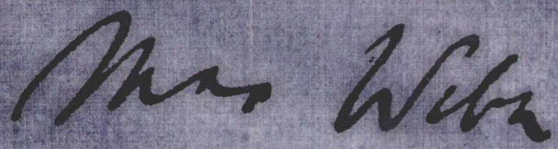

Max Weber

Collected methodological writings

Edited by Hans Henrik Bruun and Sam Whimster

Translated by Hans Henrik Bruun

WEBER IN TRANSLATION

ROUTLEDGE

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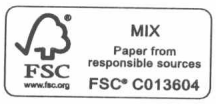

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MAX WEBER BIBLIOGRAPHY, WITH ABBREVIATIONS

The Weber Bibliography covers all books, articles and sections of books by Max Weber translated or mentioned in this book. For ease of reference, it has been separated from the general Bibliography (pp. 502ff.) and put at the very beginning of the volume.

Each entry comprises:

- the full relevant bibliographical references (for articles: the text from which the articles have been translated);
- the English translation of the title (except for translations of Weber's work);
- a short abbreviated reference (preceded by "Q:"), in italics, for quotation in the main text.

A few other abbreviations frequently employed in this volume can also be found here.

German titles

Books

Max Weber Gesamtausgabe, Tübingen: J.C.B. Mohr (Paul Siebeck), 1984–
("Max Weber. Complete edition")
Q: *MWG*

Gesammelte Aufsätze zur Wissenschaftslehre (ed. Johannes Winckelmann), 3rd ed., Tübingen: J.C.B.Mohr (Paul Siebeck), 1968
("Collected methodological essays")
Q: *GAW*

Gesammelte Aufsätze zur Soziologie und Sozialpolitik (ed. Marianne Weber), Tübingen: J.C.B.Mohr (Paul Siebeck), 1924
("Collected essays on sociology and social policy")

Gesammelte Aufsätze zur Religionssoziologie (ed. Marianne Weber), Tübingen: J.C.B.Mohr (Paul Siebeck), 1920–1921
("Collected essays on the sociology of religion")

"Die protestantische Ethik und der Geist des Kapitalismus", *GARS*, pp. 17–206)
("The protestant ethic and the spirit of capitalism")
Q: *Protestant Ethic*

Wirtschaft und Gesellschaft. Grundriß der verstehenden Soziologie (ed. Johannes Winckelmann), 5th ed., Tübingen: J.C.B.Mohr (Paul Siebeck), 1976
("Economy and Society. Outline of interpretive sociology")
Q: *WG*

"Soziologische Grundbegriffe", *WG*, pp. 1–30
("Basic sociological concepts")
Q: *Basic Concepts*

Articles

"Roscher und Knies und die logischen Probleme der historischen Nationalökonomie", *GAW*, pp. 1–145
("Roscher and Knies and the problems of historical economics")
Q: *Roscher and Knies*

"Roschers 'historische Methode'", *GAW*, pp. 3–42
("Roscher's 'historical method'")
Q: *Roscher*

"Knies und das Irrationalitätsproblem", *GAW*, pp. 42–105
("Knies and the problem of irrationality")
Q: *Knies I*

"Knies und das Irrationalitätsproblem (Fortsetzung)", *GAW*, pp. 105–46
("Knies and the problem of irrationality (continued)")
Q: *Knies II*

"Geleitwort", *Archiv für Sozialwissenschaft und Sozialpolitik* 19 (1904), pp. I*–VII*
("Accompanying remarks")
Q: *Accompanying Remarks*

"Die 'Objektivität' sozialwissenschaftlicher und sozialpolitischer Erkenntnis", *GAW*, pp. 146–214
("The 'objectivity' of knowledge in social science and social policy")
Q: *Objectivity*

"Kritische Studien auf dem Gebiet der kulturwissenschaftlichen Logik", *GAW*, pp. 215–90
("Critical studies in the logic of the cultural sciences")
Q: *Critical Studies*

"R. Stammlers 'Überwindung' der materialistischen Geschichtsauffassung", *GAW*, pp. 291–359
("R. Stammler's 'overcoming' of the materialist conception of history")
Q: *Stammler*

"Nachtrag zu dem Aufsatz über R. Stammlers 'Überwindung' der materialistischen Geschichtsauffassung", *GAW*, pp. 360–83
("Addendum to the essay on R. Stammler's 'overcoming' of the materialist conception of history")
Q: *Addendum*

"Die Grenznutzlehre und das 'psychophysische Grundgesetz'", *GAW*, pp. 384–99
("The theory of marginal utility and the 'fundamental law of psychophysics'")
Q: *Marginal Utility*

"'Energetische' Kulturtheorien", *GAW*, pp. 400–26
("'Energetical' theories of culture")
Q: *Energetics*

Review of Adolf Weber, *Die Aufgaben der Volkswirtschaftslehre als Wissenschaft* ("The tasks of economic theory as a science"), *Archiv für Sozialwissenschaft und Sozialpolitik* 29 (1909), pp. 615–20. (Excerpts)
Q: *Adolf Weber*

"Über einige Kategorien der verstehenden Soziologie", *GAW*, pp. 427–74
("On some categories of interpretive sociology")
Q: *Categories*

"Die drei reinen Typen der legitimen Herrschaft"
("The three pure types of legitimate rulership")
Q: *Legitimate Rulership*

"Der Sinn der 'Wertfreiheit' der soziologischen und ökonomischen Wissenschaften", *GAW*, pp. 589–40
("The meaning of 'value freedom' in the sociological and economic sciences")
Q: *Value Freedom*

"Wissenschaft als Beruf", *GAW*, pp. 582–613
("Science as a profession and vocation")
Q: *Science*

English translations

The Methodology of the Social Sciences (ed., tr. Edward A. Shils, Henry A. Finch), New York: The Free Press, 1949

Roscher and Knies. The Logical Problems of Historical Economics (tr., intr. Guy Oakes), New York: The Free Press, 1975

Critique of Stammler (tr., intr. Guy Oakes), New York: The Free Press, 1977

Archival references

GStA I Geheimes Staatsarchiv Preußischer Kulturbesitz, Hauptabteilung I (Berlin)
BSB Bayerische Staatsbibliothek (Bavarian State Library) (Munich)

Other abbreviations

The present volume as a whole will be referred to in abbreviated form as *Meth. Writings*.

The journal *Archiv für Sozialwissenschaft und Sozialpolitik* ("Archive for social science and social policy") will be referred to in abbreviated form as *Archive*.

INTRODUCTION

Hans Henrik Bruun and Sam Whimster

Methodology is not a hard and fast category. In Max Weber's day, it could perhaps be said to refer, in formal terms, to the constitution, by means of concepts, of scientific knowledge. Methodology in this sense was linked to the discussion of the constitution, by means of categories, of knowledge in general (epistemology), to formal logic, and also to the discussion of theoretical approaches or systems within a particular branch of scientific investigation (scientific theory). This is the general criterion that we have applied in identifying the "methodological" parts of Weber's oeuvre (although it must be said that Weber himself was not always consistent in his use of terms such as "method", "logic" and "methodology").[1] It should be noted that the sense of the term "methodology" adopted for this purpose may differ from the way in which the term is used nowadays, where the emphasis is often on practical methods of survey analysis and the use of qualitative data and related concepts of validity and reliability.

The selection of texts

The editors have sought to reflect the range of Weber's methodological concerns to the widest possible extent by bringing together in this volume, as far as possible, all his methodological writings. It may be an overstatement to say that this collection is *complete*: when one is dealing with the whole, vast corpus of Weber's writings, in which many methodologically relevant passages have to be identified and excerpted from larger units of material, such a claim would necessarily be subjective. But we can confidently claim that what we present is by far the most extensive collection of Weber's methodological writings published to date, in any language. Almost a hundred pages of text included in the present volume have never appeared before in full in English; and most of the notes and drafts, and some of the letters, have never before been published in full, even in German. Moreover, we feel justified in adding that, in terms of textual criticism, the state of the texts translated in the *Methodological Writings* (apart from a few texts and many letters, which have already appeared in the *MWG*) is better than that of any other published version, in any language.[2]

1 For a detailed discussion, see Bruun, *Methodology*, pp. 7–11.
2 For a detailed description of the state of the texts included, see pp. xxix–xxx.

The main source of Weber's articles translated in the *Meth. Writings* is the German collective volume, *Gesammelte Aufsätze zur Wissenschaftslehre* ("Collected Essays on the Theory of Science"), originally published shortly after Weber's death by his widow Marianne. Some of these texts, however, turn out to be borderline cases.

Thus, we have it on Weber's own authority that the *Categories* essay is a hybrid between methodology and sociological theory.[1] It would have been unwarranted, however, to include only the "methodological" first part of the essay in the present volume, since Weber himself, in three separate letters to his publisher, says that (the whole of) *Categories* should form part of a planned collective volume of "methodological" essays.[2]

There are shadowy references in *Categories* to a study of what Weber calls "legitimacy consensus". The essay on *Legitimate Rulership*, which was posthumously published in *GAW*, is certainly such a study, and the *Categories* references might be seen as an argument for including it in the *Meth. Writings*. But, in substance, *Legitimate Rulership* deals with sociological theory, not with methodology, and the essay has therefore not been included here.[3]

As far as the important piece on *Basic Concepts* is concerned, it does contain methodological considerations of the same kind as those to be found in the first three sections of *Categories* – indeed, *Categories* can in many respects be seen as an early draft of *Basic Concepts*. However, *Basic Concepts* was written for, and has its natural place within, the framework of *Economy and Society* (*WG*); moreover, in the form in which it is included in *GAW*, it is only an *excerpt* from the first chapter of *WG*, not a fully rounded unit. It has therefore been left out of the present volume and belongs properly to the final version of WG.

All the other essays in *GAW* have been included in the *Meth. Writings*, since each of them indisputably and as a whole falls under the heading of methodology as we understand it.

A careful screening of all other printed Weber texts of a methodological nature has led to the inclusion of three further items: the *Accompanying Remarks*, the *Declaration* and the *Adolf Weber* review. These items raise other problems, however.

In the case of the *Accompanying Remarks* and the *Declaration*, the problem is that of authorship. The major part of the *Accompanying Remarks* was probably drafted, not by Weber, but by Werner Sombart.[4] However, it is highly probable that Weber left his definite mark on it, and the text as a whole, which is signed by all three editors of the *Archive* (of whom Weber was one) is closely linked with Weber's *Objectivity* article, which it was originally meant to precede immediately. The *Declaration* was signed both by Max Weber and by Werner Sombart, but it was a rebuttal of remarks made in an article (by their co-editor Edgar Jaffé!) in which they were both singled out for comment, and the text seems to bear the clear imprint of Weber's style and ideas.

1 In a letter to Heinrich Rickert of 5 September 1913 (see below, p. 407), he says that text of *Categories* "starts with some 'method[olog]ical' remarks, although all the purely logical aspects have been 'minimized' as much as at all possible". This undoubtedly refers to Sections I–III of the essay.

2 Letters of 24 May and 1 December 1917 (*MWG II/9*) and 8 November 1919 (BSB, Ana 446). In the 1919 letter, Weber also suggests including his "Survey on the Selection and Adaptation of the Work Force of the Self-Contained Heavy Industry". The fact that Weber proposes this Survey, which almost wholly deals with *method* and not with *methodology*, for inclusion in what he himself calls a "collection of methodological–logical essays" is eloquent proof of the inconsistency of his terminology in this area. (See also the Glossary entry on *Methode*.)

3 It should be noted that *Legitimate Rulership* did not form part of the first or second edition of *GAW*. Its inclusion in the third edition was justified by the editor, Johannes Winckelmann, precisely on the basis of the above-mentioned internal references.

4 For the full discussion, see the Introduction to the *Accompanying Remarks*, p. 99.

The *Adolf Weber* review raises a different problem: Should all texts be translated in full, or should only the (in our judgement) methodologically relevant passages be included? Ideally, of course, everything should be included, so that the selection is left to the discretion of the reader. But a moment's consideration of the texts involved – in particular the interventions and letters – will show that this procedure would increase the bulk of the volume to unacceptable proportions, with little substantive gain. The reader would need to spend much unprofitable time sifting long passages dealing with theoretical or substantive issues of quite a different sort in order to identify the occasional methodological nugget. Others may dispute some of the choices that we have made in selecting the passages that seemed relevant; but we feel reasonably confident that everything of general methodological importance has been included. Any deletion has, of course, been carefully indicated in the text.

Weber's active involvement in the contested field of methodology meant that he was not content just to write critical reviews and essays. He attended conferences, debating and arguing with colleagues and insisting on proper methodological approaches. Marianne Weber collected these interventions in the debates of the German Sociological Society and the Association for Social Policy in her edition of Weber's *Gesammelte Aufsätze zur Soziologie und Sozialpolitik* (1924). In these interventions, Weber is often at his most fluent and polemical. All the passages that deal with methodology have been selected for inclusion in this volume.

The Letters section comprises a large number of excerpts of methodological importance taken from Weber's correspondence. Here, too, the style is direct, and no punches are pulled. An emblematic example of Weber's delight in polemics is his brutally friendly demand that his correspondent (Friedrich Gottl) should "polemicize *as sharply as possible*" when discussing points where he disagreed with Weber (*MWG* II/5, p. 50). The exchanges of views reflected in the letters provide some of the freshest and most illuminating insights into Weber's own thinking.

We are presented with similar rewarding insights in the Notes and Drafts, where Weber notes his first, unvarnished reactions when confronted with new books (for instance, by his friend and mentor Heinrich Rickert), or launches himself (as in the draft on Simmel) into rough-and-ready discussions of the ideas of others. Here, the selection of texts has been based on the same principles as elsewhere, but, occasionally, a text has had to be deselected in spite of the interest that it might present because it seemed, even as a whole, to be too fragmentary or delphic to be of wider interest.

Existing English translations

All of the essays of *GAW* have already been translated into English. Some of the interventions and one or two of the letters included here have also been translated. The problems that these translations present the anglophone reader with, however, are multiple. To begin with, almost all of the translations are more than thirty years old. The historic translation by Edward Shils and Henry Finch of three of the main essays (*Objectivity*, *Critical Studies* and *Value Freedom*) was even published well over half a century ago, in 1949. This is not just a question of age, but also one of context: in Weber's day, these essays were authoritative, but occasional interventions: setting the new direction of a leading social science and policy journal, correcting an esteemed classical historian, or establishing the protocol of handling values in the debates of the Association for Social Policy. Post 1945, however, they were taken as the systematic bedrock for those social sciences that demanded the assurance of objectivity and empirical science when dealing with the meanings and intentions of volitional social actors.

Moreover, the translations are widely scattered: in book form, apart from the Shils/Finch edition, we have the translation by C. Wright Mills and Hans Gerth of *Science* (1947), as well

as the two major separate translations by Guy Oakes of *Roscher and Knies* (1975) and *Stammler* (1978). The rest of the translations of articles (*Accompanying Remarks*, *Marginal Utility*, *Energetics* and *Categories*), as well as the translated interventions and letters, were published in a wide range of not always very accessible journals.

All these translations were pioneering, but they were of widely varying quality. Each solved translation problems in its own way, so that the anglophone reader with a more general interest in Max Weber's methodology has hitherto lacked an integration of the translation terms employed. In the same way, the indispensable editorial apparatus of introductions, explanatory endnotes, glossary, bibliography and indexes has obviously been fragmented (if indeed it was present at all, which was by no means always the case). All the editorial apparatus of this kind is provided in integrated form in the present volume.[1]

Max Weber as a methodologist

Max Weber only started to write on methodological issues, properly speaking, after his recovery from a serious and lengthy nervous breakdown around the turn of the century. Before then, he had pursued a successful and productive research career in fields as different as legal and socio-economic history and contemporary survey research. The methods that he employed as a legal scholar, as an agrarian historian and in his survey analyses were, by the standards of the day, extremely sophisticated (on important points, they may even be seen as foreshadowing his post-1900 methodological positions). But method, as a body of skills and expertise, was something that was taught and discussed in the professorial seminar. It must be explicated in scholarly publications, but that was all.

There was therefore no intrinsic reason why Weber, on the road to recovery, should turn to writing dedicated articles in the difficult field of methodology. He certainly did not regard such studies as an end in themselves, as he wrote in an article in 1906:

> Methodology can never be more than a self-reflection on the means that have *proved useful* in [scientific] practice; and one does not need to be made explicitly aware of those means in order to produce useful work, just as one does not need to have knowledge of anatomy in order to walk "correctly".
>
> (p. 140)

This severely instrumental view of methodology remained unchanged until the end of his life. According to a report by a student who took part in his seminar on sociological categories in the summer of 1919, Weber had, only half jokingly, said that he had almost wanted to throw the notes for this seminar out of the train window (he had just arrived in Munich from Heidelberg); and, towards the end of the semester, he delivered himself of the following clipped judgement: "Method is the most sterile thing that exists [. . .] Method *alone* has never yet created anything".[2]

It seems probable that, in fact, the determining factor behind at least Weber's first properly methodological work (*Roscher and Knies*) was simply one of academic courtesy: he had felt unable to evade a request by his colleagues for an article, for a quasi-official *festschrift*, about his immediate predecessor as professor of economics at the University of Heidelberg, Karl Knies[3]

1 See p. xxx.
2 GStA Berlin I, Rep. 92, Nl. Max Weber, no. 30/5.
3 See the Technical Introduction to *Roscher and Knies* below, p. 94.

– a piece of work which then quite outgrew its original proportions (and outran its deadline), ending up in the unwieldy shape of three lengthy methodological articles discussing a number of authors besides Knies. However, the 400 or so pages of this volume are weighty testimony to the fact that, once engaged, willy-nilly, in the field of methodology, Weber was driven to involve himself deeply in a variety of methodological issues that he found too important to be left unresolved or, worse, were being treated in damagingly erroneous formulations. His methodological writings thus became a running commentary on developments and new approaches during a period of rapid expansion of academic knowledge. The complexity of these developments is reflected in the range of topics and subject materials Weber was prepared to tackle: philosophy (in particular epistemology), history, politics, ethics, religion, economics, psychology, psychiatry, law, philology, statistics and sociology, as well as the natural sciences (where the range of his knowledge is impressive).

Weber never wrote a book on methodology (indeed, he did not write a single new book from 1891 to the end of his life). All his methodological work is in article or pamphlet form and has an occasional background (reviews, editorial introductions or contributions to debates, lectures, and the like), and this variety of forms is matched by a variety of styles. Weber himself ruefully described *Roscher and Knies* as a "wretched patchwork, in which [I] in fact work almost exclusively with other people's ideas" (p. 374). At least, *Roscher* has a single subject – the methodological naïvety of one "old master" of German economics – but the two *Knies* essays are a disjointed succession of shapeless and often awkwardly formulated critiques of authors whose writings and theories were important in their day, but have now largely been forgotten. At the same time, however, Weber's presentation and discussion of problems already demonstrate considerable philosophical sophistication; his comments are often both acute and profound; and his growing enjoyment of this kind of intellectual task is evident. *Objectivity* has quite a different, and much more accessible, character. It provides a clear exposition of the methodological principles on which a social science journal mainly devoted to social economics should proceed. This leads Weber to a painstaking examination and relativization of the contentious concept of "objectivity", and to a lengthy presentation and discussion of the logic and possibilities of the ideal type concept, which is one of Weber's main contributions to the methodology of the social sciences (see pp. 190–204). Here, we see Weber at his most pedagogic and constructive. *Critical Studies* is an elegantly polite argument with the methodological views of a distinguished historian, Eduard Meyer. "The intention is to take M[eyer]'s treatise as a starting point, to go on by illustrating, one by one, a number of separate logical problems, and then to give a critique, from the standpoint thus arrived at, of a number of other recent works dealing with the logic of the cultural sciences" (p. 140). On the basis of his specific criticism of Meyer for equating the voluntarism of action with the privilege of the irrational, Weber presents his own solution to this central problem of social theory. In his subsequent methodological essay on Stammler, and the posthumous *Addendum*, Weber launches a massive attack on this influential thinker who, by claiming that he could, on a Kantian conceptual basis, "overcome" the materialist conception of history and furnish the historical sciences with a secure methodological basis, in many respects seemed to occupy the same intellectual territory as Weber himself. Weber was quite aware of this dangerous proximity, and he is correspondingly vehement in his treatment of Stammler's – in his view – deliberate conceptual confusion and unclear presentation, employing rhetorical devices of sarcasm and ridicule to considerable (albeit often clumsy) effect. *Marginal Utility* is another extended review essay that provides Weber with the opportunity to criticize the use of psychology in underpinning economic theories of marginalism. Because of his respect for the author in question (Lujo Brentano), Weber's critique in this essay is much more constructive and deferential. But, in the next one (*Energetics*), there is little sign of such respect.

The Nobel Prize-winning chemist Wilhelm Ostwald had imprudently tried to extend the significance of his "energeticist" approach into the wider fields of social life, and this provides Weber with an occasion for hauling him, with great energy and occasional flashes of grim humour, over some very uncomfortable methodological coals.

This marks the end of the six-year period (1903–1909) of Weber's concentrated involvement with methodology. What follows are some more incidental pieces, albeit of major significance. In *Categories*, the polemical aspect is entirely absent. The author is here at his most innovative and constructive, as he combines fundamental methodological discussions with an attempt to fashion a systematic conceptual language of interpretive sociology. *Value Freedom* spells out, clearly and in great – occasionally somewhat tedious – detail, the implications of Weber's central concept of "value freedom" for academic teaching, for the logical analysis of value positions in a number of different fields, and for empirical investigations. Finally, in *Science* – the reworked text of a lecture – we can hear Weber's distinct speaking voice as he presents the over-expectant students with a cool and dispassionate analysis of the current situation of the universities, followed by an intensely moving personal statement of the preconditions and consequences of embracing science as a vocation.

The occasional and often polemical character of Weber's published methodological work is compounded by his frequent and deliberate neglect of literary form,[1] and by his impatience to move on to ever new fields of investigation.[2] Necessary details, for example, are often found and secured in footnotes or within rampaging critiques, rather than developed through sober exposition. Nor was Weber consistent in his definition and use of terms such as methodology, theory, interpretation, and even ideal types.

Against this background, it is understandable that there have been prolonged and heated debates among Weber scholars as to whether Weber's writings on methodology should be read as a unified theory of knowledge of the social sciences, or whether they are simply to be taken as a collection of occasional pieces, each with its own agenda. Some bold commentators, such as Wilhelm Hennis, have searched out central questions that in their view govern the whole of Weber's oeuvre. Others (Alexander v. Schelting, Dieter Henrich, Thomas Burger, to name but a few) have claimed to find an underlying unity at least in Weber's methodological thought, often tied to the systematic ideas of neo-Kantian thought; the opposing camp, with Friedrich Tenbruck as its most radical protagonist, reject the notion that Weber's methodological writings constitute a systematic whole.

In looking for an answer to this question, we lack the aid of a major synthetizing introduction by Weber himself to a collection of his methodological writings (such as the one written, for

1 Weber's adoring widow Marianne is a reliable witness in this respect: "Once the material is in flow, so much streams out of the storehouse of his mind that it often cannot be forced into a transparent sentence structure. And after all, he wants to finish the work quickly and, if possible, to be brief, since reality presents him with ever new problems [. . .] Much has to be formulated quickly in sentences that pile up one after the other, and what cannot find room there must be accommodated in footnotes. The reader will 'kindly' have to take as much trouble with [the result] as he himself had taken!" (Weber, *Biography*, p. 322). This is not just an ex post justification: In a revealing early postcard from Marianne Weber to her husband, who is sending her bits of manuscript for *Roscher and Knies* to transcribe, she says. "It is a pity that you put so many wise comments into the footnotes: they should be in the main text, that is much nicer" (30 December 1902; Düsseldorf Max Weber Centre). If Max Weber actually took this suggestion to heart, we can only quake at the thought of what we might otherwise have been faced with: already as it is, major parts of his early methodological work abound in often page-long footnotes.

2 A telling circumstance testifying to this is the constantly recurring footnote at the end of Weber's articles: "A further article was planned".

example, for his studies on the economic ethic of world religions). As mentioned above (p. xii), he had made plans, together with his publisher, for such a collected edition to appear after the war. Because of his untimely death in 1920, the title under which the methodological essays were brought out by his widow Marianne in 1922 (*Gesammelte Aufsätze zur Wissenschaftslehre* ("Collected essays on the theory of science")) was certainly not that envisaged by Weber himself.[1]

As editors, we do not wish to venture into pronouncements on such large and intractable differences of interpretation. What we have done in this volume is to lay the evidence, in its totality, before the reader. In addition to this abundant material, we only wish to offer some brief observations by way of initial guidance:

In spite of the fragmented and unsystematic appearance of Weber's methodological writings, his thinking about methodology does tend to keep to defined, if not always precise, tracks. The outlines of his own methodological stance can often be traced through the condemnation and refutation of his enemy's position.

This collection of his voluminous and disparate writings in this field, with their many overlaps, repetitions, reworkings and different applications, therefore makes it possible to accumulate a number of his key ideas under certain headings. We may think of these headings as silos, and, as we proceed through his methodological writings, these silos fill up. The contents are not always well structured, but one can at least impose a certain basic order on the material in this fashion. This facilitates the attainment of a more coherent understanding of Weber's methodology, or at least a more precise identification of the gaps and unresolved issues that it contains.

The historical background

Weber's engagement with methodological problems took place against a background of fundamental disagreements about how science and academic knowledge should be conceived, and about their relationship to practical policy. These controversies had already lasted for decades. Following the era of liberal economic policy in Prussia and the North German Confederation in the 1860s, the Association for Social Policy was founded under the leadership of Gustav Schmoller, who argued that the policies of the state should override the *laissez aller* of liberalism. Schmoller and his leadership of academics within the Association for Social Policy were known as "academic socialists". Max Weber belonged to a younger academic and political generation of such "academic socialists" who, while accepting the basic orientation of their elders, were opposed to their belief that academic research could and should in itself provide ethically based social and economic policies.[2] Weber vigorously led the opposition to this attitude, arguing that policy was a matter of practical judgement and could not be directly derived from scientific research.[3] But, at the same time, another, more general debate was in full swing between the methodological claims of the natural sciences, on one side, and the claims of the humanities and social sciences to pursue their own distinctive methods, on the other. By the end of the nineteenth century, enormous advances had been made by the natural sciences, whose objectivity seemed beyond doubt, and this led their disciples to claim a methodological monopoly in all

1 For an overview, see Bruun, *Methodology*, p. 3.

2 For a general history of the Association for Social Policy, see Lindenlaub, *Conflicts*.

3 His inaugural lecture as professor of economics in Freiburg astounded its academic audience by the brutality with which this was expressed, and Weber himself, in a letter to his brother (GStA I, Nl. Max Weber, No. 4, letter of 17 May 1895), expressed satisfaction at having given the "ethical culture" a "firm kick".

scientific disciplines. The cultural and historical sciences were constantly on the defensive against this self-confident positivism (see Ringer, *Mandarins*). In this debate, Weber was squarely on the side of the historians and the humanities in rejecting the sociological positivism of Comte and Spencer and other naturalistic methodologies. Within the science of economics (where the debate was known as the "quarrel about methods"), Weber opposed the extreme idea that economics could or should simply be reduced to quantifiable laws. But we know from his lectures of the 1890s, and from remarks in the methodological essays, that his sympathies were really on the "Austrian" side of the quarrel, and he approved of what he saw as the balanced theoretical stance of its chief spokesman, Carl Menger (see below, p. 249).

Weber's position in this debate, which owed much to the views of the neo-Kantian philosopher Heinrich Rickert (see pp. xix–xxi), was above all set out in *Objectivity*: "The social science that *we* want to pursue is a *science of reality*. We want to understand *the distinctive character* of the reality of the life in which we are placed and which surrounds us . . ." (p. 114). Weber was pleading for an independent methodology unimpeded by claims that lay outside the proper sphere of social science.

Philosophy of history

In the three essays on Roscher and Knies, Weber conducts a critique of a wide range of philosophies of history: the Hegelian philosophy of the movement of reason in history, the materialist philosophy of history in its naturalistic, evolutionist, monist and Marxist guises, and neo-Hegelian philosophy as represented by Roscher and Knies and by the Italian philosophers, Croce and Lipps. In *Energetics*, Comte's philosophy of history of evolutionary stages is attacked, and Comte himself is dismissed as a "grandiose pedant" (p. 260). Weber sums up his account as follows: "The fracture in both Knies' and Roscher's basic epistemological [position] must be explained by the withered remainders (which have [moreover] been warped in an anthropological–biological direction) of Hegel's great ideas; these remainders were still very characteristic of the philosophies of history, language and culture of various influential currents of thought in the middle decades of the last century" (p. 93). What these philosophies of history had in common was the assumption that history proceeded on the basis of hidden yet ineluctable forces. Development or evolution, as he noted in a fragment, "is not some hidden agent behind the course of history, so that our foremost task would then be to ferret it out everywhere and without exception and to extract its essence from the material" (p. 416). At the level of research methods, adherents to a philosophy of history were often involved in the close scrutiny and analysis of facts and phenomena. This might look like academic science, but, in Weber's eyes, that appearance was deceptive. These scholars were simply looking for material that would underpin their preconceptions. For Weber, this was a totally unacceptable starting point for scientific and empirical research, the more so when it was pretended that the facts could "speak for themselves" (p. 346).

Neo-Kantianism

While Roscher, Knies and others represented the remaindered part of Hegelian panlogism, the new and formidable enemy was what Weber termed "naturalistic monism" (p. 122). As was described above, the rapidly growing prestige of the natural sciences had led many to believe that by "uncovering law-like regularities, [it was possible] to arrive at a monistic knowledge of total reality" (p. 122). In particular, when it seemed to become possible to encompass within a "generally valid evolutionary principle" everything of importance in the field of *biology* – the

science of life itself – "then it looked as if the twilight of the gods for all valuational viewpoints in all sciences was drawing near" (p. 122). In other words, the battle against what is now termed "positivism" was about to be lost.

One possible defence against the positivist onslaught would be to demonstrate that the objects of the "humanities" (as we would call them) were intrinsically different from those with which the natural sciences dealt. This was the view put forward by Wilhelm Dilthey, who argued that knowledge of the object of the "sciences of the human spirit" ("Geisteswissenschaften") – the "spirit" – required intuition and empathy, which did not form part of the methods of natural science. This solution had a natural appeal, as it corresponded to what actually went on in many branches of the humanities. But it suffered from a central weakness, since the very science dealing with the "spirit" – psychology – seemed increasingly accessible to a treatment along the lines of the natural sciences. (Weber's strong aversion to any "psychologistic" interpretation of economic concepts such as "value" (as discussed in *Marginal Utility*) or of basic logical truths was no doubt in part due to his desire to avoid being identified with Dilthey's line of thought.)

Instead, it was the neo-Kantian school of philosophy, in particular its so-called "South-Western" branch (Windelband, Rickert and Lask), that opened what seemed to be the right path out of the humanist predicament, through a return to Kant's epistemology. Kant's basic idea was that we can only attain knowledge of the world around us indirectly, by means of categories and concepts: as Weber himself put it towards the end of *Objectivity*, "the basic idea of modern epistemology, which goes back to Kant, is that concepts are, and can only be, theoretical means for the purpose of intellectual mastery of the empirically given" (p. 123). Therefore, the important thing was to show that the methods and concepts on which knowledge in the humanities was necessarily based were essentially different from those which the natural sciences, with equal necessity, had to employ.

It was Wilhelm Windelband who first, in a lecture in 1894 (*History and Natural Science*), established the argument for separating the natural sciences from the "historical sciences" on the basis of differences of approach rather than of subject matter. The approach of the natural sciences, he said, was "nomothetic" ("law-seeking"), but the historical sciences are "idiographic" ("describe what is distinctive"): they want to know about the individual or concrete reality, whose particularity is unique and unrepeatable.

Windelband's distinction was central. But he did not elaborate the argument underlying it, nor did he face up to the challenge of producing a convincing argument that established the idiographic method as having a methodological status as firm and unquestioned as that of the generalizing natural sciences.

This was left to the great systematizer of the movement, Heinrich Rickert, to accomplish in his book *The Limits of Concept Formation in the Natural Sciences*, whose second part was published in 1902. His epistemological point of departure was that the historian – like any scientist – confronts a reality (usually in the past) that presents an infinite multiplicity of phenomena, both intensively and extensively: the horizon of the past can be infinitely extended, and at any one point there exists a reality that can be subdivided into ever more particular components (Rickert, *Limits of Concept Formation*[5], pp. 35–38). How, then, would the historian select those elements of infinite reality that he wanted to deal with? Here, Rickert argued that the concepts of the historical sciences (corresponding to Windelband's "idiographic" disciplines) are formed by being linked to theoretical values, by a "value relation" ("Wertbeziehung"). These values, in their turn, in order to have general validity, must be valid for a community: they must be "cultural values". Thus, designating a period of history under a concept, as for example when Jacob Burckhardt named a period in fifteenth- and sixteenth-century Italian history "the Renaissance", not only created an analytic concept, but also established a value relation between

what happened in the past and the present value interests of the historian.[1] This, in brief, is Rickert's central thesis, his central concept, and the central prop underpinning the status that he wishes to ascribe to the historical (or "cultural", as he also calls them) sciences.

There is no doubt that Weber was much impressed with Rickert's book, which he read immediately after its publication. A series of postcards to his wife (p. 374) chart his progress through the work, which, he concluded, was "*very* good". His early methodological essays are full of references to Rickert and to the logic of his concept of "value relation". This was precisely the instrument that he needed to cut down the various pernicious methodological errors of the day, as he identified them. In particular, *Objectivity* applied the neo-Kantian standpoint towards knowledge to the practical sphere of social economics, which at its most extensive was concerned with the determination of cultural life by economic factors and, conversely, how economic life is influenced by culture.

But, at the same time, one senses that Weber's adherence to the Rickertian doctrine is not quite wholehearted. He has "doubts about the terminology", he writes to his wife (and to Rickert himself), and his own methodological essays are designed, he says, to "test" the usefulness of Rickert's very abstract philosophical constructions within more down-to-earth scientific disciplines. In any event, as we shall see, there were limits beyond which he could not follow Rickert's construction. Ever since the earliest systematic exposition of Weber's methodology (Schelting, *Theory of Science*), and particularly since the early 1970s, a constant debate has therefore been going on as to the precise extent to which Weber's methodology is dependent on Rickert's thought.[2] In the section on "Values" below, we shall return to some of the premises of this debate, which remains inconclusive, although one may perhaps see a growing tendency to stress Weber's independence of his philosophical mentor (and close personal friend). Certainly, some of the drafts reproduced below (pp. 413–14) seem to point in that direction. At any rate, the debate has given added point to the cruel jest by the German philosopher Karl Jaspers, who, in a heated argument with Rickert a short time after Weber's death, when Rickert denied Weber the rank of philosopher, retorted that Rickert's own philosophy would only be remembered through Max Weber's writings.[3]

The third member of the South-West German school, Emil Lask, although still quite young at the time (born in 1875, he was Rickert's pupil), made his independent contributions to Weber's conceptual universe. He coined the term "emanationism", which Weber took over, for Hegelian and neo-Hegelian theories where reality was seen as an emanation of abstract concepts; and he figuratively burnt any vestigial bridgehead from concrete reality to scientific deductions and inferences by arguing in his doctoral thesis of 1901 (*Fichte's Idealism*) that a *hiatus irrationalis* – an irrational chasm – separated the bruteness of reality from the way that scientists and academics construed and manipulated that reality by their methods. As Guy Oakes explains this extreme version of scepticism: "There is a hiatus because reality cannot be derived from concepts. And it is irrational because reality can be rationalized only by conceptualizing it, which according to analytic theory is impossible" (Rickert (Oakes), *Limits of Concept Formation*, p. xvi).

1 The example is illustrative, and is not Weber's own.

2 For a recent overview and discussion, see Bruun, *Methodology*, pp. 7–11, 115–64.

3 See Adair-Toteff, *Confrontation*. In fact, Weber never claimed to be a philosopher, though he had followed philosophical lectures at university and was quite capable of arguing on equal terms with Rickert on philosophical matters.

Values

The "values" silo overflows with methodological contributions from Weber's hand, of many different kinds. The concept of "value relation" had allowed the neo-Kantians to define a method that seemed to match the generalizing method of the natural sciences. But the question was whether the results obtained by this method of value relation could claim the same kind of objectivity that the generalizing sciences so confidently boasted of. Rickert tried to solve this problem by way of an intricate philosophical argument intended to demonstrate that the values in question were not just generally valid empirically (for a particular "culture"), but that they had a necessary connection with values that were in their turn (metaphysically) objective. Philosophically, this argument made (neo-Kantian) sense. But there is general agreement that Weber did not accept it. Nowhere in his writings do we find him embracing it, and, in the note usually referred to as the "Nervi fragment" from 1903, he definitely distances himself from such metaphysical constructions (pp. 413–14). More than that, he regularly, and with great rhetorical vehemence, advances the exactly opposite view: that the ultimate values are in necessary conflict, and therefore can in no way be said to be objective.

In fact, this conception of a fundamental "value conflict" is the necessary corollary of the principle of the "value freedom" of science, which is a major Weberian contribution to the methodology of the social sciences. Value freedom as a principle was also part of Rickert's argument. The values to which reality was related in order to guide the selection of material for the "cultural" or "historical" sciences were theoretical, not practical. A political history of Germany, for example, would rest on the conviction that politics as such was a cultural value, but it would not imply the taking of any particular political side. Rickert even went so far as to opine (after Weber's death) that Weber owed the idea of the value freedom of science to him.[1] But that was going too far. Weber accepted the logic of value freedom in its methodological form, but there is often a characteristic fuzziness about his pronouncements in this context. His memorable statement that "the transcendental precondition of every *cultural science* is *not* that we find a particular, or indeed any, 'culture' *valuable*, but that we *are* cultural *beings*, endowed with the capacity and the will to adopt a deliberate *position* with respect to the world, and to bestow *meaning* upon it" (p. 119; Scaff, *Iron Cage*, pp. 83–87) certainly does not make a strong case for "theoretical" values. And when Weber argues that social scientists are forced to make a choice as to what aspect of that infinite reality they will study, he says that this choice is governed by the "value ideas" of the social scientist, which determine what is "worth knowing": "only a finite *part* of [that infinite reality] should be the object of scientific comprehension, – should be 'important' (in the sense of 'worth knowing about')" (p. 114). Again, these, "value ideas" may well be practical rather than theoretical.

But, if Weber sometimes seems a little unwilling to reproduce Rickert's rather bloodless philosophical version of the argument concerning value freedom (the more so, perhaps, as it was ultimately tied to a system of metaphysically objective values), he is on the other hand entirely clear and consistent in his demand for value freedom in practice, as far as both scientific concepts and – not least – scientific results are concerned: science cannot, and therefore should not pretend to be able to, prove the validity of any practical value whatsoever, he says, over and over again. As is to be expected, this argument in its manifold ramifications forms the main substance of Weber's essay of *Value Freedom*. But it is noteworthy that it is already present, fully fledged, in his early (1904) essay on the "objectivity" of the social sciences. (In fact, this essay – *Objectivity* – mainly deals with the *subjective* preconditions of science, that is to say: with its

1 Rickert, *Limits of Concept Formation*[5], p. 758.