

# THE POSITIVIST DISPUTE IN GERMAN SOCIOLOGY

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- Theodor W. Adorno On the Logic of the Social Sciences. Original: 'Zur Logik der Sozialwissenschaften. Korreferat zu Popper, Oktober 1961', in *Kölner Zeitschrift für Soziologie und Sozialpsychologie*, vol. 14, 1962.
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INTRODUCTION  
TO THE ENGLISH TRANSLATION

The present volume contains some of the key contributions to a controversy which has raged in social scientific and philosophical circles in Germany since 1961. The immediate origin of the controversy lay in the conference held by the German Sociological Association in Tübingen in 1961 on the logic of the social sciences in which Popper presented his twenty-seven theses on that topic. This was replied to by Adorno and the discussion which followed at that conference is summarised by Dahrendorf. In a different form, the controversy was continued by Habermas and Albert from 1963 onwards and many other writers took up various issues and aspects of the controversy. Of these other contributions, only Pilot's was added to the original German edition. In 1969 the present volume appeared in Germany with considerable additional material by Adorno. This provoked the short afterword from Albert who was dismayed at the form which the volume had taken. This translation of the 1969 volume contains an additional essay not in the original, namely, Popper's review of the German volume.

The introduction to the translation sets out to locate this controversy within a wider context. Some remarks are made on the most ambiguous notions in this dispute, namely those of positivism and scientism. The fact that no one in the controversy claims to be a positivist has led Dahrendorf to speak of 'the third man' in the debate<sup>1</sup> and Giddens to suggest that 'the debate is like Hamlet without the prince'.<sup>2</sup> On the other hand, all the contributors to the debate have claimed allegiance either to critical rationalism or to critical theory. This adherence to the central role of theoretical criticism is one reason which led Dahrendorf to suggest, with reference to the original Popper-

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<sup>1</sup> R. Dahrendorf, *The Positivist Dispute in German Sociology*, p. 125.

<sup>2</sup> A. Giddens, ed., *Positivism and Sociology* (London, 1974), p. 18.



Adorno debate that a superficial glance at the controversy might incline one to the view that 'it could indeed have appeared, astonishingly enough, as if Popper and Adorno were in agreement.'<sup>3</sup> This is clearly far from being the case since, as the controversy proceeded, even the notion of a debate between competing standpoints became problematical.

There is some difficulty, too, in asserting that this controversy is merely a methodological dispute and can thus be seen as an extension of earlier controversies in the social sciences in Germany, notably the *Methodenstreit*, and the *Werturteilsstreit*. However, in order to examine this claim, some attempt will be made to sketch the earlier disputes in the social sciences in Germany in order to highlight the distinctive features of the present controversy. Nor should one assume that this dispute is confined to sociology as the title of the volume might suggest. Indeed, since the dispute contained in the present volume did not cease with its publication in 1969 but rather was continued, either through its extension to other areas, or to an expansion of the issues presented here, it would seem fruitful to sketch out the later stages of the controversy and some of the other commentaries upon the dispute. Finally, the issues presented in this dispute do not merely have relevance to the development of the social sciences in Germany. It can be argued that not only does the positivist dispute raise important issues for a dominant tradition in the social sciences outside Germany but that it has direct relevance for some of the recent controversies which have taken place in the philosophy of science and in the social sciences in the Anglo-American tradition in recent years.

#### I

Whilst positivism may be the 'ghost in the machine' in this dispute, it is certainly easy to discover a wide range of possible definitions of the constituent elements of positivism. It is difficult to find a generally acceptable nominalist definition of the term. Positivism is not a static entity but is itself dynamic and had taken different forms in various historical contexts. To take one example, Popper's assertion that he is not a positivist may be seen in the light of his criticism of the Vienna Circle, of which he was never a member. Popper had been very critical of the logical positivists

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<sup>3</sup> R. Dahrendorf, loc. cit., p. 123-4.

and it is certainly not possible to classify him, in any simple manner, in that school.<sup>4</sup> However, as Habermas and others have argued, logical positivism is only one variant—albeit a most important one in the development of the philosophy of science in this century—of positivism.<sup>5</sup> Although many writers have pointed to the long history of many features of positivism, especially if one takes into account the distinctive manner in which it has incorporated features of both empiricism and rationalism, modern positivism developed with certain types of reflection upon the growth of the natural and moral sciences.

In a section of *Knowledge and Human Interests* in which Habermas attempts to reconstruct the pre-history of modern positivism, the author examines the work of the most widely-known and perhaps least read of the positivists, Auguste Comte. As well as arguing that Comte sets out to justify 'the cognitive monopoly of science' through a philosophy of history whose ultimate goal is scientific technical progress, Habermas shows that Comte's varied usages of the term 'positive' can be translated into a set of methodological rules which may be summarised as follows:

1. 'all knowledge has to prove itself through the *sense certainty* of systematic observation that secures intersubjectivity.' (*le réel*)
2. '*Methodical certainty* is just as important as sense certainty . . . the reliability of scientific knowledge is guaranteed by unity of method.' (*la certitude*)
3. 'The exactitude of our knowledge is guaranteed only by the formally cogent construction of theories that allow the deduction of lawlike hypotheses.' (*le précis*)
4. 'Scientific cognition must be technically utilizable . . . Science makes possible technical control over processes of both nature and society . . . the power of control over nature and society can be multiplied only by following rationalist principles—not through the blind expansion of empirical research, but through the development and the unification of theories.' (*l'utile*)
5. 'our knowledge is in principle *unfinished and relative*, in accord-

<sup>4</sup> It is clear that Popper has a very precise notion of positivism which his opponents in this dispute do not share. For a recent account by Popper himself of his relations with the Vienna Circle, see 'Autobiography of Karl Popper' in P. A. Schilpp, ed., *The Philosophy of Karl Popper* (La Salle, Ill., 1974), esp. pp. 62ff.

<sup>5</sup> For the diversity of logical positivism itself see the useful collection in A. J. Ayer, ed., *Logical Positivism* (New York, 1959).

ance with the "relative nature of the positive spirit" (*le relative*)<sup>6</sup>

As well as illustrating the way in which this version of positivism assimilates both empiricism and rationalism, these methodological rules already point to the contradictory heritage of positivism. The sceptical or critical motive in positivism seeks to exclude whole areas of knowledge through a series of demarcations whilst its affirmative impulse seeks to secure knowledge through methodological rules. This contrast between critical enlightenment and the defence of a restrictive theory of science has been a permanent feature of positivism's history. It might be argued that in this uneasy combination of the sceptical and affirmative motives lies the instability of positivism.<sup>7</sup> Thus, at various points of its development, positivism has attempted to become more radical, to seek new ways of re-establishing its critical or restrictive claims.

The methodological rules which can be derived from Comte's writings also point to some modern features of positivism, which have retained Comte's positive impetus even though they may now take a different form. Von Wright suggests three basic tenets of positivism:

1. 'methodological monism, or the idea of the unity of scientific method amidst the diversity of subject matter of scientific investigation.'
2. 'the exact natural sciences, in particular mathematical physics, set a methodological ideal for all other sciences.'
3. Causal scientific explanation which consists in 'the substitution of individual cases under hypothetically assumed general laws of nature'.<sup>8</sup>

The first tenet is a more general version of the construction of Comte's second rule as well as being symptomatic of the whole process of laying down restrictive methodological rules. The second tenet is contingent upon the developments of mathematical physics in this century. Some positivist traditions might substitute formal logic for mathematical physics. The third tenet in

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<sup>6</sup> J. Habermas, *Knowledge and Human Interests*, trans. J. Shapiro (Boston, 1971/London, 1972), pp. 74-77. It should be clear from this that the subsequent history of positivism emphasized different aspects of these rules at various stages of its development.

<sup>7</sup> This is argued in H. Schnädelbach, *Erfahrung, Begründung und Reflexion. Versuch über den Positivismus* (Frankfurt, 1971).

<sup>8</sup> G. H. von Wright, *Explanation and Understanding* (London, 1971), p. 4.

the social sciences might today be exemplified by Hempel's covering law model.<sup>9</sup> This seems to suggest the unsatisfactory nature of any historical definition of positivism. However, von Wright also suggests that positivism is often characterized as being closely bound up with 'a "scientistic" and "technological" view of knowledge and its uses'. Yet this seems to shift the problems associated with positivism onto another equally problematical term, that of scientism.

As with positivism, no one in the present controversy claims to be committed to scientism. For example, Popper has argued that those of his opponents who accept the difference between science and the humanities as being one which rests upon the method of understanding are themselves committed to scientism. Thus, Popper states, when the supporters of such a standpoint 'denounce a view like mine as "positivistic" or "scientistic", then I may perhaps answer that they themselves seem to accept, *implicitly and uncritically*, that positivism or scientism is *the only philosophy appropriate to the natural sciences*'.<sup>10</sup> By scientism Popper means 'a name for the aping of what is widely *mistaken* for the method of science' rather than Hayek's original notion of 'the slavish imitation of the method and language of science'.<sup>11</sup> For Popper, then, scientism refers to the acceptance of a false methodological position.

Habermas' notion of scientism is related to what he takes to be the replacement of theories of knowledge by the philosophy of science positivistically interpreted. ' "Scientism" means science's belief in itself: that is the conviction that we can no longer understand science as *one* form of possible knowledge, but rather must identify knowledge with science'.<sup>12</sup> As such of course, it is closely bound up with the development of positivism. Indeed Habermas goes further and argues that 'Positivism stands and falls with the principle of scientism, that is that the meaning of knowledge is defined by what the sciences do and can thus be adequately explicated through the methodological analysis of scientific procedures. Any epistemology that transcends the framework of methodology as such now succumbs to the same

<sup>9</sup> See C. G. Hempel, *Aspects of Scientific Explanation* (New York/London, 1965), esp. pp. 333ff.

<sup>10</sup> K. R. Popper, *Objective Knowledge* (Oxford, 1972), p. 185.

<sup>11</sup> *Ibid.*

<sup>12</sup> J. Habermas, *loc. cit.*, p. 4.

sentence of extravagance and meaninglessness that it once passed on metaphysics.<sup>13</sup> In the present controversy, Habermas takes as a crucial feature of scientism that it equates scientific rationality with rationality in general. This is central to the exchanges between Habermas and Albert.

Other contributors to the controversy have taken scientism to imply other basic tenets. Lorenzen, for example, in his contribution to the later debate, 'Szientismus versus Dialektik', takes scientism to be a characterization of theories which hold that a rational legitimation of practical principles is impossible.<sup>14</sup> Such a polemical notion extends far beyond the confines of a pre-occupation with methodology and has been related to subsequent attempts to reconstitute practical philosophy.<sup>15</sup> Apel, in his contributions to the later controversy, also starts out from a broader notion of scientism as implying that reflection on the subject of the scientific process is minimal since this subject is itself understood as a scientific object.<sup>16</sup> In the extension of scientism to the practical sphere, Apel argues that scientism as the absolutism of value-free scientific rationality rests upon three premises. Firstly, that intersubjective validity is equivalent to the objectivity of the subjects of science; secondly, that science is the value-free description and explanation of facts (secured, in part, through formal logic); and thirdly, that no value judgments can be derived from facts. All these premises lie at the heart of the Habermas-Albert dispute where the nature of science as a specific form of activity and the particular type of practice derived from such a notion of science are placed in question.

It might be argued that much of the positivist dispute is the

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<sup>13</sup> Ibid, p. 67. Elsewhere Habermas writes, 'By scientism I mean a basic orientation prevailing in analytical philosophy, until recently the most differentiated and influential philosophy of our time. This orientation says that a scientific philosophy, just like science itself, must proceed *intentione recta*, i.e. it must have its object before itself (and is not allowed to approach it reflexively)', in 'A Postscript to *Knowledge and Human Interests*', trans. C. Lenhardt, *Philosophy of the Social Sciences*, vol. 3, 1973, p. 158.

<sup>14</sup> P. Lorenzen, 'Szientismus versus Dialektik' in R. Bubner, K. Cramer, R. Wiehl, eds., *Hermeneutik und Dialektik*, vol. 1 (Tübingen, 1970).

<sup>15</sup> See the collection, M. Riedel, ed., *Rehabilitierung der praktischen Philosophie* (Freiburg, vol. 1, 1972; vol. 2, 1974). Also F. Kambartel, ed., *Praktische Philosophie und konstruktive Wissenschaftstheorie* (Frankfurt, 1974).

<sup>16</sup> K.-O. Apel, *Transformation der Philosophie*, 2 vols. (Frankfurt, 1973), esp. 'Einleitung', 'Szientismus, Hermeneutik, Ideologiekritik' and 'Szientismus oder transzendente Hermeneutik?'. See further his 'The A Priori of Communication and the Foundation of the Humanities', *Man and World*, vol. 5, 1972.

result of terminological confusion since it is certainly the case that the participants claim their views are misunderstood, not examined or not held up to genuine criticism. Yet this apparent process of talking past one another might in turn have its origin not in conceptual confusion as such but in the situation which Feyerabend refers to as the incommensurability of theories.<sup>17</sup> The main groupings in this dispute lay claim to the development of a critical stance, either as critical rationalism or as a critical theory of society. However, once again the dispute centres around the nature of the criticism and the foundations of such criticism and rationalism. It does begin to look as if one difficulty in this debate is that there is more than one ghost in the machine.<sup>18</sup>

## II

This is certainly not the first time that many of the issues raised in this dispute have been the subject of heated controversy. It is a feature of the development of the social sciences in Germany that they have historically produced a number of important controversies, ostensibly concerned with methodology but actually more wide-ranging. At times they have provided a degree of reflection upon the activity of doing social science which has no comparable development in other social scientific traditions. The earlier controversies will be briefly presented in order to locate the present dispute within some historical perspective. One cannot, of course, appeal to these past controversies in order to offer easy solutions to the present dispute, but it is necessary to show how the positivist dispute both continues earlier themes or develops new points of departure.

Long before the original Schmolter-Menger *Methodenstreit* in economics in the 1870s and 1880s in Germany, one can discern aspects of contrary positions which are relevant to the present dispute. Within the empiricist tradition of philosophy, Hume's discussion of values is relevant for the later *Werturteilsstreit*. Habermas has pointed to the contrary motives of the Scottish

<sup>17</sup> The incommensurability argument is made most forcefully in P. Feyerabend, 'Against Method', in M. Radnor and S. Winokur, eds., *Minnesota Studies in the Philosophy of Science*, vol. 4 (Minneapolis, 1970). For an expanded version see P. Feyerabend, *Against Method* (London, 1975). This argument is rejected by Popper especially in his remarks on 'the myth of the framework'. See K. Popper, 'Normal Science and its Dangers' in I. Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge, 1970), pp. 56ff.

<sup>18</sup> See K. Popper, 'Wider die grossen Worte', *Die Zeit*, 24.9.1971.

Economists in the second half of the eighteenth century who, though they directed their research against existing institutions and authorities, did not basically question the postulate of continuous progress in society.<sup>19</sup> Their early version of sociology can be seen as the precursor of what Brinkmann was later to refer to as sociology as an 'oppositional science'. That critical tradition of sociology was continued in the immediate post French Revolution period by Saint Simon, the conservative tradition by de Bonald.<sup>20</sup> Negt has high-lighted the opposition of a Hegelian and Comtean social theory, an opposition which still has relevance for the contenders in the present positivist dispute.<sup>21</sup> The whole of Marx's critique of political economy stands as a critical chapter in the development of methodological reflection upon critical social science, though its relevance for the methodological controversies later in the century is hardly ever drawn.<sup>22</sup> This is all the more surprising since Marx and Engels were engaged in the development of a methodology which they saw as both logical and historical. As Engels remarks cryptically 'the logical method of approach was therefore the only suitable one. This however is indeed nothing but the historical method, only stripped of the historical form and divested of chance occurrences.'<sup>23</sup> In a very different form, the controversy sur-

<sup>19</sup> J. Habermas, 'Kritische und konservative Aufgaben der Soziologie' in *Theorie und Praxis* (Neuwied/Berlin, 1963).

<sup>20</sup> H. Marcuse, *Reason and Revolution* (New York, 1941).

<sup>21</sup> O. Negt, *Strukturbeziehungen zwischen den Gesellschaftslehren Comtes und Hegel* (Frankfurt, 1964).

<sup>22</sup> An exception is O. Morf, *Geschichte und Dialektik in der politischen Ökonomie* (Frankfurt, 1970).

<sup>23</sup> F. Engels, 'Review' (of Marx's *A Contribution to the Critique of Political Economy*) appended to K. Marx, *A Contribution to the Critique of Political Economy* (London/New York, 1971), p. 225. For Marx the logical method did not imply applying Hegel's logic abstractly to the subject matter of political economy. As Marx himself remarked of a writer's attempt 'to present political economy in the Hegelian manner . . . He will learn to his cost that to bring a science by criticism to the point where it can be dialectically presented is an altogether different thing from applying an abstract ready-made system of logic to mere inklings of such a system', K. Marx and F. Engels, *Selected Correspondence* (Moscow, 2nd ed., 1965), p. 102. Marx also avoided the polarization of history and nature which was so characteristic of later controversies in Germany. On this see A. Schmidt, *The Concept of Nature in Marx*, trans. B. Fowkes (London, 1971). In *The German Ideology* Marx writes, 'We know only a single science, the science of history. History can be contemplated from two sides, it can be divided into the history of nature and the history of mankind. However, the two sides are not to be divided off; as long as men exist, the history of nature and the history of men are mutually conditioned.' Quoted in A. Schmidt, loc. cit., p. 49.

rounding a historical or logical approach lay at the centre of the Schmoller–Menger *Methodenstreit*.

In the early pages of his most important contribution to the *Methodenstreit* published in 1883 Menger, with reference to economics, writes ‘the progress of our science at present is hindered by the sway of erroneous methodological principles’.<sup>24</sup> In the course of that decade Menger and Schmoller debated whether economics should proceed according to the ‘exact’ or ‘historical’, the ‘deductive’ or the ‘inductive’, the ‘abstract’ or the ‘empirical’ method.<sup>25</sup> Menger argued that the world of phenomena supply two types of knowledge for science—concrete phenomena which are individual and empirical forms which are general. Those empirical forms which are repeated, Menger terms types, whilst relations which regularly recur are typical relations. Knowledge of these latter are as important as concrete phenomena. Thus for Menger, without cognition of typical relations we would be deprived of a deeper understanding of the world and of the prediction and control of phenomena. Menger goes on to suggest that there are three groups of economic science: historical economics, concerned with individual concrete phenomena, individual relations, individual knowledge; theoretical economics concerned with types, typical relations and general knowledge; and practical economics concerned with techniques, with economic policy and finance. Menger argues that the historical and theoretical are exclusive approaches. However, Menger does refer to the role of understanding in his commitment to the theoretical orientation. He argues that understanding is gained in two ways; as historical understanding where we investigate a phenomenon’s individual process of development, and as theoretical understanding where we recognize a phenomena to be a special case of certain regularity in the succession or co-existence of phenomena. These two types of understanding should be strictly separated. The theoretical

<sup>24</sup> C. Menger, *Problems of Economics and Sociology*, trans. F. Nock, ed. and introd. L. Schneider (Urbana, 1963), p. 31. This is a translation of C. Menger, *Untersuchungen über die Methode der Sozialwissenschaften und der politischen Ökonomie insbesondere* (Leipzig, 1883).

<sup>25</sup> The battle lines of that debate were in fact more complex. See G. Ritzel, *Schmoller versus Menger. Eine Analyse des Methodenstreits im Hinblick auf den Historismus in der Nationalökonomie* (Frankfurt, 1950); R. Hansen, ‘Der Methodenstreit in den Sozialwissenschaften zwischen Gustav Schmoller und Karl Menger’, in A. Diemer, ed., *Beiträge zur Entwicklung der Wissenschaftstheorie in 19. Jahrhundert* (Meisenheim, 1968); D. Lindenlaub, ‘Richtungskämpfe im Verein für Sozialpolitik’, *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, (Beiheft 52, 1967), pp. 96f.



approach in economics proceeds through a realistic empirical method to give us real types and empirical laws, and through what Menger terms the exact scientific method to enable us to move from the simplest to the most complex elements.

Schmoller, the leading figure in the historical school, questioned the role of general theories in the social sciences.<sup>26</sup> Menger's version of economics, he argued, could only lead to the empirically empty formation of models, including Robinsonades, based on abstract principles. Menger had argued that historical economics could fulfil only a subordinate role in economics. Schmoller saw historical science as itself generating rules which should explain reality and which must be tested. Further, Schmoller sought to distance metaphysics, abstract thought and ideals from economic theory and concentrate upon the actual life process of society. Habermas argues that the historical school countered Menger's approach with two related theses: firstly that 'economics is not concerned with the functions of quantities of goods but rather with the interdependence of economic actions' and secondly that 'since intentional action can only be interpreted through understanding, a strict mathematically formulated scientific economic theory is not possible'.<sup>27</sup> Yet Schmoller did attempt to thrust aside the separation of history and economic theory and to make history a necessary part of theory. However, he hoped to do this whilst rejecting both Menger's characterization of historical method and that implicit in Rickert's and Windelband's distinction between the natural and cultural sciences. The degree of concretion demanded by the historical school was often lacking. Weber, for example, later rightly criticized Roscher's reduction of the complex interaction of nature, society and the individual to the abstract reified notion of 'the people'.<sup>28</sup>

It is difficult to characterize the issues at stake in the original *Methodenstreit*, since they cannot be taken to rest upon a simple

<sup>26</sup> See G. Schmoller, 'Zur Methodologie der Staats- und Sozialwissenschaften', *Schmollers Jahrbuch*, 1883. The debate was continued with the publication of K. Menger, *Die Irrtümer des Historismus in der deutschen Nationalökonomie* (Vienna, 1884).

<sup>27</sup> J. Habermas, 'Zur Logik der Sozialwissenschaften', *Philosophische Rundschau*, Beiheft 5 (Tübingen, 1967). Reprinted with additional material (Frankfurt 1971), p. 128.

<sup>28</sup> M. Weber, 'Roscher und Knies und die logischen Probleme der historischen Nationalökonomie', *Gesammelte Aufsätze zur Wissenschaftslehre*, 3rd ed. (Tübingen, 1968). For a brief account of Weber's relation to the *Methodenstreit* see W. Cahnman, 'Weber and the Methodological Controversy', in W. Cahnman and A. Boskoff, eds., *Sociology and History* (New York, 1964).

debate between inductionism and deductionism. Schmoller, for example, was aware that induction and deduction must go together but did not realize the real meaning of their combination. In this controversy it was perhaps not the relation of theory to reality which was at issue but rather what constitutes theory, in a context in which both Schmoller and Menger rejected classical political economy.<sup>29</sup>

The later stages of the *Methodenstreit* roughly coincide historically with the development of the neo-Kantian attempt to ground the separation of the natural from the historical or cultural sciences, a separation which many see to be a central error in the positivist dispute. As Popper argues, 'Labouring the difference between science and the humanities has long been a fashion, and has become a bore. The method of problem solving, the method of conjecture and refutation, is practised by both. It is practised in reconstructing a damaged text as well as in constructing a theory of radioactivity.'<sup>30</sup> Against this view the predominance of the unified science ideal and of methodological monism has been seen by Habermas as the reason why 'the lively discussion of the methodological distinction between natural and cultural scientific research which was first opened by neo-Kantianism is today forgotten; the problematic which it sparked off does not appear real any more'.<sup>31</sup> Habermas goes on to argue that whilst the dominant positivist interpretation of research has adopted the unified science thesis and accounted for any dualism in science in terms of distinctions between levels of development, it still remains true that research continues to take separate paths which take little notice of one another, either as a general methodology of empirical science or as a general hermeneutics of social and

<sup>29</sup> In many ways the lines between the historical and theoretical traditions in political economy were more clearly drawn in the later Böhm-Bawerk-Hilferding controversy concerning Marx's methodology. See E. von Böhm-Bawerk, *Zum Abschluss des Marx'schen Systems* (Berlin, 1896); R. Hilferding, 'Böhm-Bawerk's Marx-Kritik' in M. Adler and R. Hilferding, eds., *Marx Studien*, vol. 1 (Vienna, 1904). These are translated in P. Sweezy, ed., *Karl Marx and the Close of his System and Böhm-Bawerk's Criticism of Marx* (New York, 1949). See also Böhm-Bawerk's own contribution to the *Methodenstreit* in Böhm-Bawerk, 'The Historical versus the Deductive Method in Political Economy', *Annals of the American Academy of Political and Social Science*, vol. 1, 1890. For a commentary on the Böhm-Bawerk-Hilferding controversy see B. Rüter, *Die Auseinandersetzung zwischen Böhm-Bawerk and Hilferding über Marx. Darstellung und Kritik* (Cologne, 1926); E. Kauder, 'Austro-Marxism versus Austro-Marginalism', *Journal of the History of Political Economy*, 1971.

<sup>30</sup> K. Popper, *Objective Knowledge*, loc. cit., p. 185.

<sup>31</sup> J. Habermas, 'Zur Logik der Sozialwissenschaften', 2nd ed., p. 71.

historical science. For this reason, and since the neo-Kantian distinctions play an important role in the *Werturteilstreit* and Weber's attempted resolution, it is useful to return to these writers.

Windelband and Rickert were the leading figures in the South-West School of neo-Kantianism.<sup>32</sup> Windelband's distinction between history—already a well developed study in Germany—and natural science in his rectoral address of 1894 was not, as many later interpreters suggest, based on a metaphysical dualism of nature and spirit (*Geist*), which Windelband explicitly rejected. Rather, this distinction was based on the formal character of their cognitive goals. Unlike Dilthey's earlier division, Windelband's is not based on psychological or hermeneutic grounds but is instead logically based. Windelband's concern is with the methods of research and not with the object of research, which could, he argued, be investigated by either method. It is in this way that we should understand Windelband's distinction between a science generating laws and a science of individual events, between nomothetic and idiographic sciences. It is thus not an ontological demarcation of scientific realms but a typology of scientific modes of procedure. As Schnädelbach comments 'The application of nomothetic or idiographic procedures is thereby directed not according to the object but according to the cognitive interest or the cognitive goal.'<sup>33</sup> In terms of utility, Windelband ascribes to the natural sciences a technical goal or interest and to the historical-idiographic sciences a practical goal or cognitive interest. He argues strongly against the view that our knowledge can only be nomothetic and for the view that the dualism of 'these two moments of human knowledge' are not reducible to one another and cannot be transcended.

<sup>32</sup> There were two schools of neo-Kantian philosophy, only one of which concerns us here. The Marburg School, whose central figures were Cohen and Natorp, was interested primarily in natural scientific knowledge and took scientific cognition to be the prototype of all cognition worthy of the name. Epistemology for them was therefore the analysis of the logical foundations of the exact sciences. In some respects modern positivism has its roots in this tradition. The South-West School, whose leading figures were Windelband and Rickert, focused their attention on the historical and cultural sciences. The original statement of their position was W. Windelband, 'Geschichte und Naturwissenschaft', *Präludien*, vol. 2, new ed. (Tübingen, 1924). Amongst Rickert's most relevant works are H. Rickert, *Kulturwissenschaft und Naturwissenschaft* (Tübingen, 1899), and H. Rickert, *Die Grenzen der naturwissenschaftlichen Begriffsbildung* (Tübingen/Leipzig, 1902).

<sup>33</sup> H. Schnädelbach, *Geschichtsphilosophie nach Hegel* (Freiburg/Munich, 1974), pp. 141–2. Schnädelbach provides a very concise account of the neo-Kantian tradition in this volume.

Rickert followed Windelband in his commitment to an epistemological and logical starting point for his analysis and to the thesis that 'the value relevance (*Wertbeziehung*) of the objects of knowledge possess central importance for the special position of disciplines to which history belongs'.<sup>34</sup> However, Rickert extended his analysis and distanced himself from Windelband in important respects. As well as relativizing the nomothetic-idiographic distinction to a relative typological opposition between generalizing and individualizing methods, Rickert changed the distinction itself to one between natural and cultural sciences in order to remove any association of the *Geisteswissenschaften* with psychology.<sup>35</sup> Whereas Windelband based his opposition of nomothetic and idiographic upon the logical dualism of general and particular judgments, Rickert shifted the basis for the distinction to the level of scientific concept formation. This was necessary for Rickert since, as Schnädelbach explains 'if, with Kant, one starts out not from facts as finished objects, but from facts of consciousness in the sense of a variety of sensory perceptions then a *constitution of facts as scientific objects* is required *before* one can apply judgement to the facts'.<sup>36</sup> This insistence on the epistemological priority of concept formation over the activity of judgment—and in this he was following Kant—led Rickert to transfer Windelband's problem of classification to the level of scientific concept formation.

The specific realm of the cultural sciences are for Rickert constituted from the prior value relevance of empirical material whereas the dominant perspective in the choice and synthesis of data is generation of laws. Thus value and law generation are the two organizational principles in cultural and natural scientific concept formation. However, not only is our constitution of cultural objects a process of individualizing concept formation

<sup>34</sup> Ibid, p. 144.

<sup>35</sup> It is worth pointing out here that the term '*Geisteswissenschaften*' was originally introduced into German through the 1863 translation of J. S. Mill's *A System of Logic*, when Schiel, the translator, interpreted the title of Book VI of that work, 'On the Logic of the Moral Sciences' as 'Von der Logic der Geisteswissenschaften oder moralischen Wissenschaften'. Dilthey had brought the term into more familiar usage and, in the period in which Rickert was writing, had tended to make the association between psychology and *Geisteswissenschaften*, though this was hardly his intention. On Dilthey's examination of the cultural sciences see J. Habermas, *Knowledge and Human Interests*, loc. cit., chs. 7 and 8.

<sup>36</sup> H. Schnädelbach, loc. cit., p. 146.

but the importance of values takes on new meaning. For Rickert 'the unity and objectivity of the cultural sciences is determined by the unity and objectivity of our concept of the cultural and this in turn, by the unity of and objectivity of values which we value'.<sup>37</sup> At this point, however, Rickert must move on to a cultural philosophy and a philosophy of value. Yet such a philosophy would be relegated to the level of metaphysics within the neo-Kantian tradition. Their strict interpretation of epistemology as a logic of science would necessarily lead them to a strict separation of the critique of knowledge and hermeneutics, such that the latter would be removed from consideration. Rickert construed the concept of culture on the basis of transcendental idealism: 'culture as the essence of appearances under a system of valid values has a transcendental meaning—it says nothing about the objects, but rather determines the conditions for the possible interpretation of objects'.<sup>38</sup> For Rickert science can only ask of values whether they are valid, not whether they exist. This can only lead to a restriction of the notion of understanding. As Habermas argues, Rickert remains trapped in the dichotomies of facts and values, empirical existence and transcendental validity and nature and culture.<sup>39</sup>

If the South West German neo-Kantian tradition did pose basic problems for the cultural sciences in terms of their relation to values then they did so at a largely theoretical and formal level. This is in contrast with the heated controversy known as the *Werturteilsstreit*, a controversy which has not only continued to exist in the social sciences but one which, in the context of the positivist dispute, Dahrendorf argues 'Even if the fronts have perhaps been reversed, the controversy over value judgments has forfeited little of its explosiveness in German Sociology after fifty years'.<sup>40</sup>

The original *Werturteilsstreit* commenced in earnest in 1909 at the Vienna general meeting of the *Verein für Sozialpolitik* and

<sup>37</sup> H. Rickert, *Kulturwissenschaften und Naturwissenschaften*, loc. cit., p. 137.

<sup>38</sup> J. Habermas, *Zur Logik der Sozialwissenschaften*, loc. cit., p. 76.

<sup>39</sup> In terms of one of the debates which has succeeded the positivist dispute, namely the Habermas-Luhmann controversy, Bubner has suggested that it is possible to trace Luhmann's position, especially on the notion of meaning, back to Rickert. See R. Bubner, 'Wissenschaftstheorie und Systembegriff. Zur Position von N. Luhmann und deren Herkunft', in R. Bubner, *Dialektik und Wissenschaft* (Frankfurt, 1973).

<sup>40</sup> R. Dahrendorf, 'Remarks on the Discussion', *The Positivist Dispute*, p. 127.

continued in the years up to 1914.<sup>41</sup> The *Verein* had been founded in 1872 as a social reform movement which opposed both the isolation of economic life from the rest of society, which was seen to be exemplified in the work of the Manchester School of economics, and revolutionary socialism. However, though a reform movement it never took up a concrete social political programme as such but published studies of specific concrete problems in the socio-economic sphere.

The original 1909 discussion placed in question the conditions for the possibility of a normative social and economic science, with Sombart arguing that what was decisive was whether economics could be considered a science, whilst his opponent Knapp argued that the *Verein*, by its very nature, must be engaged in political activity. Max Weber, though he argued for the principle of a value free (*Wertfreiheit*) science whilst recognizing the value relevance (*Wertbeziehung*) of all scientific research, maintained that the *Verein* must remain a forum for the discussion of political evaluations and goals. If the *Verein* was to remain concerned with the political sphere then some other organization should perhaps concern itself with value free scientific research. In fact, one important consequence of the 1909 meeting was the foundation of a separate sociological association which had its first meeting in 1910. This move heralded the professionalization of sociology in Germany and its increasing separation from politics and from the study of economics which was itself caught up in attempts to separate positive economics (*Volkswirtschaftslehre*) from normative economics (*Volkswirtschaftspolitik*). Symptomatic of the latter split and of the attempt to develop a scientific study of values and norms is Weber's definition of sociology as 'the scientific investigation of the general cultural meaning of the socio-economic structure of human communal life'. This split did not mean that the discussion of the role of value judgments in social science ceased in either of the two institutions. The discussion papers circulated by Max Weber, Schmoller and others in 1912 as a preliminary basis for a meeting of the *Verein* in 1913 showed that the debate was hardly over. Schmoller asserted the possibility of 'objective value judgments' and the hope that

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<sup>41</sup> For a detailed account of the *Verein*, see D. Lindenlaub, loc. cit. See also W. Hofmann, *Gesellschaftslehre als Ordnungsmacht. Die Werturteilsfrage-heute* (Berlin, 1961); C. von Ferber, 'Der Werturteilsstreit. 1909/59', *Kölner Zeitschrift für Soziologie*, vol. 11, 1959; H. Albert and E. Topitsch, eds., *Werturteilsstreit* (Darmstadt, 1971).

ethics might increasingly become an empirical science. Weber, however, asserted the permanent struggle of a plurality of values, even 'the ethical irrationality of the world', and the need for science in its study of values to examine their existence but not their validity. Behind this demand for value free science lies an epistemological conception derived from the neo-Kantians, namely, that value judgments are not the result of cognitive acts. In fact Weber later saw the justification of practical judgments as meaningless: 'It is [therefore] in principle meaningless, since the diverse value orders of the world stand in an insoluble struggle with one another'.<sup>42</sup>

At the first meeting of the newly founded German Sociological Association, Weber was not alone in asserting the non-partisan nature of sociology. Töinnes, too, argued that the new association was a learned society and not a school and that consequently 'We wish therefore as sociologists to concern ourselves only with what is, not with what, from whatever viewpoint, on whatever grounds, should be'.<sup>43</sup> Such views were not accepted by some members of the association and the issues continued to be debated up to the outbreak of the First World War.

Whilst it is hardly possible to develop the methodological standpoint of Max Weber in this context, it is important to point out at this juncture that Weber's work in this period was not solely preoccupied with methodological issues in the abstract.<sup>44</sup> As well as being concerned with the abstract theories of economics and specifically the development of the notion of the ideal type from Menger, Weber was deeply preoccupied with developments in historical research too.<sup>45</sup> Nor was he concerned merely with philosophical issues surrounding methodology, but rather with

<sup>42</sup> M. Weber, *Wissenschaftslehre*, loc. cit., p. 603.

<sup>43</sup> F. Töinnes, 'Wege und Ziele der Soziologie', *Verhandlungen des Ersten Deutschen Soziologentages, 1910* (Tübingen, 1911), p. 23.

<sup>44</sup> On Weber's methodology see F. Tenbruck, 'Die Genesis der Methodologie Max Webers', *Kölner Zeitschrift für Soziologie*, vol. 11, 1959; H. Baier, *Von der Erkenntnistheorie zur Wirklichkeitswissenschaft. Eine Studie über die Begründung der Soziologie bei Max Weber*, unpublished habilitation thesis (Münster, 1969). For a classic earlier analysis see A. von Schelting, *Max Webers Wissenschaftslehre* (Tübingen, 1934).

<sup>45</sup> See the collection by W. Mommsen, *Gesellschaft, Politik und Geschichte* (Frankfurt, 1974), esp. the essays, 'Soziologische Geschichte und historische Soziologie' and "'Verstehen" und "Idealtypus". Zur Methodologie einer historischen Soziologie'. In the latter essay, Mommsen points to Weber's interest in the methodological dispute in history surrounding Karl Lamprecht's attempt to ground an exact cultural history. Cf. Mommsen, loc. cit., pp. 21 ff.

the development of an empirical science, a science of reality (*Wirklichkeitswissenschaft*). Thus, as Rickert argued, 'Logical investigation for Weber certainly never remained an end in itself' but was always directed towards 'actual questions of social life'.<sup>46</sup> Yet much of Weber's methodological writing has suffered from later interpretations which have extracted problems which he raised, e.g. the role of value judgments and understanding, and so distanced them from empirical study by placing them firmly in the sphere of an autonomous meta-science of methodology that their real relevance for the practice of scientific research is often lost. For example, it is a distortion of Weber's viewpoint to relegate his category of understanding to a heuristic device as is often the case in neo-positivist interpretations of his work. Even though Weber did not use understanding as a way of distinguishing the natural from the human sciences, and although he was critical of the notion of *Verstehen*, he did not give it a subordinate place to nomological explanation; rather understanding and explanation were seen as complementary, whilst at the same time understanding served as a connecting link between causal knowledge of social phenomena and a value relevant interpretation of social phenomena.<sup>47</sup>

In the field of sociology in the post First World War period the value problem received a more radical statement in the development of the sociology of knowledge in Germany, particularly as exemplified by Mannheim's *Ideology and Utopia*.<sup>48</sup> This enterprise is sharply criticized in the present positivist dispute by both Popper and Adorno. Of more relevance for the present dispute were the rise of critical theory and the development of logical positivism. Horkheimer, in a number of essays written in the nineteen thirties, particularly 'Traditional and Critical Theory' and 'The Latest attack on Metaphysics', sought both to distinguish critical theory from contemporary notions of theory and to attack

<sup>46</sup> Quoted in H. Baier, loc. cit., p. 62.

<sup>47</sup> See W. Mommsen, loc. cit., pp. 208ff.

<sup>48</sup> K. Mannheim, *Ideologie und Utopie* (Bonn, 1929). It is possible to see Mannheim's position in this work as a radical version of Weber's notion of value pluralism. Weber's later discussion of the value problem in relation to science has been seen as part of a further controversy generated in the early 1920s, which Kracauer terms 'the so-called *Wissenschaftsstreit*'. See S. Kracauer, 'Die Wissenschaftskrisis' in S. Kracauer, *Das Ornament der Masse* (Frankfurt, 1963). See also K. Singer, 'Die Krisis der Soziologie', *Weltwirtschaftliches Archiv*, vol. 16, 1920-21; E. Wittenberg, 'Die Wissenschaftskrisis in Deutschland im Jahre 1919', *Theoria*, vol. 3, 1937.



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