

DAVID KAISER

A Mannheim for All Seasons: Bloor, Merton, and the Roots of the Sociology of Scientific Knowledge

The Argument

David Bloor often wrote that Karl Mannheim had “stopped short” in his sociology of knowledge, lacking the nerve to consider the natural sciences sociologically. While this assessment runs counter to Mannheim’s own work, which responded in quite specific ways both to an encroaching “modernity” and a looming fascism, Bloor’s depiction becomes clearer when considered in the light of his principal introduction to Mannheim’s work — a series of essays by Robert Merton. Bloor’s reading and appropriation of Mannheim emerged from his background in experimental psychology and his attempts to supercede Merton’s own structural-functional program for the sociology of knowledge. By retracing this extended trail of readings and re-readings, we may begin to understand the roots of Bloor’s curious interpretation of Mannheim’s sociology of knowledge, and inquire in a reflexive way about the present and future directions of science studies.

Introduction: Merton and Mediations of Mannheim

When David Bloor began to construct his “strong programme” for the sociology of scientific knowledge in the early 1970s, he frequently presented his work as a logical continuation of the work of Karl Mannheim. Bloor made this connection explicit in several places. In the beginning of his 1973 article on “Wittgenstein and Mannheim on the Sociology of Mathematics,” Bloor wrote simply that “Mannheim’s conception of the sociology of knowledge is a close approximation to the strong programme” (Bloor 1973, 175). When outlining the four tenets of his strong programme three years later, Bloor similarly conceded that “they are by no means new, but represent an amalgam of the more optimistic and scientific strains to be found in Durkheim (1938 [1895]) [*The Rules of Sociological Method*], Mannheim ([1929]1936), and Znaniecki ([1940] 1965) [*The Social Role of the Man of Knowledge*]” (Bloor [1976] 1991, 7). To Bloor, there was a clear and direct link between his sociology of scientific knowledge and the corpus of work in the sociology of knowledge from the 1920s and 1930s.

This link was more than one of similarity: Bloor's strong programme aimed to address and correct what to him was an obvious lacuna in the older work on the sociology of knowledge. Mannheim's work, as Bloor read it, had pioneered an approach for studying knowledge in the humanities and social sciences, but had continually shied away from undertaking such studies of scientific knowledge. Bloor explained the problem in the beginning of his 1973 article: "One of the central problems of the sociology of knowledge is the status of logic and mathematics. These branches of knowledge are so impersonal and objective that a sociological analysis scarcely seems applicable. Time and again, in his *Ideology and Utopia*, Karl Mannheim's determined advocacy of the sociology of knowledge stops short at this point" (Bloor 1973, 173).

Bloor was far less gentle in his depiction of the problem in his 1976 manifesto for the strong programme, *Knowledge and Social Imagery*. Here he explained, "the cause of the hesitation to bring science within the scope of a thorough-going sociological scrutiny is lack of nerve and will" (Bloor [1976] 1991, 4). And about Mannheim in particular, we learn that "despite [Mannheim's] 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" (Bloor [1976] 1991, 11). To Bloor, then, the next logical step appeared obvious: his strong programme would derive its "strength" by carrying Mannheim's program forward, bringing sociological descriptions to realms of knowledge in which even the mightiest of social theorists had formerly feared to tread.

Bloor's habit of presenting this relationship between his strong programme and the classic sociologies of knowledge has garnered much attention lately. Several studies have found Bloor's readings of these earlier works to be problematic. Both Michael Lynch and Michael Friedman have questioned Bloor's particular reading and his use of the philosophy of Ludwig Wittgenstein (Lynch 1992; Friedman 1998; cf. Bloor 1973; Bloor 1983; Bloor 1992). Warren Schmaus' recent analysis, *Durkheim's Philosophy of Science and the Sociology of Knowledge*, similarly takes issue with Bloor's depiction of the work of Emile Durkheim (Schmaus 1994, 18–20, 66–8, 241–3, 254–68; cf. Bloor [1976] 1991, 47–52; Bloor 1982). In the present study of Bloor's readings of Mannheim, I will argue that Bloor's rendering of Mannheim's corpus does not square well with Mannheim's own attempts to clarify the roots, meaning, and purpose of his work. In particular, it seems that Bloor has construed Mannheim's work backwards: as we will see below, it was not that Mannheim was "too weak" to study science, but that the natural sciences appeared to Mannheim both to provide the wrong methodological model and to offer a merely peripheral subject for his sociological program.

Yet to take from these recent critiques of Bloor the single message that Bloor "got it wrong" would not be a very enlightening, or even interesting, conclusion. Instead, by following carefully how Bloor developed his rather curious reading of Mannheim, we may begin to unpack some of the roots of the "strong programme," as it was coalescing in Britain in the mid-1970s. As a first attempt, then, to try to

understand Bloor's divergent reading of Mannheim, we might adopt a distinctly Mannheimian analysis: Mannheim and Bloor were quite differently "situated" within contrasting "perspectives," and hence naturally developed different sociological visions. Mannheim (1893–1947), for example, earned his doctorate in philosophy studying Kantian epistemology and was seated in the midst of 1920s and 1930s debates within the German sphere about the object and role of the *Geisteswissenschaften*. He met weekly during his early years with the other members of Georg Lukács's Budapest "Sunday Circle"; later, after fleeing Budapest following the fall of the short-lived Soviet Republic there in 1919, Mannheim joined the weekly meetings of Alfred Weber and Heinrich Rickert in Heidelberg; and, by the end of the 1920s, he shared office space with Max Horkheimer and Theodor Adorno in Frankfurt. After the Nazi seizure of power, Mannheim, the Hungarian-born son of assimilationist Jewish parents, was again displaced, landing finally in London. In contrast to each of these sets of conversations and dislocations, Bloor's work was colored by his previous training in experimental psychology, by the long-standing empiricist traditions within British sociology, and by the post-World War II emergence of science as a major recipient of government funding and attention.

Although the excavation of Mannheim's and Bloor's socio-historical perspectives could be extended further, I would like instead to give here a closer reconstruction both of Mannheim's work and of Bloor's encounter with it. Mannheim's sociological writings all turned on his particular ideas about the proper epistemological place of the natural sciences with respect to the cultural sciences. As a young student, Mannheim developed an interest in mysticism and literature, rather than chemistry and physics, as beacons for providing meaning in the midst of the fragmentation of modern life. This early aversion to the natural sciences soon took more disciplined form, as he adopted Dilthey's methodological distinctions between the natural and cultural sciences: Mannheim became convinced that the forms of knowledge differed substantially between the two realms. As the clouds of fascism gathered, Mannheim believed that he and his fellow sociologists had a moral obligation to understand the failings of Germany's liberal democracy; what demanded study were historical and cultural movements close to home, rather than seemingly arcane communities of scientists and mathematicians. Thus, by the mid-1930s at the latest, Mannheim had turned his studies away from both the methods and the content of the natural sciences, in what was for him in part an ethical, political move.

Bloor dynamically re-fashioned and re-deployed Mannheim's work in ways which cohered from *his own* new and particular vantage point. This re-deployment of Mannheim's program was significantly shaped by the mediating influence of Robert Merton. I would thus like to paint Bloor's incorporation of Mannheim as part of a *double move*: he was both garnering an image of Mannheim from Robert Merton, and actively spurning Merton's own structural-functionalist program in the sociology of science.

And here the trail of readings and re-readings becomes even more tangled: Robert Merton produced three extended review essays on the work of Karl Mannheim and other early sociologists of knowledge, in which Merton emphasized several specific criticisms of Mannheim's approach. Bloor repeatedly turned to these writings by Merton for an authoritative interpretation of Mannheim's work. Yet at the same time, Bloor and his Edinburgh colleague Barry Barnes were engrossed in efforts to supersede Merton's own program for the sociology of science, as it had emerged in the mid-1940s and crystallized in the 1950s and 1960s. Bloor's resolution was to fix upon just those elements of Mannheim's work which Merton had criticized, and to develop from them a new program for the sociology of scientific knowledge. By "strengthening" the parts of Mannheim's work which Merton had repeatedly portrayed as problematic, Bloor could thus be guaranteed to chart a course different from Merton's own directives for the sociology of science. After disentangling these many intermediate and interrelated steps, we will be able to appreciate how Bloor's seemingly strange deployment of Karl Mannheim cohered in what was for him likely a straightforward and logical way. Although Bloor's reading of Mannheim was different from other possible ways of understanding Mannheim's work, Bloor's interpretation can be read more charitably against the backdrop of this confluence of influences.¹

I do not claim that Mannheim was the only, or even the most important, influence on Bloor's strong programme, nor that the strong programme should be read exclusively as an extended reaction to Mannheim's "failings." Instead, Bloor's explicit treatment of Mannheim offers a window onto background assumptions and the general orientation of Bloor's program as it was taking shape in Edinburgh in the early- and mid-1970s. Bloor's was a heavily scientific program, drawing inspiration and direction from what he saw as the methods of the natural sciences — methods he had practiced as an experimental psychologist, and which shaped his reactions to Merton's Mannheim. Such features come into tighter focus by exploiting a kind of triangulation, contrasting my own reading of Mannheim's writings with the differently-situated readings of Merton and Bloor. To this end, several early sections of this paper are devoted to my own reconstruction of Mannheim's corpus, and of Merton's early reviews of it. With these readings on the table, we may then return to interrogate Bloor's work.

¹ Professor Bloor has recalled in discussion with me that his reading of Mannheim was indeed significantly shaped by his reading of Merton's review essays, along the lines sketched here. One may object that Bloor's evocation of Mannheim (and others, such as Wittgenstein and Durkheim) in his early methodological writings served largely rhetorical purposes, aiming to establish a "legitimate" legacy upon which his own program would build. Although such motives may have played a role in Bloor's selection of "forebears," this alone is insufficient to determine the specific readings and reconstruction of their work which Bloor produced.

Mannheim's Reactions to Modernity: "Authentic Experience" versus Natural Science

In order to avoid a facile *Zeitgeist* reading of Mannheim's work and its deep interrelations with his particular culture and situation, it will be helpful to read Mannheim's essays as elements of a series of continuing conversations or dialogues with his mentors and colleagues.² In some cases, his actual conversations have been preserved, such as his early letters to his Hungarian countryman and role model, Georg Lukács (Gábor 1975; Lukács 1986, 122, 151, 184–91, 197–8, 239–41, 266, 268–9; cf. Sárközi 1986; Gluck 1985, 11–42; Woldring 1986, 12–13). Mannheim's reverence for Lukács is immediately clear from these letters, as for example when the precocious seventeen-year-old, Mannheim, thanks the "elder" Lukács (himself only twenty-five) for his inspiration and intellectual-emotional stimulus, in a letter dated July 3, 1910:

I hope I can convince you that your guidance is invaluable to me at this stage of my development — precisely because it has come from you. ... I know only too well that in the end one has to find one's own way; but I also believe that one human being can help the other, that there is the possibility of learning from one another, and that, when that happens, we receive a precious gift which comes with great responsibility. (Lukács 1986, 122; Gábor 1975, 95)

The young Mannheim shared much with his older friend. In particular, the two Hungarian intellectuals joined a wider group of German social theorists in their common reactions to "modernity."

Ferdinand Tönnies helped to forge the links between "modernity," industrialization, and society in his classic 1887 study, *Gemeinschaft und Gesellschaft* (Tönnies [1887] 1920; see also Ringer [1969] 1990, 164–71; Liebersohn 1988, 11–39).³ Tönnies romanticized the pre-industrialized days when people lived in small, close-knit communities (*Gemeinschaften*); the onset of science-based industrialization for Tönnies meant the end of these more immediate and compassionate types of personal interactions, and brought a shift to cold, de-personalized societies (*Gesellschaften*). Many of Mannheim's colleagues and mentors followed Tönnies in romanticizing this fall into fractured, inauthentic experience.⁴ Max Scheler, in

² Mara Beller has used this technique quite effectively in discussing Niels Bohr's presentation of "complementarity" (Beller 1992; see also Warwick 1992, 1993). For some of the recent scholarship on Mannheim, see (Pels 1996; Kettler and Meja 1995; Ringer 1993; Pels 1993; Meja and Stehr 1990; Longhurst 1989; Woldring 1986; Loader 1985; Kettler, Meja, and Stehr 1984; Remmling 1975).

³ Mannheim was familiar with Tönnies' work at least as early as 1922; he cites *Gemeinschaft und Gesellschaft* throughout his 1922 manuscript, "The Distinctive Character of Cultural Sociological Knowledge," translated and reprinted in (Mannheim [1922] 1982).

⁴ A partial list would include Georg Simmel, Ernst Troeltsch, Max Weber, Georg Lukács, Siegfried Kracauer, Walter Benjamin, Max Horkheimer, and Theodor Adorno. Tönnies' work was emblematic of the German academic mandarins' sense of "cultural crisis" throughout the period 1880–1930. (See, e.g., Ringer [1969] 1990, chapters 3–6; see also Benjamin 1989; Frisby 1986; Jay 1973; Jay 1984; Liebersohn 1988; and Wohl 1979). For a longer view of these discussions, compare

particular, built explicitly upon Tönnies' categories when developing his first attempts at a sociology of knowledge. (Scheler [1926] 1980; Stickers 1980, 7–8) Upon Tönnies's death, Mannheim himself opined that "German sociology could hardly be imagined" without Tönnies' "famous contrast between *Gemeinschaft* and *Gesellschaft*" (Mannheim 1936, 313).

Mannheim's own early and sustained reaction was to turn away from the natural sciences, and to aim to recapture "authentic experience" in the modern age with decidedly un-scientific means. He, too, saw a clear link between lost community and lost authenticity of experience. While a university student in Budapest, on January 5, 1912, Mannheim wrote to Lukács about his hopes for a "great community of us men, our great togetherness," which was "more than all knowledge, because knowledge always springs from doubt, but this is certainty itself, it is experience in the most profound sense, it is provided by the direct order of experience" (Gábor 1975, 96; cf. Lukács 1986, 185). His first turn was to mysticism; when he was eighteen, Mannheim (still a student at the Budapest Gymnasium) sent a copy of an essay he had just completed to Lukács, writing, on March 13, 1911, "I ought to wait and ought not appear in front of you already with the first and still timely document of my life, my essay about the mystics" (Gábor 1975, 95; cf. Lukács 1986, 151). Several months later, January 5, 1912, Mannheim wrote to Lukács to report on his studies of Middle-High-German grammar, which would be necessary to translate the works of Meister Eckhart, the so-called "father of German mysticism" (Gábor 1975, 100; cf. Lukács 1986, 190).⁵ Mannheim's early fascination with Eckhart did not fade quickly; as late as 1922 (four years after having completed his doctorate, *summa cum laude*, in Kantian epistemology), Mannheim cited several specific Eckhart sermons alongside other, more frequently cited academic sources, in his manuscript, *The Distinctive Character of Cultural Sociological Knowledge* (Mannheim [1922] 1982, 43, 131).

Mannheim's search for "authentic experience" also led him to a deep veneration of Dostoevsky. In his rich letter to Lukács from January 5, 1912, Mannheim began to profess his desire to study the works of Dostoevsky, to right the shortcomings of modern society with lessons gleaned from the nineteenth-century novelist: "It is therefore possible for the problems of philosophy, let us say, those of medieval mystics, or of the ancient Indians, to live on, to develop further, to find resolution in the novel or in the life of a Dostoevsky" (Lukács 1986, 185; cf. Gábor 1975, 96). Several pages later in this same letter, Mannheim announced his mission:

(Anderton 1993). It is interesting to compare these social theorists' trope of groping for a lost "authentic experience" with other reactions to "modernity" at the time, such as the morally suspect musings of Ernst Jünger on *Frontierlebnis*, the irreducible, bonding experience of the [World War I] front. (Cf. Herf 1984, 70–74; Theweleit [1978] 1989, 2:180–5).

⁵ Johann Eckhart, known as Meister Eckhart, was a thirteenth-century Dominican priest and professor of philosophy, as well as the "father of German mysticism." Lukács himself was familiar with his works, as his letters from 1910 show, and it is quite possible that Lukács had introduced Mannheim to the works of Eckhart in the first place. See (Gluck 1985, 153–4; Lukács 1986, 143).

I wish to write on Dostoevsky. Not only because I feel that I should be able to pose my problems and my questions through a study of his work, but because I feel that a knowledge of his life promises solutions, since I feel that his life and his world are most akin to ours in all their vicissitudes, all their lack of fulfillment and their distortions. I feel that his fate can provide either negative or positive answers to what disturbs us who are living now. ... I wish to know and I wish to resurrect that cold Petersburg sun in which he walked, which was right then, and the torture of the soul which he felt in a night of which only one can exist, as if it existed now and in me. (Gábor 1975, 98–100; cf. Lukács 1986, 188–9)

Although his planned book on Dostoevsky never came to be, Mannheim carried this early, vivid enthusiasm for the power of literature with him; he chose to study German literature as one of his minor fields for his doctorate. And like his fascination with Meister Eckhart, his hopeful wishes for understanding the modern condition through Dostoevsky were also shared, and perhaps inspired, by Lukács himself. Eight months before Lukács received this impassioned letter from Mannheim, he himself had written to a friend, "For a long time I thought life consisted in this lack of inward harmony. Today I no longer believe that. To put it simply: read Dostoevsky and you will see what I mean. Now I see life as sharply and clearly as literature."⁶ For both of these intellectuals, literature appeared to provide the immediacy and authenticity that seemed so threatened by life in the modern age.⁷ Reading Dostoevsky, not fumbling in a chemistry laboratory, was what seemed to these young scholars to promise salvation in the post-Lapsarian age of industrialization.

Dilthey and the Limitations of the Methods of the Natural Sciences

As Mannheim grew older and began to study the work of other German social theorists, his alienation from the natural sciences took on a new dimension. Mannheim began to draw on the work of one of Lukács's Berlin professors, Wilhelm Dilthey. In his writings on the *Geisteswissenschaften* (often translated as "human" or "cultural" sciences), Dilthey set out to demarcate a set of disciplines that were fundamentally different from the natural sciences, and conducted with an independent methodology. The ultimate basis for knowledge in these human sciences, wrote Dilthey, was "lived experience" (*Erlebnis*). In the preface to his

⁶ Lukács to Irma Seidler, April 18, 1911. Quoted in (Liebersohn 1988, 184). This letter is reprinted with a slightly different translation in (Lukács 1986, 154–7). Lukács's later book, *Theory of the Novel* (1920), was intended to serve as an introduction to a larger study of Dostoevsky's work, although this second book never materialized. See (Gluck 1985, 184–93; Liebersohn 1988, 186). Mannheim reviewed Lukács's *Theory of the Novel* in (Mannheim [1920] 1971).

⁷ Fruitful comparison could be made here with the attitudes of American writers at that time, such as Sherwood Anderson. See (Lears 1993). For more on the general "anti-modernism" in America at the turn of the century, see (Lears 1981).

1883 *Introduction to the Human Sciences*, he made his break with the Enlightenment epistemologists clear:

Earlier epistemology, both empirical and Kantian, explains experience and knowledge on the basis of a framework which is purely ideational. There is no real blood flowing in the veins of the knowing subject fabricated by Locke, Hume, and Kant, but only the diluted juice of reason as mere mental activity. But dealing with the whole man in history and psychology led me to take the whole man — in the multiplicity of his powers: this willing-feeling-perceiving being — as the basis for explaining knowledge and its concepts. (Dilthey [1883] 1988, 73)

Dilthey's romantic view of the role of primordial, authentic experience as the basis for the study of people, their history, and their societies, was certain to impress Mannheim, who was already well-versed in Eckhart and Dostoevsky. Dilthey went on to distinguish between the forms of knowledge in the natural sciences and in the human sciences: the natural sciences necessarily were restricted to "explanation" (*Erklärung*), while the human sciences could penetrate deeper, and yield a fuller, idealistic or transcendental "understanding" (*Verstehen*) (Dilthey [1883] 1988, 97–9; cf. Wirth 1937; Makkreel 1975; Ermarth 1978; Bulhoff 1980; Betanzos 1988). Mannheim cited this distinction of Dilthey's in his 1922 manuscript, and incorporated it in all of his later writings on the sociology of knowledge.

Mannheim's study of Dilthey followed in the aftermath of German social scientists' *Methodenstreit*, during which scholars in the 1880s and 1890s attempted to balance the older, historical-interpretive methods with more positivist, scientific approaches (Ringer [1969] 1990, 301–4; Frisby 1976, xv–xxvii). Perhaps most important for Mannheim on this question of methods were debates which began in the 1890s over Dilthey's own approach to the *Geisteswissenschaften*. The Baden, or southwest-German school of neo-Kantians, which was centered around Wilhelm Windelband and his student Heinrich Rickert, disliked Dilthey's romantic, supra-rationalist fascination with mystical "lived experience." With Georg Simmel, they followed Dilthey in drawing a distinction between the natural and human sciences, but they viewed this distinction as one of taxonomy, not ontology; they thought Dilthey had gone too far in separating the very worlds described by the two types of scholarship, instead of seeing the two studies as simply focusing on different aspects of the same world (Windelband [1894] 1980, especially 174–5; Rickert [1902] 1986, 8–9, 17; cf. Oakes 1988, Ringer [1969] 1990, 311–2; Bulhoff 1980, 31–2). Although Mannheim familiarized himself with Rickert's work, and in fact met regularly with him throughout 1920, he ultimately sided with Dilthey.⁸

For Mannheim, as for Dilthey, the crux of the difference between the natural

⁸ Mannheim cited Rickert's *The Limits of Concept Formation in the Natural Sciences* throughout his 1922 manuscript (Mannheim [1922] 1982). Rickert had attended regularly a private seminar given by Mannheim in 1920 in Heidelberg, entitled "What is Sociology?" (Woldring 1986, 21).

sciences and the human sciences was more than simply a logical question of taxonomy, or even of methods. It was a question of the ultimate foundation of the knowledge gained in the two areas. Dilthey's distinction between the *Erklärung* of the natural sciences and the *Verstehen* of the human sciences had been an evaluative one: the human sciences, with their firm grounding in empathetic, lived experience, promised knowledge of the world that was fundamentally more important than that gleaned in the natural sciences. Mannheim followed Dilthey in making this evaluation, and surpassed Rickert's merely methodological distinction. He pronounced this openly in several places. In his 1924 essay on "Historicism," Mannheim elevated the intuitive reach of historical knowledge over the limitations of the positivist natural sciences:

Historians indeed may grasp past epochs from those epochs' own centres, a mode of interpretation called *the immanent critique and representation of the past*. This is possible through "understanding" (*Verstehen*) as an intuitive faculty of the historian which enables him to penetrate into his subject-matter, into the concrete valuations of the epochs in question, to a degree which is denied us when we are dealing with nature. (Mannheim [1924] 1952, 105; emphasis in original)

He continued this argument four years later, this time joined by Werner Sombart, at the Sixth Congress of German Sociologists, held in Zurich in 1928 (Meja and Stehr 1990, 91, 106). As Mannheim would write several years later, a blind attention to "calculable external facts" could never render an understanding of the historical-social perspectives necessary for an empathic sociology of knowledge (Mannheim [1934] 1953, 215–6).

The methods and forms of knowledge in the natural sciences failed to impress Mannheim for problems dealing with people's culture and history; as he began to compose his first essays on the sociology of knowledge, therefore, Mannheim sought to chart a distinct methodological course for the social sciences. In concluding his 1921 essay "On the Interpretation of *Weltanschauung*," for example, Mannheim noted approvingly that "we detect a gradual emancipation from a methodology oriented entirely on the natural sciences" (Mannheim [1921/22] 1952, 82). Soon thereafter, Mannheim announced that "the positivistic and Kantian conceptions of science, which took exact natural science to be the sole ideal prototype to which all sciences, including the cultural sciences have to conform, ... was doomed to failure" when treating such disciplines as sociology (Mannheim [1924] 1952, 126). He later decried the "fact that the natural sciences have been selected as the ideal to which all knowledge should aspire," and argued that "the model of modern mathematical-natural science cannot be regarded as appropriate to knowledge as a whole" (Mannheim [1929] 1936, 290, 166). Over the course of the 1920s, Mannheim concluded that the social and cultural sciences could not be pursued with methods borrowed from the natural sciences.

Mannheim's ultimate grounding for his sociology of knowledge thus sprang

from his early reactions to “modernity”: with his mentors Lukács and Dilthey, Mannheim sought a means of recapturing “authentic experience.” His early studies of mysticism, literature, and the work of Wilhelm Dilthey convinced him that the answers lay beyond the reach of natural-scientific methods. His own answers were formulated throughout the mid-1920s, culminating in his 1929 *Ideologie und Utopie*. Drawing on his reaction to “modernity,” Mannheim performed a “Copernican Revolution”: the natural sciences occupied merely a peripheral space in his apportionment of the social and intellectual cosmos, and were not the center around which epistemology and sociology should revolve. This maneuver came in tandem with Mannheim’s particular incorporation of the Kantian legacy.

Kant’s Legacy and Mannheim’s “Objective Perspectivism”

Along with his common reaction against the fragmentation of life in the modern age and his search for “authentic experience,” Mannheim shared with his Hungarian and German colleagues a sustained interest in the study of Immanuel Kant’s works. Mannheim’s entire circle of mentors and colleagues shared in this inherited legacy: Wilhelm Dilthey studied under Friedrich Adolf Trendelenburg, one of the earliest figures responsible for the rise of neo-Kantianism; Dilthey himself labored over his own associations with Kant, filling his *Introduction to the Human Sciences* with discussion of his similarities and differences with the famed Königsberg professor (Dilthey [1883] 1988, 44–51, 157, 262–3, 322–3; cf. Betanzos 1988, 10, 18–21; Köhnke [1986] 1991, 11–35). Wilhelm Windelband and Heinrich Rickert were self-proclaimed neo-Kantians. Max Horkheimer had written both his dissertation and habilitation on Kant’s epistemology, and Theodor Adorno had studied Kant’s *Critique of Pure Reason* with Siegfried Kracauer (Jay 1973, 6–7, 21–2).

Mannheim began his study of Kant’s works at an early age. Interspersed between his reports to Lukács about his desire to write on Dostoevsky and his efforts to translate Meister Eckhart, Mannheim proudly explained in his January 5, 1912, letter to Lukács that he had enrolled in Bernát Alexander’s seminar on Kant at the University. The excited nineteen-year-old exclaimed, “I am progressing slowly in my study of Kant, but — I believe — really understanding him. I have already linked three longer trains of thought to this study, which will perhaps form the foundation of a paper on Kant I may write” (Gábor 1975, 100; cf. Lukács 1986, 190).⁹ Mannheim continued his studies of Kant’s epistemology, completing his dissertation on the topic in 1918. A portion of his dissertation was published that

⁹ In this letter, Mannheim thanked Lukács for his friendly introduction to Professor Alexander, and attributed Alexander’s kindness towards Mannheim to Lukács’ intervention. Lukács’ and Alexander’s relationship is treated in (Gluck 1985, 82–3).

year in the Hungarian journal *Athenaeum*, and later published in the German journal *Kant-Studien* (Mannheim [1918, 1922] 1953). Indeed, even as his studies turned to sociology, Mannheim maintained an interest in Kant; he attended the 1933 Frankfurt meeting of the *Kant Gesellschaft* (Woldring 1986, 35). The 1922 publication of Mannheim’s dissertation in *Kant-Studien* coincided with his drafting of an essay on cultural-sociological knowledge that was published posthumously. Much as his early letter to Lukács showed the ease with which Mannheim could intermingle discussions of Dostoevsky, Eckhart, and Kant, so too did his more mature work emerge with these three strands intimately intertwined.

Overall, Mannheim’s reaction to the Kantian legacy was typical of many scholars within the German sphere at the turn of the century. By the end of the nineteenth century, Kant’s epistemology was often analyzed by these intellectuals as *separable*: some aspects were emphasized while others were downplayed or ignored (see, e.g., Höffding [1894] 1908, 2:29; cf. Kaiser 1992). These sorts of distinctions were institutionalized within Germany, with the Marburg school of neo-Kantianism focusing on the limitation of knowledge to phenomena, and the Baden, or southwest-German school focusing on Kant’s idealism (Ringer [1969] 1990, 305–11; Köhnke [1986] 1991, 176–8, 261–3). Mannheim, too, treated Kant’s work as separable, and chose to emphasize the limitation theme in constructing his own sociology of knowledge.

In his *Critique of Pure Reason*, Kant discussed how to guarantee the objective reality of a given judgment. A judgment may be deemed unconditionally and objectively real, he wrote, if “we add to the concept of the subject of a judgment the limitation under which the judgment is made” (Kant [1787] 1929, 72 (A27/B43); emphasis added). Elsewhere I have termed this Kant’s method of “conceptual containment”: by including within the concept itself the conditions and limitations under which the concept is made, then any judgment concerning the (contained) concept will be objectively real (Kaiser 1992, 218–20). For Kant, such containment was necessary for solving the problems of metaphysics: we would be deluding ourselves if, neglecting conceptual containment, we attempted to gain knowledge of things in themselves: “If I ascribe redness to the rose in itself, or extension to all objects in themselves, without paying regard to the determinate relation of these objects to the subject, and without limiting my judgment to that relation, illusion then first arises” (Kant [1787] 1929, 89 (B70)). This limitation of judgments to the specific conditions of their validity formed the kernel of Kant’s response to skepticism and relativism.

Throughout his dissertation on Kantian epistemology, Mannheim too emphasized the constitutive “ultimate presuppositions” that undergird all knowledge. For example, he wrote there that “however isolated it may seem, a concept still has systematic presuppositions” (Mannheim [1918, 1922] 1953, 19). In fact, he defined epistemology itself as pursuing two tasks: “firstly, to analyze all possible knowledge with a view to laying bare its ultimate presuppositions, and secondly, to ascertain whether these ultimate presuppositions can guarantee the cognitions grounded in

them to be true knowledge — in other words, to evaluate those ultimate presuppositions” (Mannheim [1918, 1922] 1953, 65; see also 47). And like Kant’s conceptual containment, Mannheim noted that every concept “has a place where it properly belongs, and that it will show at once if it is ‘transferred’ into an alien sphere, where it can only be applied ‘metaphorically’” (Mannheim [1918, 1922] 1953, 23). The epistemologist, then, had to study the proper systematization of concepts with a view to criticizing concepts that were applied outside of their prescribed, limited realm of legitimate application. When he turned to study the sociology of knowledge immediately after publishing this long epistemological study, Mannheim adopted an approach that was structurally equivalent to the twin tasks he had just adumbrated for the epistemologist.¹⁰ And like Kant before him, Mannheim was confident that this approach guaranteed that the sociology of knowledge would not slide into relativism.

In developing his method for the sociology of knowledge, “objective perspectivism,” Mannheim isolated two uses of the word “ideology.” The first was “particular,” operating on a psychological level of “interests”: an opponent cannot see another side on a particular position because he or she is blinded by interests on the specific issue. The second, Mannheim termed the “total” use of ideology. Total ideology questions the opponents’ entire *Weltanschauung*, “including his conceptual apparatus.” It becomes more than simply a question of misunderstanding a particular, isolated point of contention, but rather the entire means of viewing and understanding the world are at odds (Mannheim [1929] 1936, 56–8). For Mannheim, analyses of ideologies in the particular sense remained too focused on the specific thoughts of individual subjects to aid the sociologist; but with a thorough use of the total conception of ideology, “the simple theory of ideology develops into the sociology of knowledge.” This thorough use requires that the “analyst ... has the courage to subject not just the adversary’s point of view but all points of view, including his own, to the ideological analysis” (Mannheim [1929] 1936, 59, 78–9). Mannheim gave this conclusion an explicitly Kantian telling in an essay from 1931, writing that “truth in itself,” like Kant’s “thing in itself,” could never be an object of objective knowledge apart from consideration of the embedding *Weltanschauung* (Mannheim [1931] 1936, 306). Much like the epistemologist studies concepts embedded within specific realms of legitimate application, the sociologist of knowledge hence should study ideas as springing from within specific worldviews and contexts. In this way, Mannheim’s sociology of knowledge sprang from a critical, reflexive acknowledgment of the sociologists’ own cultural background and place in history.

This feature of Mannheim’s sociology thus shared in the early twentieth century traditions of historicism: historical actors’ specific thoughts and actions were

¹⁰ Mannheim cited his 1922 *Kant-Studien* article throughout many of his early essays on the sociology of knowledge (see, e.g., Mannheim [1924] 1952, 93; Mannheim [1926] 1990, 45; Mannheim [1928] 1990, 82).

considered inseparable from their historical contexts and settings (Mannheim [1924] 1952; cf. Rossi 1975, 16–19). Yet for Mannheim, this historicist attention to placement in history did *not* lead to the relativism that is usually associated with the historicist movement. It was the critical reflexivity on the sociologists’ part that Mannheim believed could distinguish between the error of relativism and the need for what he termed “relationism.” Nearly every one of Mannheim’s writings during the 1920s includes an explicit repudiation of relativism (Mannheim [1918, 1922] 1953, 39–41; Mannheim [1924] 1952, 93, 104–5, 120–3, 127–30; Mannheim [1925] 1971, 97; Mannheim [1928] 1990, 55).

Mannheim posited what he termed “relationism” and “perspectivism” in place of relativism: knowledge arises within given historical-social contexts, and these contexts (or “perspectives”) cannot be ignored when discussing specific ideas. Mannheim repeatedly drew upon the metaphor of visual perception and perspective when discussing his program of sociology:

The controversy concerning visually perceived objects (which, in the nature of the case, can be viewed only in perspective) is not settled by setting up a non-perspectivist view (which is impossible). It is settled rather by understanding, in the light of one’s own positionally determined vision, why the object appeared differently to one in a different position. (Mannheim [1931] 1936, 301; cf. Jay 1993)

In this way, relationism led to “a new type of objectivity,” parallel to the example of visual perspective, by including an examination of the perspective from which a body of ideas was developed along with a study of the ideas themselves (Mannheim [1929] 1936, 5; cf. Mannheim [1931] 1936, 282–3, 296–7, 301). Again relying on a visual metaphor, Mannheim connected the notions of reflexivity, perspectivism, and objectivity in a passage from “The Problem of a Sociology of Knowledge”: “human consciousness can grasp a landscape *as landscape* only from various perspectives; and yet the landscape does not dissolve itself into its various possible pictorial representations. Each of the possible pictures has a ‘real’ counterpart and the correctness of each perspective can be controlled from the other perspectives” (Mannheim [1925] 1971, 97; emphasis in original). Here we see the importance for Mannheim of Kant’s example of conceptual containment: objectivity was indeed still possible for the sociologist, as Kant believed it had been for the epistemologist, so long as a historicist attention to contexts was treated as we treat visual perspectives every day. For Kant, concepts were always bounded within specific limited areas of application, and overstepping these bounds led to the chimerical crises of metaphysics. For Mannheim, concepts, like visions of landscapes, were limited to specific worldviews or perspectives, and likewise should not be pressed beyond their legitimate bounds of application. The sociologist could then study such concepts along with their larger, embedding worldviews, always keeping in focus that such studies themselves sprang from a particular position. It was this last, reflexive point which was crucial for Mannheim’s crusade for objectivity.

Mannheim's seemingly facile recovery of objectivity became a frequent target for many of his critics. In a review of Mannheim's 1929 *Ideologie und Utopie*, for example, Max Horkheimer, the new director of the Institut für Sozialforschung, chided Mannheim (at the time his Frankfurt neighbor) for claiming prematurely to have solved the problems of relativism and objectivity. To Horkheimer, Mannheim's perspectivist approach was merely a simplified and naive Gestaltist approach to epistemology (Horkheimer [1930] 1993; cf. Meja and Stehr 1990; Bailey 1994; Jay 1974). Why, then, did Mannheim insist so strongly that his sociology of knowledge was objective? As we will see in the next section, objectivity for Mannheim carried significant ethical overtones. Fighting to cope with life in the modern age, Mannheim believed that giving up on the quest for objectivity would have been both cowardly and disastrous.

The Ethics of Mannheim's Objectivity and the Poverty of Scientism

From his early student years as a member of Lukács's Budapest "Sunday Circle," Mannheim's work had always developed in the midst of explicitly political considerations. Much as was occurring within German academic circles with the *Werturteilsstreit*, or debate over the place and role of value-judgments within sociological studies, many Hungarians saw their intellectual efforts becoming increasingly politicized throughout the 1910s (Gluck 1985, 90–4, 99–101; cf. Weber [1904] 1949; Weber [1917] 1949; Pels 1993, 47–52; Ringer [1969] 1990, 146–62; Proctor 1991, 85–98, 134–54; Frisby 1976; Käsler [1979] 1988, 184–97). Béla Balázs, Lukács's partner in organizing the Sunday Circle, had even confided in his diary in 1912 his belief that the group should organize formally their own political party (Gluck 1985, 18). Though Mannheim did not become as politically active as his role model Lukács did during the short-lived Hungarian Soviet Republic in 1918–1919, Mannheim was labeled "a flag-bearer of the criminal régime" by the reactionary overthrowers of the Republic, and forced to flee Budapest for Heidelberg (Gábor 1975, 104; Woldring 1986, 16–21). The entire Sunday Circle was labeled by Admiral Horthy's administration as "a special and particularly dangerous secret political organization" (Gluck 1985, 211).

Upon landing in Heidelberg, Mannheim soon found himself ostracized by his own Sunday Circle comrades. According to Balázs' diary, Mannheim's request to join the displaced Sunday Circle, then meeting in Vienna, was rejected because he had not followed the group along its radical-political Marxist turn (Kettler, Meja, and Stehr 1984, 38; cf. Gluck 1985, 210–11). The question of Mannheim's relationship with Marxism in fact dominated his early reception amongst German and Hungarian critics. Alfred Weber, who had participated in a raucous debate over the proper place of Marxism in academic sociology at the 1924 Congress of German Sociologists, queried Mannheim at the 1928 Congress whether "all this [is] anything more than a brilliant rendition of the old historical materialism,

presented with extraordinary subtlety?" Werner Sombart, however, leaped to Mannheim's defense, arguing that the Marxist historical materialism "denies the objectivity of existence and the reality of the spirit while Mannheim affirms them," and Mannheim, keen to emphasize the need for such objectivity, agreed with this assessment (Kettler and Meja 1995, 93; cf. Pels 1993, 52–5; Meja and Stehr 1990, 86–92, and Ringer [1969] 1990, 227–30).

A few years prior to this exchange, Mannheim himself had mused that "in periods like ours, in which reflexivity, and a many-sided relativism are reducing themselves to absurdity, as it were, a fear grows up instinctively about where all this will lead" (Mannheim [1925] 1986, 180). Mannheim soon found vindication for these fears with the fall of the Weimar Republic in 1933, and his second forced emigration, this time from Frankfurt to London.

Writing in exile merely months after the rise of Hitler's Reich, Mannheim made clear the problems with relativism which he sought to supersede with his objective perspectivism: "Relativism would mean that there are no objective values, *therefore* moral obligation cannot exist. Relationism, on the other hand, stresses the fact that there *is* a moral obligation, but that this obligation *is derived from the concrete situation to which it is related*" (Mannheim [1934] 1953, 212; cf. Mannheim [1924] 1952, 84; emphasis in the original). It was this "concrete situation" which was of most concern for Mannheim in these years. Three years earlier, in a passage he would later include in the English edition of *Ideology and Utopia*, Mannheim had linked his methodological turn away from the methods of the natural sciences with the surrounding political world of the sociologist: "At a moment when historical-social forces place *other types of knowledge in the centre of the arena* it is necessary to revise the older premises which had been, if not exclusively, at least to a large extent formulated for the understanding and justification of the natural sciences" (Mannheim [1931] 1936, 290–1; emphasis added). Mannheim's "Copernican Revolution" thus meant more than simply an academic's musings on the foundations of knowledge. It was a self-consciously political, ethical move on Mannheim's part: scientism alone could not answer the crises of the day.

In March 1934, Mannheim delivered an impassioned lecture on these themes at the University of London. He began by exhorting his audience: "I believe that our society has reached a decisive turning-point in its history, and that in situations such as confront us to-day we must have both the will and the vision to take an all-inclusive view of society and its historical background" (Mannheim 1934, 3). Describing their age, which had just witnessed "the mushroom-like rise of dictators around us," Mannheim painted a picture of the march of scientific and technological progress, from wheelbarrows, to horses and carriages, to cars and airplanes, and quickly turned to some of the horrors attending this technology (Mannheim 1934, 8). Anticipating by several years the air battles over London, Dresden, and Tokyo, Mannheim asked his listeners to imagine a pilot who, using "the most up-to-date results of technical ingenuity," "hurls a hurricane of bombs and ... lays

waste everything and annihilates everybody underneath him”:

In his scientific and technical knowledge man has, indeed, made miraculous strides forward in the span of time that separates us from the days when the carriage came into use; but is human reason and rationality, in other than the technical field, to-day so very different from what it was in the distant past of which the wheelbarrow is a symbol? (Mannheim 1934, 3–5)¹¹

As Mannheim emphasized with italics, this state of affairs signaled a “*disproportionate development* of human faculties”: “the development of man’s technical powers over nature is far ahead of the development of his moral faculties and his knowledge of the guidance and government of society” (Mannheim 1934, 5). In fact, Mannheim feared that individuals suffering from such disproportionate development were those most likely to be able to use the modern media to spread demagogic propaganda and enroll the masses (Mannheim 1934, 34–5). With “contemporary society” thus threatened by such a “catastrophic” disproportionality, Mannheim here first exclaimed explicitly that it was not only a mimicry of the *methods* of the natural sciences that was dangerous (Mannheim 1934, 5). So too was a restricted attention to the *content* of the natural sciences, to the detriment of the *Geisteswissenschaften*.

Upon the heels of his forced emigration from the Nazi regime, then, Mannheim argued that it was “urgent” for sociologists to act quickly to right this imbalance (Mannheim 1934, 11). To his early devaluation of the methods of the natural sciences he now added the obligation to study mankind’s “moral faculties” and means for “government of society” in place of the “technical powers over nature.” By the mid-1930s, Mannheim’s alienation from the natural sciences was thus complete: a restricted attention to the natural sciences, or worse to the even more arcane natural scientists, could never point the way to coping with the present political-cultural crises.¹²

With his purposeful de-centering of the natural sciences and his faith in objective perspectivism in mind, we may now make sense of Mannheim’s reactions to American sociology during the 1920s and 1930s. In a 1932 review of a collection of methodological essays by American sociologists, Mannheim let his feelings on the

¹¹ Mannheim’s position here can be contrasted with the famous introduction to Max Horkheimer’s and Theodor Adorno’s 1947 *Dialectic of Enlightenment*, in which they struck a far more thoroughly pessimistic note about the inevitability of modern technology to lead to the carnage of Auschwitz. Upon the conclusion of World War II and the Holocaust, Horkheimer and Adorno wrote that “the fully enlightened earth radiates disaster triumphant” (Horkheimer and Adorno [1947] 1989, 3; cf. Herf 1984, 9–10). In 1934, Mannheim was still hopeful that the problems, once identified, could be rectified.

¹² Of course, Mannheim’s politically-loaded turn away from the natural sciences was not the only option taken by scholars within the German sphere at this time. In the same year that Mannheim published his *Ideologie und Utopie*, the Vienna Circle published its *Wissenschaftliche Weltauffassung* manifesto. Members of the Vienna Circle hoped to base all knowledge and political action on strictly natural-science-based methods, to eliminate all vestiges of dangerous metaphysics (see the 1929 “The Scientific Conception of the World: The Vienna Circle,” in Neurath 1973, 299–318; cf., e.g., Cartwright, Cat, Fleck, and Uebel 1996; Uebel 1991; Proctor 1991, 163–181; Galison 1990; Galison 1996).

limitations of the methods of science be known: “American sociology seems to yield too much to the fascination of natural science” (Mannheim [1932] 1953, 190).¹³ The American insistence upon “empirical” studies within sociology left Mannheim cold:

We must admit a very marked and painful disproportion between the vastness of the scientific machinery employed and the value of the ultimate results. The subject and title of most of the contributions evoke the highest expectations; yet, after having reached their conclusions, one is tempted to ask, disappointedly, “Is this all?” (Mannheim [1932] 1953, 187)¹⁴

Two years later, Mannheim reiterated his point: Sociology built on this “empiricist” model, like the natural sciences it sought to emulate, remained unable to penetrate to the depths of qualitative understanding (Mannheim [1934] 1953, 224–5). For Mannheim, the Americans suffered from a severe case of methodological scientism, springing from, and ultimately limited by, the notion that the natural sciences should be central to all other scholarly pursuits.

Mannheim also keenly sensed the differences between his own culture and work and that of his British colleagues. Three years after his re-location to London, he collaborated with two Chicago sociologists on an English translation of his 1929 *Ideologie und Utopie*. Unlike the original German edition, which contained only three chapters and ran 250 pages, the 1936 English edition included five chapters and totaled 350 pages. The fifth chapter was a translation of Mannheim’s 1931 essay, “Wissenssoziologie,” but the first chapter was a 50-page introductory essay prepared by Mannheim especially for the English edition. His translators, Louis Wirth and Edward Shils, at first objected that the new introduction would not be necessary. Yet Mannheim remained resolute. In a particularly revealing letter to Wirth, dated February 15, 1936, Mannheim reiterated why the new “Preliminary Approach to the Problem” was a necessary addition to *Ideology and Utopia*. He feared that “the old book would simply appear as a document out of a time inaccessible to” his intended Anglophone readers. Furthermore, Mannheim complained that in Great Britain,

¹³ This essay was a review of *Methods in Social Science* (Rice 1931). This example parallels the gulf separating German and American traditions of historical scholarship at the time, as discussed by Peter Novick: many American historians saw in Leopold von Ranke’s “wissenschaftliche Objektivität” a mandate for empirical, scientific history, which Novick argues was almost entirely opposite to Ranke’s own brand of essentialist, humanistic scholarship (Novick 1988, 24–31; cf. Igger 1962).

¹⁴ Mannheim’s 1932 critique highlights a different periodization of American sociology from that presented in (Kuklick 1983). Kuklick writes that “Before World War II the intellectual climate of American sociology was congenial to the growth of a sociology akin to Mannheim’s. Yet in the postwar period American sociologists committed themselves to ahistorical theory, positivist methodology, and team research: their ‘scientistic’ sociology did not permit the historicism, relativism, and holism necessary to Mannheimian analysis” (Kuklick 1983, 287; cf. 289–292). Obviously, Mannheim himself thought otherwise, and didn’t hesitate to publish his views in the pages of *The American Journal of Sociology*! For more on the turn of American sociologists to “scientistic” methods, see (Shils 1970, 770–1, 781–2; Heims 1991, chapter 1; Ross 1991, 390–470; Ross 1994, 187–9; Bannister 1987). The long-standing tradition of scientism within Anglo-American philosophy is treated in (Sorell 1991).

there is, as it were, absolutely no tradition of sociology in existence, and by the term one either means "social surveys" or "descriptions of social situations." People have no clear conception here of an empiricism which could also be something more than simply tallies, statistics, and inventories. Something introductory must be said about these matters, to make the path of the reader generally secure. If the book had begun so that the reader would immediately fall into the definitions of total and particular ideologies, then practically every Englishman after a few pages of reading would have put down the book as "too abstract." So I believe that both your introduction and the new opening of the book involves the reader in a context of problems in a concrete form, so that, when once the reader is interested in these matters, he then comes to enjoy a great deal of what his tradition did not provide him adequately.¹⁵

Mannheim was painfully aware of the tremendous challenges facing "translation" of his work from Germany to England, both literally and more broadly considered: as he feared, a straightforward translation of his original text would have appeared "inaccessible to them," stemming as it did from a very particular culture and set of dialogues.¹⁶ The British, like the Americans, appeared to Mannheim to suffer from a shallow, scientific, positivist tradition, which could only recognize "tallies, statistics, and inventories" as the tools for a sociologist. As it turned out, Mannheim's 1936 anticipation of Anglo-American reactions to *Ideology and Utopia* proved to be rather prophetic.

Before turning to Bloor's own reactions to Mannheim's oeuvre, let us examine closely a principal introduction of Mannheim's work to many of the American and British sociologists whose work Mannheim himself had criticized so severely: Robert Merton's reviews of the sociology of knowledge.

Merton on Mannheim

A few months after the English edition of *Ideology and Utopia* was published in 1936, Robert Merton produced his first review and critique of the sociology of knowledge. The review was published in *Isis*, then under the direction of his

¹⁵ This letter, Mannheim to Louis Wirth, February 15, 1936, is reprinted and translated in (Woldring 1986, 404–5).

¹⁶ Mannheim's collaboration with Wirth and Shils to translate *Ideology and Utopia*, and his often difficult relations with his newfound British colleagues, are treated in much detail in (Kettler, Meja, and Stehr 1984, 107–119; Kettler and Meja 1995, 176–188, 193–246). In particular, Kettler, Meja, and Stehr point out that Mannheim directed the translation into English of *Ideologie und Utopie* as more than a simple linguistic translation, actively changing phrasings in a pre-emptive effort better to fit the book into the British traditions in social theory. For example, the distinct notions in the German edition of *Seinsgebundenheit* and *Seinsverbundenheit* were collapsed in the English version, which drew on "the psychological frames of reference of English post-utilitarian philosophy of mind [and on] American pragmatism" (Kettler, Meja, and Stehr 1984, 114).

mentor, George Sarton. Although this essay purported to provide a "general survey of the subject," more than two-thirds of the essay concentrated specifically on Mannheim's *Ideology and Utopia* (Merton 1937). In December of the same year, Merton's other advisor, Talcott Parsons, organized a session at the annual meeting of the American Sociological Society, dedicated entirely to discussion and quite extensive critique of Mannheim's *Ideology and Utopia* (Kettler and Meja 1995, 220–5). Merton's 1937 review was followed by a longer essay in 1941, entitled "Karl Mannheim and the Sociology of Knowledge" (Merton [1941] 1957). For this article, Merton consulted twelve distinct sources from Mannheim's developing oeuvre on the sociology of knowledge, tracing the development of his thought from the 1923–4 essays through to the English edition of *Ideology and Utopia*. Finally, in 1945, Merton again returned to the subject with another essay entitled simply "The Sociology of Knowledge" (Merton [1945] 1973).

It was this final essay upon which Bloor relied for an introduction to and interpretation of 1930s sociology of knowledge. Throughout Bloor's article, "Wittgenstein and Mannheim on the Sociology of Mathematics," this work by Merton was the only secondary source on Mannheim to which Bloor turned. He cited Merton's 1945 essay, as it was reprinted in the 1968 edition of *Social Theory and Social Structure*, calling the latter an "authoritative textbook" (Bloor 1973, 174, note 4).¹⁷ A few pages later, Bloor returned to this same essay of Merton's for an explanation of the work of Max Scheler, conceding that it is Merton "from whom this account of Scheler is taken" (Bloor 1973, 179, n. 14). In Bloor's 1976 *Knowledge and Social Imagery*, he again cited Merton's *Social Theory and Social Structure*, along with the reprint of Merton's 1945 review essay, this time as included in the 1973 collection, *The Sociology of Science* (Bloor [1976] 1991, 192). In order to unpack Bloor's particular reading of Mannheim, then, we should examine carefully the package in which he received his dosage of Mannheimianism: the treatments by Merton (cf. Lynch 1993, chapters 2–3).

In each of his three review essays, Merton emphasized the same criticisms of the sociology of knowledge. His principal complaint was that the work by Durkheim, Mannheim, Scheler and others did not pay careful attention to different *types* of knowledge. In his 1937 *Isis* review, Merton concurred with another critic of Mannheim's *Ideology and Utopia*, that the work suffered from a "serious confusion of essentially different spheres." The trouble with such confusion was that the sociological analyses which might be pertinent for one realm of knowledge might not allow easy generalization (Merton 1937, 501). In the 1941 essay specifically on

¹⁷ Although Bloor did not cite Merton's 1941 essay, "Karl Mannheim and the Sociology of Knowledge," it appeared directly after the reprint of the 1945 essay in the collection, *Social Theory and Social Structure*, from which Bloor read the 1945 essay. Considering Bloor's explicit interest in Mannheim, then, it is likely that Bloor was at least familiar with the 1941 essay as well; although, as discussed below, all three of Merton's essays repeated the same criticisms, so that Bloor's knowledge of the 1945 essay alone would have introduced him to the same points and emphases as would be found in Merton's earlier essays.

Mannheim's work, this criticism was expanded. Under the boldface subheading, "Types of Knowledge," Merton again alerted his readers that

It will be noted that the foregoing theorems pertain less to positive knowledge than to political convictions, philosophies of history, ideologies and social beliefs. And this at once opens a basic problem. Which spheres of "thought" are included in Mannheim's theses concerning the existential determination (*Seinsverbundenheit*) of thought? Precisely what is embraced by the term "knowledge" to the analysis of which the discipline of *Wissenssoziologie* is nominally devoted? For the purposes of this discipline, are there significant differences in types of knowledge? (Merton [1941] 1957, 496)

Merton quickly proceeded to answer his many questions with quite definite answers: "Mannheim does not meet these issues specifically and at length in any of his writings." The problems which this engendered for Mannheim's entire program were obvious to Merton: "His failure in this respect introduces serious discrepancies between some of his theorems and specific empirical inquiries. ... The identification of different types of inquiry by subsuming them under one rubric serves only to confuse rather than to clarify the mechanisms involved in 'existential determination'" (Merton [1941] 1957, 496-7).

Readers of Merton's widely reproduced 1945 essay, such as David Bloor, once again encountered this criticism of Merton's, following as before under the subheading "Types of Knowledge." Citing his 1941 essay, Merton delivered his verdict: "Even a cursory survey is enough to show that the term 'knowledge' has been so broadly conceived as to refer to every type of idea and every mode of thought ranging from folk belief to positive science." And again the problem with this approach was made clear: "The question is, of course, whether these diverse kinds of 'knowledge' stand in the same relationship to their sociological basis, or whether it is necessary to discriminate between spheres of knowledge precisely because this relationship differs for the various types." For the work of Marx, Engels, Scheler, Mannheim, Durkheim, and Sorokin, Merton could only conclude as he had in both his 1937 and 1941 reviews: "For the most part, there has been a systematic ambiguity concerning this problem" (Merton [1945] 1973, 18-9).

After highlighting this general problem inherent in the German school of sociology of knowledge, concerning their lack of distinction between different types of knowledge, Merton continued by noting that Mannheim played fast and loose when it came to the status of scientific knowledge. In his 1941 review of Mannheim's program, Merton lashed out at the vague and often contradictory status of knowledge in the physical sciences, as treated in Mannheim's work. This criticism grew out of his original complaint: in some parts of Mannheim's writings, according to Merton, "we learn that the social process penetrates into the 'perspective' of 'most of the domains of knowledge'." Yet in other places, Merton continued, Mannheim argued that "the content of 'formal knowledge' (analytic statements? logic? mathematics? formal sociology?) is unaffected by the social or historical

situation. Such immunity is enjoyed by the 'exact sciences' but not by the 'cultural sciences'" (Merton [1941] 1957, 496-7). To Merton, Mannheim included the physical sciences within his rubric of "knowledge" when convenient, but then reversed his stance and systematically excused the physical sciences in other parts of his work. To Merton, the implications for Mannheim's sociology of knowledge were clear: "Had Mannheim systematically and explicitly clarified his position in this respect, he would have been less disposed to *assume* that the physical sciences are wholly immune from extra-theoretical influences and, correlatively, less inclined to urge that the social sciences are peculiarly subject to such influences" (Merton [1941] 1957, 497-8). Merton repeatedly offered this explicit critique, and Bloor seemed to follow Merton in this particular reading of Mannheim.

Merton did not want to see the physical sciences removed entirely from sociological analysis; yet the kind of sociological "determination" underlying knowledge in the physical sciences seemed to Merton to be in need of differentiation from the *Seinsverbundenheit* of other, distinct types of knowledge. By failing to make space for such divisions of knowledge, Mannheim had been forced in the end simply to leave some kinds of knowledge outside the scope of sociological explanation. And to Merton, this caused the crucial failure of Mannheim's work.

This criticism also received a long and explicit airing in Merton's 1945 review essay. Here Merton spent four full pages detailing the inconsistent status of knowledge in the physical sciences, as treated by the sociologists of knowledge. Not only had Marx and Engels reversed their earlier positions and removed the physical sciences from their sociological purview; Mannheim had committed the same crucial recantation: "Mannheim follows in the Marxist tradition to the extent of exempting the 'exact sciences' and 'formal knowledge' from existential determination but not 'historical, political and social science thinking as well as the thought of everyday life'" (Merton [1945] 1973, 21-2). Merton left no room for doubt: Mannheim's sociology of knowledge was crippled by his sloppy and inconsistent treatment of knowledge in the physical and natural sciences.

Readers of the 1945 essay, such as David Bloor, were thus handed an interpretation of Mannheim's sociology which emphasized its failure to bring the knowledge of the physical sciences within the scope of its analysis. Considering the authority which Bloor invested in Merton's analysis of Mannheim's work, we can begin to make sense of Bloor's repeated characterizations of Mannheim as "stopping short" before discussing the physical sciences. Yet this source alone is not enough to explain why Bloor then decided to "strengthen" Mannheim's work, rather than simply to abandon it as Merton had done. To understand this, we must take a further look at Merton's own program for the sociology of science, and Bloor's reactions to it.

Merton, Barnes, and Bloor on the Autonomy and Uniqueness of Science

It has become common when discussing Robert Merton's long career to note a distinction between his work from the 1930s and the work that followed in the 1940s through the 1960s. The earlier work, including his famous dissertation on science and society in seventeenth-century Britain, concentrated on the interactions between the institutions of science and the broader societies in which these institutions were embedded. The post-World War II work, however, shifted focus, and tended to concentrate on social processes specific to the internal workings of science (Mendelsohn 1989, 274; Storer 1973, xix–xx). Such distinctions gain credence from Merton's odd use of the third person in 1970 when discussing "the author" of his own 1938 monograph (Merton [1970] 1973).

Perhaps the most famous portion of Merton's postwar work concerns his "norms of science." First outlined in a 1942 paper, Merton isolated four norms which supposedly described scientists' behavior: "universalism," "communism," "disinterestedness," and "organized skepticism" (Merton [1942] 1973; cf. Mendelsohn 1989, 285–7; Hollinger 1983). In a paper from 1957 entitled "Priorities in Scientific Discovery," Merton continued his analysis of the scientific ethos in terms of norms. Here he discussed norms such as scientists' emphasis upon originality and the related reward systems internal to science, such as eponymy. He then went on to describe the tensions between these norms and other institutional norms of science, such as humility (Merton [1957] 1973).

It was this work on the ethos and norms particular to science that grew into the "Mertonian" program for the sociology of science. And while it did indeed proceed in directions different from his 1930s work, there was at least one thematic connection between the "two Mertons": his work on the norms of science, begun in 1938 and 1942, can be seen as filling in exactly what he had found lacking in Mannheim's sociology of knowledge, as he wrote about it in 1937, 1941, and 1945. Merton had read Mannheim as failing to isolate and characterize those sociological aspects peculiar to scientific knowledge. By focusing on the internal social workings of science, Merton could now address precisely the kinds of questions, unique to science, which had been omitted from Mannheim's approach.¹⁸

Yet beginning in the early 1970s, a rising generation of sociologists sought to supersede this Mertonian program of studying science in terms of a particular ethos and a timeless set of norms. Part of the motivation for challenging the

¹⁸ It may be objected that Merton's postwar work was not concerned directly with the "existential determination" of the content of scientific knowledge, as had been the goal of the sociologists of knowledge, but rather was restricted to questions of scientists' behavior and interactions. In fact, this was one of the criticisms made by Barry Barnes and David Bloor in the early 1970s. Still, Merton's postwar emphasis upon those aspects that were special and peculiar to the institution of science stands in marked contrast to his characterization of the "failure" of the sociology of knowledge. Considering that his first work on the norms of science was produced in 1938 and 1942, while his extended critiques of the sociology of knowledge were published in 1937, 1941, and 1945, it is reasonable to assume that such considerations informed Merton's choices for his postwar work.

Mertonian program at this time sprang from a more general move within sociology, anthropology, and history away from structural, functionalist explanations (Giddens 1987; Marcus and Fischer 1986, 7–44; Hunt 1989, 1–22; Chartier 1988, 1–16). One of the first salvos to incorporate this criticism against Merton's program was launched by Bloor's Edinburgh colleague, Barry Barnes. In a paper with R. G. A. Dolby, Barnes called attention to an important element of the ethos of science, as painted by Merton: the *autonomy* of science from any broader society (Barnes and Dolby 1970, 5).

Barnes and Dolby objected that Merton's concentration on science's norms had led him to exaggerate the distinctions between science and other social activities. Merton's norms, Barnes and Dolby argued, "are based on the view that a specifically 'scientific approach' exists common to the evaluative procedures of all scientists, and specific to them." Barnes and Dolby further charged that by postulating such a *unique*, isolated realm within which science operated, Merton had lost sight of the important ways in which science's norms were shaped and negotiated within a broader society: "It is our view that the general orientation which starts by identifying 'given' governing norms within science has resulted in neglect of processes by which normative structures have changed as the financial support, technological framework and esoteric content of scientific activity has itself changed" (Barnes and Dolby 1970, 8). To Barnes and Dolby, Merton's postulate of the autonomy of science had led him astray.

In their conclusions, Barnes and Dolby reiterated their two principal objections to Merton's postwar program for the sociology of science. The first was Merton's notion of a sharp difference between science and other social activities. On this point, Barnes and Dolby countered that scientists addressed and mediated dissent "by virtue of common language and culture among scientists, not by overriding specifically 'scientific' norms." And on Merton's failure to locate the practice of science within wider social contexts, Barnes and Dolby continued: "Similarly the intensity of normative conflict is cushioned by the order and rule structure of surrounding society; no scientist is totally in the power of others, ... and few scientists are *solely* responsible to their specialist colleagues" (Barnes and Dolby 1970, 24–5; emphasis in the original). Winds of change within the sociology of science had thus begun to blow from Edinburgh. Building on these criticisms of Merton's program, Barnes and Bloor began to build their "strong programme" for the sociology of scientific knowledge (Barnes 1974; Bloor [1976] 1991; cf. Ashmore 1989).

With Barnes, Bloor also sought to counter the Mertonian postulates of science's autonomy and uniqueness. And to do this, he once again returned to Merton's own portrait of Mannheim. Merton had criticized Mannheim for excluding scientific knowledge from his sociological investigations; then Merton had proceeded to study science as a self-enclosed social unit. To avoid the problems which this Mertonian approach seemed to engender, Bloor decided to go the other way: to resurrect Mannheim's description of the *Seinsverbundenheit* of knowledge, and

to extend this to scientific knowledge. Perhaps Merton had overstated the need to discuss so many distinct "types" of knowledge, each with its own means of sociological determination. By treating scientific knowledge in the same way that Mannheim had treated knowledge in the humanities and social sciences, science would not be cast as unique. And by discussing scientific knowledge in terms of these sociological underpinnings, Bloor could avoid Merton's exaggeration of science's autonomy from broader social settings and contexts.¹⁹ To build his strong programme for the sociology of scientific knowledge, Bloor effectively pitted Merton's portrait of Mannheim against Merton's own postwar program.

Bloor's Mannheim

From Robert Merton, Bloor learned that Mannheim had failed to treat knowledge in the natural sciences. With Barry Barnes, Bloor agreed that Merton's solution to this shortcoming was inadequate. Hence Bloor could return to Mannheim's "existential determination" or *Seinsverbundenheit*, and call for "causal" accounts of scientific knowledge. Likewise, Bloor could return to Mannheim's "total conception of ideology," and call for "symmetric" accounts, which treated the formation of true and false beliefs in the same way. And yet these "translations" of Mannheimianism into the sociology of scientific knowledge strike our eyes, following the lengthier discussion of Mannheim's work above, as incomplete at best (cf. Pels 1996). We may capitalize on this disjuncture to peer more closely at the vision undergirding Bloor's strong programme.

The first, and most obvious, point to note concerns the question of methods. Mannheim struggled for decades over the methodological and epistemological divide between the natural sciences and the cultural sciences. To Bloor, there appears little need for struggle:

Throughout the argument I have taken for granted and endorsed what I think is the standpoint of most contemporary science. In the main science is causal, theoretical, value-neutral, often reductionist, to an extent empiricist, and ultimately materialistic like common sense. ... The overall strategy has been to link the social sciences as closely as possible with the methods of other empirical sciences. In a very orthodox way I have said: only proceed as the other sciences proceed and all will be well. (Bloor [1976] 1991, 157)

¹⁹ Bloor cited Merton's "Priorities in Scientific Discovery" paper, along with Merton's two collections (*Social Theory and Social Structure*, and *The Sociology of Knowledge*) in (Bloor [1976] 1991). In chapter 4, Bloor framed most of this anti-Mertonian analysis as a critique of the philosophy of Karl Popper, instead of explicitly in terms of Merton's work. Still, the terms with which Bloor characterized Popper's "Enlightenment" approach to studying science shared much with Barnes' explicit critiques of Merton, quoted above. In a paper from 1971, Alex Dolby explicitly linked Merton's and Popper's shared assumptions in a general empiricist base (Dolby [1971] 1972, 312-3). We know from Bloor's 1973 article that he was well versed in Merton's essays on Mannheim's work. Still, I am not trying to allege, by this series of inferences, that Merton was the only, or even the principal, target of Bloor's critiques.

In a more recent textbook on the sociology of scientific knowledge, Bloor, Barnes and John Henry again spell out their methodological allegiances explicitly: "We see the sociology of scientific knowledge as part of the project of science itself, an attempt to understand science in the idiom of science. ... We ourselves honour science by imitation: in our study of science we try to emulate its own matter-of-fact, non-evaluative approach" (Barnes, Bloor, and Henry 1996, viii). Here there is no hint of Dilthey's distinction between *Verstehen* and *Erklärung*, nor of Simmel's, Rickert's, or Windelband's methodological demarcations, all of which had so exercised the young Mannheim.

Where Mannheim characterized such pronouncements by British and American sociologists in his own day as signs of "methodological asceticism" which could lead only to "sterilization" (Mannheim [1932] 1953, 189), Bloor instead flaunts the scientism of his program, writing in 1976, "No doubt I will be exposed to the further charge of 'scientism,' that is, an over-optimistic belief in the power and progress of science." As Bloor conceded then, "the charge of scientism is well aimed." Bloor found it ironic even at this early stage that "this scientific approach, when practiced by the sociology of knowledge and applied to science itself," would be misconstrued as "a denigration of science" (Bloor [1976] 1991, 160).²⁰

Whither this scientism, so proudly and unapologetically displayed by Bloor? It certainly does not originate solely from any close encounters with Mannheim's work. Instead, where Mannheim himself drew inspiration from Dostoevsky's novels and Kantian epistemology, Bloor's incorporation of Mannheim's *Seinsverbundenheit* and ideology-conception was forged against the backdrop of Bloor's previous training in experimental psychology. Bloor's training is noted in the "Notes on Contributors" accompanying his 1973 "Wittgenstein and Mannheim on the Sociology of Mathematics" (Bloor 1973, 208), though it is only much more recently that Bloor has made such links explicit. In accepting the J. D. Bernal Prize from the Society for the Social Studies of Science, Bloor reminisced that "my exposure to the Psychological Laboratory and the Applied Psychology Unit [in Cambridge, U.K.] was intellectually exciting, and I acquired a great respect for the discipline. It provided me with an orientation I have never lost" (Bloor 1997, 373). More specifically for the strong programme, "just as experimental psychologists turn their scientific curiosity onto the processes of individual cognition, so we can generalize the enterprise and turn scientific curiosity onto collective cognition." Various tenets of the strong programme, such as the symmetry principle, Bloor now says "can be seen as grounded in the practice of Cambridge experimental psychology" (Bloor 1997, 374, 379; see also 383, and Chapter 1 of Barnes, Bloor, and Henry 1996).²¹

²⁰ Charges that the strong programme represents an "anti-science" exercise, aiming to discredit or denigrate scientific activity, continue to be made (see, e.g., Fine 1996; cf. Barnes 1991, 322-3; Barnes 1994, 22; Proctor 1991, 228; Lynch 1993, 71; Shapin 1995; and Bloor 1996).

²¹ Like Bloor, most early practitioners of the sociology of scientific knowledge had not been trained originally in sociology, a point noted in the review essays by Collins (1983) and Shapin (1995).

To one trained in experimental psychology in the 1960s, adopting the methods of the natural sciences to produce empirical studies of the causes and formation of belief would appear both natural and automatic. And in an age which saw the rapid re-apportioning of power, prestige, and social status, placing federally-funded academic science onto center stage, any account of knowledge which did not treat scientific knowledge could easily be construed as "lacking the nerve and will" to undertake such difficult studies (cf. Mendelson, Smith, and Weingart 1988; Hollinger 1990 and 1995). By writing as if the methods of the natural, empirical sciences simply could be taken over into a sociological study, and by focusing exclusively on knowledge in the natural sciences, Bloor appears as much a creature of his time as Mannheim was of his own.²²

Where Mannheim and his mentors and colleagues distinguished between *Verstehen* and *Erklärung*, Bloor has called for studies which are simply "causal" and "empirical." Where Mannheim announced an "emancipation from a methodology oriented entirely on the natural sciences," Bloor has repeatedly trumpeted the scientific methodological bases of the strong programme. Where Mannheim sought objectivity via relationism, and feared that relativism of any sort posed an especially insidious threat in an age of dictators, Bloor has argued time and again that the sociology of scientific knowledge must embrace a full-fledged epistemological relativism (Bloor [1976] 1991, 158–160; Barnes and Bloor 1982). Where Mannheim characterized a narrow focus on the sciences and technology, to the exclusion of studies of government, history, and culture, as a dangerous "disproportionate development of human faculties," Bloor baited Mannheim for having been "too weak" and "lacking the nerve and will" to study the natural sciences. And yet it was Mannheim whom Bloor extolled repeatedly as a key precursor of Bloor's own strong programme. It is precisely this cacophony from which we may learn of the roots of Bloor's sociology of scientific knowledge.

Conclusions

In 1938, a beleaguered Karl Mannheim penned a brief autobiographical sketch for inclusion in a British anthology on sociology. Here he expressed his frustrations with trying to bring his sociology of knowledge to his adopted British audience. He wrote (in the third person), "Professor Mannheim's stay in this country has brought home to him at once the urgent need and the great difficulty of translating one culture in terms of another" (quoted in Kettler, Meja, and Stehr 1984, 118–9; cf. Sica 1988, 71–2). Following this extended route of readings and re-readings,

²² It is true that Mannheim often excused natural-scientific and mathematical knowledge from his sociological accounts, though he also dropped some intriguing hints to the contrary, perhaps to be left as "side-issues" after the more pressing concerns with the cultural and political failings of the day had been addressed (see Mannheim [1929] 1936, 166–7, and Werner Sombart's remarks at the 1928 Congress of German Sociologists, in Meja and Stehr 1990, 90).

and keeping Mannheim's dour view of "cultural translation" in mind, we may now understand how David Bloor's curious portrayal of Karl Mannheim's sociology of knowledge emerged and cohered from his particular vantage point. After learning from Robert Merton that Mannheim had neglected to treat scientific knowledge, it was easy for Bloor to decide that Mannheim's "nerve had failed him" before he could undertake a sociology of scientific knowledge. Yet Bloor also agreed with Barry Barnes that Merton's own postwar, functionalist program for the sociology of science suffered from its depiction of science as cut off from broader societal concerns, as unique among all knowledge-producing systems, and as faithful to a timeless and delineable set of "norms."

Bloor has also written recently of such troubles of translation, reflecting on twenty years of comment and criticism of his own strong programme. Like Mannheim, he notes the constant re-appropriation of one's work in the terms and orientation of its interpreters; appropriately enough, he turns to the pioneering experimental psychologist, Frederic C. Bartlett and his work on "conventionalization" (Bartlett 1932). On Bloor's telling,

Bartlett studied remembering by using pieces of real, living culture. He located folk stories and myths belonging to one ethnic group and presented them to members of a second ethnic group. The second group was his experimental subjects. He studied what happened to the memory of these stories by getting his subjects to recall them at a later period, and then again even later, and so on. He found a gradual change in how the story was remembered, but a change with interesting sociological properties. First, the story was progressively simplified: it lost detail characteristic of the original culture, but lost them in such a way that the mismatch with the second culture was diminished. Then, the recollected version of the story even began to have detail added to it, but of course, detail drawn from the second culture. So once again, it was shifted in the direction of the culture of the person who was remembering it. These selective and constructive processes proved to Bartlett both the creative and the culturally shaped character of the mechanisms responsible for memory. He called this process "conventionalization." (Bloor 1997, 375)

We can now see in Bartlett's findings, which Bloor has found so informative for his own sociological studies, a parable for the case of multiple-Mannheims pursued here.

In Edinburgh in the early 1970s, Mannheim's oeuvre could make sense for Bloor in a very particular way: although Mannheim himself had been "too weak" to study science, his program for the sociology of knowledge could be usefully "strengthened," and extended to study scientific knowledge. By returning to a system that Merton had criticized and abandoned, the strong programme could thereby resist Merton's portrayal of science as unique and autonomous. The combination of relying on Merton as an authority about Mannheim, of drawing

on the methodological tradition of Cambridge experimental psychology, and of desiring to treat scientific knowledge as akin to other types of knowledge, helped to steer Bloor to his specific choice of how to read Mannheim.

Yet Edinburgh in the 1970s was not Heidelberg in the 1920s. Engulfed as he was in turn-of-the-century debates within Hungarian and German academic circles over the proper methods and limits of the *Geistes-* and *Naturwissenschaften*, and responding in quite specific ways both to an encroaching "modernity" and a looming fascism, Mannheim's purposeful de-centering of the natural sciences carried in its day significant political and ethical overtones. The ideas and goals of Mannheim's generation did not float freely from the German *Methodenstreit* of the 1890s to British reactions in the 1970s to the Cold War re-fashioning of science and power, nor should we expect such transparent transmission. As we can now see, Bloor's strong programme is far from being simply a "strengthened" Mannheimianism.

It should be clear by now that Bloor's strong programme, and the specific reading of Mannheim that buttressed it, did not emerge out of thin air. Nor is the statement that Bloor's portrait of Mannheim fits only very poorly with other understandings of Mannheim's life's work intended to belittle, much less refute, Bloor's own program. Instead, by retracing the various steps linking Mannheim's work of the 1920s and 1930s, Merton's early reactions to it, his postwar redirection of the sociology of science, and Barnes' and Bloor's 1970s critiques of Mertonian sociology, we may begin to fill out a rounder picture of the roots of contemporary sociology of scientific knowledge. We can ask, for example, what were the chief concerns and interests of these early program-builders in sociology of scientific knowledge? What images of science, society, and sociological method undergirded their attempts to study scientific knowledge sociologically? How did these particular visions shape their (now twenty-five-year-old) approach, and how well do these tacit assumptions and images fit our own encounters with the history, philosophy, and social studies of science? By focusing on Bloor's mediated readings of Mannheim, we can learn something in a reflexive way about the direction of science studies for the present and the future.

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*Department of History of Science
Harvard University*