). The metaphors of a "politics" and an "economics" of knowledge, which may alternatively express this internal sociocognitive link, but which also sometimes serve as vehicles of intellectual rivalry between contemporary schools of sociology of science (cf. Bourdieu's "economic" field theory vs. Latour's "political" actor-network theory), are still liberally

²⁵ Lynch (1992:215n) suggests a workable distinction between foundationalist epistemology, the traditional *bête noire* of symmetrical and agnostic sociology, and a "small *e*" epistemology that is compatible with constructivist empirical interests. If extended in a Mannheimian direction, a defence of small *e* epistemology would undercut or at least loosen up the original Bloorian postulate about the symmetrical causal treatment of truth and error, possibly in the direction of a *weaker* postulate of asymmetry (cf. Pels 1996). Hence, and presumably different from Lynch, my small *e* epistemology would also be compatible with a "minimal" normativity, following upon its recognition of a "natural proximity" between facts and values.

mixed in some of the most central passages of Mannheim's original 1928 essay (1952:196–98, 210–14).

Anticipating Bourdieu's antieconomistic economy of practices, Mannheim defends himself against the criticism of "projecting specifically economic categories into the mental sphere." The original discovery of the role of competition by the Physiocrats and Adam Smith only demonstrated the impact of a *general social relationship* in the particular context of the economic system. The ultimate aim must be to strip our categorial apparatus of anything specifically economic in order to grasp the social fact *sui generis* (Mannheim 1952:195). In close analogue, Mannheim strives to avoid the alternative pitfall of political reductionism by emphasizing that the explanation of intellectual movements in political terms is "not intended to give the impression that mental life as a whole is a purely political matter, any more than earlier we wished to make of it a mere segment of economic life"; it is simply the case that the "vital and volitional element" in thought "is easiest to grasp in the political sphere" (1952:212). Whether expressed in the metaphor of economics or that of politics, it is evident that the activist, "volitional" and "interested" determination of thought must not be taken as a simple "reflex" of various social locations, but presupposes a specific "freedom" of the intellectual sphere (1952:195, 228–29). Mannheim accordingly distinguishes between material *interestedness* and *committedness* to styles of thought or intellectual forms, and between *social* and *intellectual* stratification (1952:183–87). Although it is perhaps too much to say that claims such as these anticipate Bourdieu's field theory of science, they do help to tone down extremist suggestions about a "seamless web" connecting science and society, and about the imperative dissolution of the modernist divide between scientific and political representation (cf. Latour 1993).

NORMATIVE COMPLEXITY

This antireductionist impulse also helps specify Mannheim's intriguing intermediate stance in the familiar contest between "partisanship" and "value-freedom"—one which is doubly defensive against the Marxian danger of direct politicization and propagandistic thought and against the Weberian danger of uncommitted, disengaged intellectualism. Thinking, for Mannheim, is inevitably positionally determined and hence committed, but not *politically* determined or evaluative in any *immediate* sense. His criticism of the liberal or modernist divorce between facts and values (e.g., 1952:216–17; cf. Proctor 1991; Root 1993) and his contrary assertion of the inevitable value-groundedness of thought provoke not a total renunciation of the ideal of political neutrality, but rather an attempt to revitalize its original impulse (Mannheim, in Meja and Stehr 1982:401). Thought does not progress by completely restraining its evaluative and pragmatic orientation, but by retaining its original *élan politique* while simultaneously subjecting it to reflexive intellectual control ([1922] 1968:42, 89n).²⁶ In this manner, the rejection of a superficial amalgamation of facts and values, and of science and politics, is balanced against the admission of the inevitable presence of a mixture of facts and values and a politics of knowledge in the deep structure of theoretical definition and empirical analysis (cf. Pels 1993).

Since I take this issue of facts vs. values, or normativism vs. naturalism, as strategically crucial for a social epistemology as here proposed, I will conclude my argument by detailing it a little more closely. I have already pointed out that SSK's scramble to consign various

²⁶ "Value-freedom," Mannheim argues in a turn of phrase that would be repeated many times after, "is possible in sociology and social knowledge in the sense that one ought to refrain from any valuation. But at a much deeper level, valuation cannot be excluded; namely at the level of the perspectivity that has entered into the formation of concepts" ([1924] 1982:247).

inherited philosophical dichotomies to the intellectual scrap heap has so far piously halted before the deconstruction of this particular dualism.²⁷ Once again, Mannheim is interesting because his agenda reaches beyond the straightforward replacement of normative inquiry by a self-satisfied sociological naturalism. His purpose is not to reject but to reformulate a concept of “noological” rationality that follows through the implications of the historicity and positionality of both epistemological and normative beliefs ([1936] 1968:265–66). Although questions of fact and right are provisionally divorced for methodological purposes, they are reconnected in order to effect a necessary transition toward an evaluative conception of the sociology of knowledge and toward an explicit defence of the utopian element in sociological thought ([1936] 1968:78ff., 235–36).

Lynch has argued that SSK’s core postulates of symmetry and impartiality were actually less an attack on Mannheim’s program than a more radical *extension* of it. In his view, Bloor, Barnes, Collins, and even reflexivists such as Woolgar, further radicalized what Mannheim called the “non-evaluative general total conception of ideology,” which advocated a provisional suspension of all judgment as to the correctness of the ideas under analysis, and hence can be easily harmonized with a naturalistic and agnosticist reading of the later Wittgenstein (Lynch 1993:42ff.). What his analysis ignores, however, is that Mannheim explicitly distinguishes *two* types of approach to ideological inquiry, which arise on the level of the general-total conception of ideology: an approach characterized by freedom from value judgments (“non-evaluative relationism”) and an epistemological and metaphysically oriented *normative* approach called “dynamic relationism” ([1936] 1968:71, 76, 88). According to Mannheim, what is originally simply a methodological technique ultimately discloses itself as a *Weltanschauung*, which encompasses metaphysical-ontological value judgments of which we remained previously unaware. The very possibility of being completely emancipated from ontological, metaphysical, and ethical presuppositions is considered a “positivistic prejudice” ([1936] 1968:78–79). Although we may begin nonevaluatively, “we shall be forced eventually to assume an evaluative position,” which explicitly avows the implicit metaphysical presuppositions that underlie and make possible empirical knowledge. This normative shift is “typical of the whole development of contemporary thought” (80–86).

Lynch’s own reflexive praxeology, for all its legitimate objections to Bloorian SSK and its “scientization” of the later Wittgenstein, continues to share much of this positivistic prejudice by refusing to entertain a normative epistemology of whatever kind (cf. Lynch and Fuhrman 1991; Lynch 1992c; Lynch and Fuhrman 1992). Indeed, insofar as ontological concerns have presently begun to inform more radical versions of SSK, such as actor-network theory, they do not implicate articulated normative commitments; the constitutive postulates of symmetry and agnosticism (“following the actants”) continue strongly to work against this. De Vries’s construction of an anti-Kantian and ontological (more precisely, Latourian) Wittgenstein, for example, keeps distant from normative concerns, recommending that we merely “expose” the way in which forms of life “actually work” as heterogeneous ensembles of words, actions, and things (1992:30–31). From a different viewpoint and tradition, Hekman, who makes such a laudable effort to rescue Mannheim from Bloorian scientism by highlighting his transition from neo-Kantian epistemology to ontology (a move she thinks Wittgenstein was unable to make), fails to specify in what sense

²⁷ Remarkably, *Social Studies of Science* editor David Edge, in his introduction to the mentioned *Handbook of Science and Technology Studies*, predicts a double move toward a more explicitly normative perspective and toward broader social-philosophical concerns in the studies of science. The “essential inseparability of facts and values,” he suggests, must be confronted directly in STS research, and “is likely to be a very fruitful focus of STS debate for the foreseeable future” (Edge 1995:16–18).

such an antifoundationalist and hermeneutical ontology integrates a value perspective (Hekman 1986).

It cannot be my purpose here to speculate about the general relationship between epistemology and ontology, even though a social epistemology would presumably refuse a strict separation between the two concerns, as it has before refused the forced choice between normative epistemology and empirical sociology. In this respect it would remain opposed to SSK's epistemological "indifference," including the various praxeological, ontological, or anthropological radicalizations of its central postulates of symmetry and impartiality. Indeed, it would prefer to consider the contest between sociological explanation and ontological description as a fraternal rivalry among fellow Wittgensteinians, and as less significant than the broader contest between naturalism (causalist *and* descriptivist) and normativism. In this range of dispute—and perhaps also going beyond what Mannheim himself had in mind—it would tend to explore the possibility of an intentional *confusion* of facts and values, as so stringently forbidden by Barnes, Bloor, and many other proponents of value-free relativism inside and outside of SSK. Following Mannheim's suggestion about the proximity between evaluative and ontological judgments, it would accordingly favor a much closer fit between the concerns of epistemology, ontology, and ethics.

It is *this* Mannheim who today may still be fruitfully opposed to the symmetrism and agnosticism of Wittgensteinian ethnography. By highlighting the normative complexity of his approach, we are at least reminded that the contest between normativism and naturalism is still open. So are the other contests which Mannheim failed to decide.²⁸ They can only be brought to a close if the well-entrenched polarization between what I have called the Mannheimian and the Wittgensteinian traditions is considered deeply problematic from both sides, and each side recognizes that it stands in need of the other in order to facilitate the "next step" in the development of a social theory of knowledge.

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²⁸ A presently unelaborated point concerns Mannheim's lack of resolve, which I take to be equally provocative here as in preceding cases, in face of the micro/macro alternative. While radical constructivist science studies have tended to privilege various micro-oriented and actor-centered approaches, mainstream sociologists such as Elias and Bourdieu have continued to emphasize the weight of institutional macrostructures and advocated methodologies that account for the structural determination of social interaction. Although both structuralists and constructivists have filed claims for the supersession of the dualism in a "relational" direction (e.g., Elias's "figurational" sociology, Bourdieu's "constructivist structuralism," or Latour's "associology"), ultimate preferences for actor- or structure-oriented approaches have remained divided, and analytical closure has not been achieved. If Mannheim's emphasis upon a "configurational" methodology and a "historical structural analysis" (1952:87, 181) of epistemological problems and forms of knowledge cannot be hailed as a solution, it helps to counteract the more radical versions of nominalism and interactionism that science studies have introduced.

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