

# **New Rules of Sociological Method**

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A Positive Critique of  
Interpretative Sociologies

**Anthony Giddens**

*Second Edition*

Polity Press

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## Preface

This study is only intended as one part of a more embracing project. While it can of course be read as a self-contained work, it touches upon various issues that are not dealt with in a detailed way, but which are vital to my project as a whole. This latter involves three overlapping concerns. One is to develop a critical approach to the development of nineteenth-century social theory, and its subsequent incorporation as the institutionalized and professionalized 'disciplines' of 'sociology', 'anthropology' and 'political science' in the course of the twentieth century. Another is to trace out some of the main themes in nineteenth-century social thought which became built into theories of the formation of the advanced societies and subject these to critique. The third is to elaborate upon, and similarly to begin a reconstruction of, problems raised by the – always troubling – character of the social sciences as concerned with, as a 'subject-matter', what those 'sciences' themselves presuppose: human social activity and intersubjectivity. This book is proposed as a contribution to the last of these three. But any such discussion bursts the bounds of this sort of conceptual container, and has immediate implications for work in the other areas. As a single project, they are tied together as an endeavour to construct a critical analysis of the legacy of the social theory of the nineteenth and early twentieth centuries for the contemporary period.

This book is about 'method' in the sense in which social philosophers characteristically employ the term – the sense in which Durkheim used it in his *Rules of Sociological Method*. That is to say, it is not a guide to 'how to do practical research', and does not offer any specific research proposals. It is primarily an exercise in clarification of logical issues. I have subtitled the study a 'positive critique' of 'interpretative sociologies'. Anyone who reads on will see that this does not mean 'positivistic'. I use it only to mean 'sympathetic' or 'constructive': the sense that predates Comte's translation of the term into a definite philosophy of social and natural science. 'Interpretative sociologies' is something of a misnomer for the schools of thought that appear in the first chapter, since some of the authors whose work is discussed there are anxious to separate what they have to say from 'sociology'. I use the term only because there is no other readily available one, to group together a series of writings that have certain shared concerns with 'meaningful action'.

The themes of this study are that social theory must incorporate a treatment of action as rationalized conduct ordered reflexively by human agents, and must grasp the significance of language as the practical medium whereby this is made possible. The implications of these notions are profound, and the book is confined to tracing through only some of them. Anyone who recognizes that self-reflection, as mediated linguistically, is integral to the characterization of human social conduct must acknowledge that such holds also for his or her own activities as a social 'analyst', 'researcher', etc. I think it correct to say, moreover, that theories produced in the social sciences are not just 'meaning frames' in their own right, but also constitute moral interventions in the social life whose conditions of existence they seek to clarify.

## Introduction to the Second Edition

Quite a number of years have passed since this book first saw the light of day, but I hope it has not lost its relevance to current problems of social theory. In *New Rules* I deal with a number of forms of interpretative sociology, as well as with certain more central sociological traditions. When I wrote it, I regarded the book – and continue to do so today – as a 'dialogic critique' of the forms of social and philosophical thought which it addresses. That is, it is a critical engagement with ideas that I see as of essential importance, but which for one reason or another were not adequately developed in the perspectives from which they originally sprang. Some have seen such a strategy as a misplaced eclecticism, but I consider such dialogic critique as the very life-blood of fruitful conceptual development in social theory.

*New Rules of Sociological Method* dovetails with other 'positive critiques' which I sought to provide in elaborating the basic tenets of structuration theory. In complementary writings that I undertook at about the same period, I addressed approaches to social analysis either left aside, or treated only in a marginal way, in *New Rules*. Such approaches included naturalistic sociology – a term which I now think of as preferable to the more diffuse and ambiguous label, 'positivism' – functionalism, structuralism and 'post-structuralism'. *The Constitution of Society* (1984) established a more comprehensive framework for the notion of

structuration than was available in *New Rules*, but did not supplant it.<sup>1</sup> *New Rules* makes an independent statement about questions of agency, structure and social transformation; its distinctive concentration is upon the nature of 'action' and the implications of an analysis of action for the logic of social science.

The debates have moved on over the period since *New Rules* was originally published, but in revising the text I have found little of substance that I think it necessary to abandon or reformulate. The work of Talcott Parsons still has its adherents and, as filtered through the writings of Niklas Luhmann and others, remains influential; but it no longer has the central position it once held. Phenomenological notions are not as widely drawn upon now as they were at the time, while post-structuralism, in its different guises, has increased its importance and has become allied to conceptions of post-modernism. I do not feel, however, that these changes make any substantial difference to the standpoint I developed in this study, which retains its validity.

*New Rules* has attracted its own share of critiques, some positive and others more destructive in impetus. I have responded to such criticism in a variety of places and shall not cover the same ground again here. Let me concentrate upon two issues only: whether or not the idea of the 'duality of structure', vital to structuration theory, merges levels of social life that should be kept apart; and whether the distinction between the 'single hermeneutic' of natural science and the 'double hermeneutic' of the social sciences should be sustained. The literature subsequent to the publication of *New Rules* contains many discussions of these problems. For purposes of simplicity, I shall focus upon those offered by Nicos Mouzelis in respect of the first question, and Hans Harbers and Gerard de Vries in respect of the second.<sup>2</sup>

Many critics have accepted the objections I made against the concept of structure as ordinarily used in sociology. Seen as 'fixed' and, in Durkheimian fashion, as 'external' to social actors, it appears as a constraint upon action, rather than also as enabling. It is to grasp this double character that I introduced the notion of the duality of structure. What are some of the objections that might be levelled against it? They include the following.

- 1 It may be true that actors routinely draw upon rules and resources, and thereby reproduce them, in the course of their day-to-day activities. Surely, however, such an orientation to rules and resources is not the only, or even the predominant, one they have? For, as Mouzelis puts it, 'Actors often distance themselves from rules and resources, in order to question them, or in order to build theories about them, or – even more importantly – in order to devise strategies for either their maintenance or their transformation.'<sup>3</sup>
- 2 Hence it follows that the idea of the duality of structure cannot properly account for the constitution or reproduction of social systems. Rules and resources are reproduced not only in the context of their practical use, but also where actors 'distance' themselves from them in order to treat them in a strategic way. When such a circumstance applies, the concept of the duality of structure is quite inappropriate. Instead, perhaps, we should speak of a *dualism*, because the individual, the 'subject', confronts rules and resources as 'objects' in the social environment.
- 3 These comments bear directly upon distinctions between micro- and macro-analysis in the social sciences. Although not discussed directly in *New Rules*, the micro/macro differentiation, as ordinarily understood, is something which I place in question. However, if we try to do without it, the critic asserts, the result is an illegitimate reductionism. Social systems have many structural properties which cannot be understood in terms of the actions of situated individuals. Micro- and macro-analysis are not mutually exclusive; each in fact requires the other, but they have to be kept apart.
- 4 The idea of the duality of structure cannot cope with action oriented to large- rather than small-scale contexts. For instance, it may work well when one considers an everyday conversation between two people in the street, but does not fit a situation where, say, a group of heads of state meet to take decisions affecting millions. The former situation, it might be said, is inconsequential in its implications for larger social orders, while the latter affects such orders in a direct and comprehensive way. In structuration theory there is an 'identification' of agency with 'micro-subjects which, by the

routine use of rules and resources, contribute to the reproduction of the institutional order. Macro action is neglected – both the type of action that results from the incumbency of authority positions . . . as well as that which results from the variable ability of individual subjects to group together in order to defend, maintain, or transform rules and resources.”<sup>4</sup>

- 5 The Durkheimian notions of externality and constraint need to be sustained, albeit perhaps not in the form in which Durkheim himself expressed them. There are degrees or levels involved; what is external and constraining for one individual may be much less so for another. This point connects with the previous ones, for it means recognizing that social life is hierarchical – rather than speaking of ‘the individual’ confronting ‘society’, we should acknowledge a multiplicity of levels of social organization, with varying degrees of disjunction between them.

In responding to such observations, let me first of all expand upon why I developed the concept of duality of structure. I did so in order to contest two main types of dualism. One is that found among pre-existing theoretical perspectives. Interpretative sociologies, such as those discussed in *New Rules*, as I have put it elsewhere, are ‘strong on action, but weak on structure’. They see human beings as purposive agents, who are aware of themselves as such and have reasons for what they do; but they have little means of coping with issues which quite rightly bulk large in functionalist and structural approaches – problems of constraint, power and large-scale social organization. This second group of approaches, on the other hand, while ‘strong on structure’, has been ‘weak on action’. Agents are treated as if they were inert and inept – the playthings of forces larger than themselves.

In breaking away from such a dualism of theoretical perspectives, the analysis developed in *New Rules* also rejects the dualism of ‘the individual’ and ‘society’. Neither forms a proper starting-point for theoretical reflection; instead the focus is upon *reproduced practices*. It is important, however, to be clear about what discarding the dualism of ‘the individual’/‘society’ means. It emphatically does not mean denying that there are social

systems and forms of collectivity which have their own distinct structural properties. Nor does it imply that those properties are somehow ‘contained’ in the actions of each situated individual. To challenge the dualism of the individual and society is to insist that each should be *deconstructed*.

Since ‘the individual’ has corporeal existence, the concept might seem unproblematic. Yet an individual is not a body and even the notion of the body, in relation to the acting self, turns out to be complex. To speak of an individual is to speak not just of a ‘subject’, but also of an agent; the idea of action (as Talcott Parsons always stressed) is thus inevitably a central one. Moreover – and this is crucial – action is not simply a quality of the individual but is, equally, the stuff of social organization or collective life as well. Most sociologists, including even many working within frameworks of interpretative sociology, have failed to recognize that social theory, no matter how ‘macro’ its concerns, demands a sophisticated understanding of agency and the agent just as much as it does an account of the complexities of society. It is precisely such an understanding that *New Rules* seeks to develop.

The concept of the duality of structure is bound up with the logic of social analysis; it does not, in and of itself, offer any generalizations about the conditions of social reproduction/transformation. This point is fundamental, because otherwise a structurationist view would indeed be open to the charge of reductionism. To say that the production and reproduction of social life are one and the same thing takes *no* position at all about the conditions of stability or change in concrete conditions of social activity. Rather, it is to say that neither on the level of logic, nor in our practical day-to-day lives, can we step outside the flow of action, whether such action contributes to the most rigid of social institutions or to the most radical forms of social change.

These things having been said, I can comment upon points 1–5 in sequence. Point 1 both misunderstands the notion of duality of structure and presumes too primitive a concept of reflexivity. All actors are social theorists, and must be so to be social agents at all. The conventions which are drawn upon in the organization of social life are never ‘blind habits’. One of the distinctive

contributions of phenomenology, and particularly of ethno-methodology, has been to show that (1) the conduct of social life continually involves 'theorizing' and (2) even the most enduring of habits, or the most unshakeable of social norms, involves continual and detailed reflexive attention. Routinization is of elemental importance in social life; but all routines, all the time, are contingent and potentially fragile accomplishments.

Individuals in all forms of society 'distance themselves' from rules and resources, approach them strategically and so forth. In some respects, for reasons just noted, this is the condition of even the most regularized modes of social reproduction. No matter how traditional a context of action, for example, tradition is chronically interpreted, reinterpreted, generalized about, as the very means whereby it is 'done'. Of course, all moments of reflexive attention themselves draw upon, and reconstitute, rules and resources; to repeat, there can be no stepping outside of the flow of action.

The sort of 'distancing' Mouzelis has in mind, however, is particularly evident in social circumstances where the hold of tradition has become attenuated. A useful distinction can be drawn here between reflexivity, as a quality of human action as a whole, and *institutional reflexivity*, as an historical phenomenon. Institutional reflexivity refers to the institutionalization of an investigative and calculative attitude towards generalized conditions of system reproduction; it both stimulates and reflects a decline in traditional ways of doing things. It is also associated with the generation of power (understood as transformative capacity). The expansion of institutional reflexivity stands behind the proliferation of organizations in circumstances of modernity, including organizations of global scope.<sup>5</sup>

So far as point 2 goes, I should reaffirm that the duality of structure 'accounts for' nothing. It has explanatory value only when we consider real historical situations of some sort. The 'duality' of the duality of structure concerns the dependence of action and structure, taken as a logical assertion, but it certainly does not involve a merging of the situated actor with the collectivity. Much better here, indeed, to speak of a hierarchy rather than the sustaining of a dualism: there are many modes of interconnection between individuals and collectivities. It is

perfectly obvious that every situated actor faces an environment of action which has an 'objectivity' for him or her in a quasi-Durkheimian sense.

As for points 3 and 4, the distinction between micro- and macro-analysis is not a very useful one in social science, at least in some of the ways in which it is ordinarily understood. It is especially misleading if seen itself as a dualism – where 'micro-situations' are those to which a notion of agency is appropriate, whereas 'macro-situations' are those over which individuals have no control.<sup>6</sup> What is important is to consider the ties, as well as the disjunctions, between situations of co-presence and 'mediated connections' between individuals and collectivities of various types. It is just not the case that what Mouzelis calls 'macro action' is left aside in structuration theory. 'Macro action', however, for the reasons he gives, is not the same as lack of co-presence: here the phenomenon of differential power is usually central. A small number of individuals meeting together may enact policies that have very extensive consequences. Macro-action of this sort is even more pervasive than Mouzelis implies, because it is by no means limited to conscious processes of decision-making; large-scale systems of power are reproduced just as strongly in more routinized circumstances of co-present interaction.

As for point 5, social life, particularly in conditions of modernity, does involve multiple levels of collective activity. Far from being inconsistent with the views set out in *New Rules*, such an observation is entirely in line with them. 'Externality' and 'constraint' cannot be seen, as Durkheim thought, as general characteristics of 'social facts'. 'Constraint' takes several forms, some of which again concern the phenomenon of differential power. The 'externality' of social facts does not define them as social facts, but instead directs attention to various different properties/contexts/levels of the environments of action of situated individuals.

In structuration theory, the concept of 'structure' presumes that of 'system': it is only social systems or collectivities which have structural properties. Structure derives above all from regularized practices and is hence closely tied to institutionalization; structure gives *form* to totalizing influences in social life.



Is it then in the end misleading to try to illuminate the conception of the duality of structure by reference to language use? It is misleading, I think, if we see language as a closed and homogeneous entity. Rather, we should conceive of language as a fragmented and diverse array of practices, contexts and modes of collective organization. As I stress in the text, the idea of Lévi-Strauss, that 'society is like a language', should be resisted strongly; but the study of language certainly helps cast light upon some basic characteristics of social activity as a whole.

All this having been said, the critic may still feel worried or dissatisfied. For is there not a long distance between 'everyday practices', the situated interaction of individuals, and the properties of the large-scale, even global, social systems that influence so much of modern social life? How could the former in any way be the medium of the reproduction of the structural properties of the latter? One response to this question would be to say that, as a result of current globalizing trends, there actually *are* very important respects in which everyday activities connect to global outcomes and vice versa. In the global economy, for example, local purchasing decisions affect, and serve to constitute, economic orders which in turn act back upon subsequent decisions. The type of food a person eats is globally consequential in respect of global ecology. On a somewhat less encompassing level, the way in which a man looks at a woman may be a constituting element of deeply engrained structures of gender power. The reproduction/transformation of globalizing systems is implicated in a whole variety of day-to-day decisions and acts.

Deconstructing 'society', however, means recognizing the basic significance of diversity, context and history. Processes of empirical social reproduction intersect with one another in many different ways in relation to their time-space 'stretch', to the generation and distribution of power, and to institutional reflexivity. The proper locus for the study of social reproduction is in the immediate process of the constituting of interaction, for all social life is an active accomplishment; and every moment of social life bears the imprint of the totality. 'The totality', however, is not an inclusive, bounded 'society', but a composite of diverse totalizing orders and impulses.

Institutional reflexivity – this notion connects the analysis of

modernity with the more generalized idea of the double hermeneutic. The 'double' of the 'double hermeneutic' again implies a duality: the 'findings' of social science do not remain insulated from the 'subject-matter' to which they refer, but consistently re-enter and reshape it. It is of the first importance to emphasize that what is at issue here is not the existence of feed-back mechanisms. On the contrary, the intrusion of concepts and knowledge-claims back into the universe of events they were coined to describe produces an essential erraticism. The double hermeneutic is thus intrinsically involved in the dislocated, fragmenting nature of modernity as such, particularly in the phase of 'high modernity'.<sup>7</sup>

Many implications flow from this observation, but I shall consider the thesis of the double hermeneutic here only from the point of view of recent debates in the philosophy and sociology of science. Such debates have their origins in the by-now accepted observation that natural science has hermeneutic traits. As discussed in *New Rules*, the old differentiation between *Verstehen* and *Erklären* has become problematic; the idea that natural science deals only, or even primarily, in law-like generalization belongs to a view of scientific activity which has now largely become abandoned. As Karen Knorr-Cetina puts it, 'Natural science investigation is grounded in the same kind of situational logic and marked by the same kind of indexical reasoning which we used to associate with the symbolic and interactional character of the social world.'<sup>8</sup>

Such conclusions have been reached as a result of sociological studies of science rather than philosophical interpretation. Thus experimentation, long considered the bedrock of scientific knowledge, has been studied as a process of the translation and construction of contextual information. But is this a 'single hermeneutic' which can be differentiated from the double hermeneutic of natural science? Some, including Knorr-Cetina, claim not. This distinction, she says, depends upon two assumptions: that human beings possess 'causal agency' not found in nature; and that, in the social world, there is a distinctive means, conscious appropriation, whereby causal agency is triggered. Neither is justified. The first rests upon too unsophisticated a notion of natural causality, for objects in the natural world may

also be said to possess causal powers. The second ignores the fact that there are equivalent, if not directly parallel, triggering mechanisms for the reception of information in the world of nature.

Harbers and de Vries suggest that these conflicting views of the double hermeneutic can be looked at in the light of empirical evidence. Knorr-Cetina bases her thesis upon historical and sociological studies of natural science. Why not consider in a direct way the influence of social science within broader frameworks of knowledge and action? According to them, the thesis of the double hermeneutic presumes two hypotheses: that where the common-sense interpretations constituting social phenomena become the subject of historical change, interpretations offered within the social sciences will change correspondingly; and that novel concepts or findings developed within social science will have to be defended not only within the sociological community but in relation to a 'common-sense forum of lay individuals'. The notion of the double hermeneutic implies that, in contrast to the situation in natural science, sociologists have a 'scientific', rather than only civic, obligation to present their ideas to a lay audience.<sup>9</sup> Harbers and de Vries examine these hypotheses by looking at developments in education in The Netherlands.

Sociologists have long been involved in documenting unequal educational opportunities. Many projects were established in different countries from the 1950s onwards in order to uncover the factors influencing such inequalities. The Dutch Project on Talents was one of these, the work of a group of eleven social investigators. The idea of the research was to study the large reserve of 'unused talents' believed to exist. In other words, it was thought that many children from poorer backgrounds were qualified for advanced levels of secondary education, but were not to be found in the appropriate schools. The results did not conform to this expectation. Children attended schools which matched their abilities; the relative under-representation of children from underprivileged backgrounds was not because of misdirected decisions about type of school after primary education. The children had already lagged behind in primary school.

These conclusions were at first accepted by most educational

authorities and government policy became based on them. Subsequently, however, another researcher published a book using new calculations derived from the same data. Using a different concept of 'talent', he concluded that a reserve of unusual talents did indeed exist. The original Project on Talents had been carried out within a definite framework of assumptions corresponding to a popular view of what a 'meritocracy' is. The second researcher attacked those assumptions, and proposed not only a different view of educational equality but a different practical orientation to reducing it. His concepts and findings contributed to a dissolution of the 'meritocratic consensus' that previously existed. In turn, the sociology of education produced new definitions of research problems and became divided into a number of opposing perspectives. These then in their turn were filtered back into public debates on educational policy issues.

Harbers and de Vries suggest that their study provides a concrete example of the double hermeneutic: public attitudes on education were altered by, and helped alter, processes of social research. Where the 'theoretical style' of research work is consonant with widely held lay assumptions, they say, common-sense assumptions remain unnoticed by all parties. In such a situation, the sociological investigator can appear as an 'autonomous scientist', much like the natural scientist. Where a variety of dissident opinions exists among the lay public, however, claims about analyses of social phenomena have to be put forward and defended simultaneously in different forums. They conclude:

social scientists are dependent upon common sense thinking in a way which is strikingly different from the relation between common sense and scientific knowledge in the natural sciences. Whereas, of course, in the latter ideas, concepts, metaphors, etcetera may be adopted from non-scientific traditions and, hence, common sense thinking may serve as a resource, common sense interpretations set limits to the social sciences and constrain their cognitive development along the lines set out in the hypotheses we have formulated.<sup>10</sup>

The view of Harbers and de Vries has been criticized by William Lynch, who defends a view close to that of Knorr-Cetina.<sup>11</sup> The social and natural sciences are after all not so

different; but to see this we must concentrate more upon natural than social science. Thus an interaction between accounts of a subject-matter and 'responses' from that subject-matter occurs in the natural as well as the social world. In social life, actors' accounts are often, even normally, 'represented' – some people, who remain silent, are spoken for by others. The same happens in natural science where scientists or lay actors 'speak for' the natural world. Similarly, the causal order of natural reality is altered by accounts imposed on it. For the natural world is not an inert, pre-given object, but is itself 'constituted' by the accounts scientists and lay agents provide.

Consider the phenomenon of deductive-nomological laws in the natural sciences. Such laws, Lynch says, 'do not hold in the real world'. Rather, they depend upon elaborate interventions which scientists make into the natural order to establish the conditions under which such laws can be 'seen to hold'. To 'extend' such laws outside the laboratory usually implies constructing conditions in which law-like behaviour can be 'appropriately manifested'. Such laws 'depend for their applicability on closed conditions that never fully obtain and require intervention and manipulation for their demonstration'.<sup>12</sup>

If natural scientists are able to claim greater autonomy than their counterparts in the social sciences, this is largely because of the degree to which a culture favourable to scientific claims has developed in modern societies. A great deal of work has gone into ensuring that natural scientists are less accountable than their social-scientific colleagues for their epistemic choices. Focusing on a double hermeneutic only in social science, therefore, reinforces a well-established tendency to obscure the cognitive and practical impact which natural science has upon the lives of lay individuals. The double hermeneutic, as applied specifically to social science, proscribes 'empirical examination of past lay constraints on natural scientific development and, potentially, further interventions in issues for which the public might claim to have a stake'.<sup>13</sup>

To assess the validity of these ideas it is necessary to go over some of the ground covered in *New Rules* about the concept of the double hermeneutic – in respect not just of the meaning of 'double' but also of that of 'hermeneutic'. The idea of the double

hermeneutic is partly a logical and partly an empirical one. All social science is irretrievably hermeneutic in the sense that to be able to describe 'what someone is doing' in any given context means knowing what the agent or agents themselves know, and apply, in the constitution of their activities. It is being able (in principle) to 'go on' – mutual knowledge shared by participants and social-scientific observers. The hermeneutic element involved here does not have a parallel in natural science, which does not deal with knowledgeable agents in such a way – even in the case of most animal behaviour.

This is the logical side of the double hermeneutic. Lay actors are concept-bearing beings, whose concepts enter constitutively into what they do; the concepts of social science cannot be kept insulated from their potential appropriation and incorporation within everyday action. The empirical side concerns institutional reflexivity, a phenomenon which, as noted previously, becomes particularly pervasive with the maturation of the modern social order. The social sciences are deeply involved in the institutional reflexivity of modernity, although they far from exhaust it. As an empirical phenomenon, institutional reflexivity lends itself to research study, although in this regard certain provisos must be made. There is no way of standing wholly apart from reflexivity, since the social-scientific observer, by making her or his results public, relinquishes control over them. The ambition of blunting institutional reflexivity by means of preventing the self-fulfilling or self-negating prophecy, as *New Rules* makes clear, is a futile one; not because research cannot sometimes take account of them, but because they are seen as contaminations of the research process, rather than as intrinsic to the relation between social science and its 'subject-matter'.

Is there any virtue in the sort of study carried out by Harbers and de Vries? There is, I think, as a case-study of certain processes of institutional reflexivity; but fresh empirical research is not needed, in my view, to document that the double hermeneutic actually exists. Institutional reflexivity is so central to modernity that a myriad of examples of it could be offered. The double hermeneutic is much more complex, and less narrowly bounded, than Harbers and de Vries assume in their formulation. There is no necessary match between changing

common-sense interpretations of social phenomena and the ideas and theories of social science. Many different connections and oppositions between these are possible. The findings of social science do, in my view, have to be defended *vis-à-vis* those whose activities they cover, and others also; but this is primarily an ethical/political issue, because of the claim to 'know better' than lay agents themselves why things happen as they do.

These considerations do not resolve the question of whether there is a double hermeneutic in natural science. If such were the case, we should have a new version of the 'unity of the sciences', albeit one which differs greatly from the old naturalistic view. Since *New Rules* was written, constructivist and ethnomethodological accounts of natural science have developed apace and, apart perhaps from their more eccentric fringes, have contributed much to the emergence of a sophisticated sociological understanding of science. I do not believe, however, that they compromise the views set out here. The 'single hermeneutic' of science should not be equated with its autonomy in respect of lay beliefs and activities. Here we must insist upon the distinction between mutual knowledge and common sense. Scientific ideas may derive from common-sense beliefs and concepts, as well as place them in question. Sometimes such beliefs act as stimulants and at other times as constraints upon natural science investigations. The concepts and findings of the natural sciences do not remain separate from the social world, or from interventions, conceptual and technological, which human beings make into the world of nature. The hermeneutics of natural science, and the associated activities of the construction of investigatory procedures, are not confined to the interplay of technical meanings. Since Gödel, we know that even the most formal systems of mathematics presume 'outside' concepts, and obviously ordinary language is the medium by which scientific procedures and discussions are produced and carried on. It is certainly not true that the thesis of the double hermeneutic as specific to social science implies a prohibition upon interactions between science and lay culture.

The relation between the natural scientist and his or her field of investigation, however, is neither constituted nor mediated by mutual knowledge, in the way I have defined that term – unlike

the relations between scientists themselves or between them and the lay public. This is why the double hermeneutic has peculiar reference to the social sciences. It is not affected by the fact that, in respect of both natural and social science, some people speak on account of those who remain silent or inarticulate. Nor is the position affected by constructivism, even in its more radical guises. For no one suggests that it is the natural world which constructs accounts of itself.

One consequence of the double hermeneutic is that original ideas and findings in social science tend to 'disappear' to the degree to which they are incorporated within the familiar components of practical activities. This is one of the main reasons why social science does not have parallel 'technological' applications to natural science, and why it typically sustains less prestige in the public eye than the natural sciences do. For the most interesting and challenging ideas are precisely those most likely to be seized upon in lay domains – although, to emphasize the point again, with many differing possible outcomes. Superficially, modern civilization seems almost wholly dominated by natural science; the social sciences are very much the poor relations, which hardly get a look in. In reality, the impact of social science – understood in the widest possible way, as systematic and informed reflection upon the conditions of social activity – is of core significance to modern institutions, which are unthinkable without it.

In revising the text of the book, I have not sought to make major changes. Nor have I added any substantially new sections, but have limited myself to making stylistic alterations and eliminating one or two paragraphs referring to material that has now become excessively dated. I have taken out about half of the notes from the original edition, but have not tried to update those that remain; the bibliography from the first edition has also been omitted.

# Introduction to the First Edition

As we know them today, the social sciences were shaped by the spectacular advances of natural science and technology in the late eighteenth and the nineteenth centuries. I say this bluntly, in awareness of the complexities which it conceals. It would certainly not be true to say that the successes of human beings in seemingly mastering nature intellectually in science, and materially in technology, were adopted uncritically as forming a model for social thought. Throughout the nineteenth century, idealism in social philosophy and romanticism in literature, in their various guises, maintained their distance from the intellectual standpoints fostered by the natural sciences, and normally expressed deep hostility to the spread of machine technology. But for the most part, authors within these traditions were as sceptical of the possibility of creating a science of society as they were distrustful of the claims of the sciences of nature, and their views served as no more than a critical foil to the much more influential writings of those who sought to create just such a science. Mentioning just one or two figures in isolation is risky, but I think it reasonable to regard Comte and Marx as the pre-eminent influences upon the subsequent development of the social sciences (I shall use this term primarily to refer to sociology and anthropology, but shall also on occasion make reference to economics and to history). Comte's influence is fundamental since, as projected through Durkheim's writings, his conception of sociological

method can readily be traced through to some of the basic themes of 'academic sociology' and anthropology in the twentieth century. Following Marx's own scornful dismissal of Comte, Marxism set itself against those streams of social theory connected to the emphases of the former author. Comte's formulation of the idea of a natural science of society was actually a sophisticated one, as anyone can check for himself by glancing through no more than a few pages of the *Philosophie Positive*, even if it lacked the subtleties (and, it must be said, some of the logical difficulties) of Marx's work, informed as the latter was by a transposed Hegelian dialectic. Both Comte and Marx wrote in the shadow of the triumphs of natural science, and both regarded the extension of science to the study of human conduct in society as a direct outcome of the progressive march of human understanding towards humanity itself.

Comte sanctified this as a doctrine. The 'hierarchy of the sciences' expresses not only a logical order of relations but an historical one too. Human knowledge first of all dispels the shrouds of mysticism in those areas of nature furthest from human involvement and control, in which humanity appears to play no role as subject: first mathematics, and then astronomy. The development of science subsequently edges closer and closer to human life, moving through physics, chemistry and biology to the creation of sociology, the science of human conduct in society. It is easy to see how, even before Darwin, evolutionary theory in biology seemed to prepare the stage for the explication of human conduct according to principles of scientific reason, and to appreciate Marx's enthusiasm for the *Origin of Species* as offering a parallel to what he and Engels sought to accomplish in their work.

An end to mystery, and an end to mystification: this is what Comte and Marx alike anticipated and strove for. If nature could be revealed as a secular order, why should human social life remain enigmatic? For perhaps there is only a short step from scientific knowledge to technical mastery; with a precise scientific understanding of the conditions of their own social existence, why should not people be able rationally to shape their own destiny? The Marxian vision is ambiguous: and some versions of what Marx had to say, I believe, can be reconciled without

difficulty, on the level of ontology at least, with this present study. I refer to those versions of Marx which regard Marxism, not as a natural science of society which happened to predict the demise of capitalism and its replacement by socialism, but as an informed investigation into the historical interconnections of subjectivity and objectivity in human social existence. But in so far as there were strongly naturalistic strains in Marx's writings, and most certainly there were, Marx can be categorized along with Comte as previsaging, and seeking to bring into being, a science of society which would reproduce, in the study of human social life, the same kind of sensational illumination and explanatory power already yielded up by the sciences of nature. By this token, social science must surely be reckoned a failure. Beside the seeming certainties, the system of precise laws attained in classical mechanics, that model for all aspiring sciences after Newton, which in the nineteenth century was unquestioningly assumed to be the goal to be emulated, the achievements of the social sciences do not look impressive.

This much is accepted, and necessarily so, by those in the social sciences today who cling to the same sort of ideal. The wish to establish a natural science of society, which would possess the same sort of logical structure and pursue the same achievements as the sciences of nature, remains prominent. Of course, many who accept it have relinquished the belief, for various reasons, that social science, in the near future, will be able to match the precision or the explanatory scope of even the less advanced natural sciences. However, a sort of yearning for the arrival of a social-scientific Newton remains common enough, even if today there are perhaps many more who are sceptical of such a possibility than still cherish such a hope. But those who still wait for a Newton are not only waiting for a train that will not arrive, they are in the wrong station altogether.

It is of the first importance, of course, to trace out the process whereby the certainties of natural science itself have been assaulted in the twentieth century. This has to a large extent come about through the internal transformation of physics and the setting aside of Newton by Einsteinian relativity, complementarity theory and the 'uncertainty principle'. But of equal significance, to this study at least, is the appearance of new forms

of the philosophy of science. One might identify two intertwining yet ultimately opposed trends in the philosophy of science over the past forty or fifty years, in the wake of the perturbations experienced in classical physics. On the one side – and this is not at all paradoxical – there has been the attempt to sustain the claim that natural scientific knowledge, or a particular characterization of it, should be regarded as the exemplar of everything which can be regarded legitimately as 'knowledge'. If the famous 'verification principle' was itself rapidly shown to be incapable of verification, and the radical attempt to expunge metaphysics from human affairs was soon abandoned, the influence of logical positivism or logical empiricism remains strong, if not preponderant. In recent decades, this orthodoxy has been challenged with mounting success. In this challenge the works of Karl Popper played a pivotal, if not entirely unambiguous, role. Whatever Popper's original views may have been, his critique of inductive logic and his insistence that, though claims to knowledge in science have to begin somewhere, there is nowhere where they *have* to begin, were of decisive importance, not only for their own value, but as a springboard for many subsequent contributions.

Some such discussions in natural science have an immediate significance for epistemological problems in the social sciences. But in any case I want to assert that social science should move out of the shadow of the natural sciences, in whatever philosophical mantle the latter be clad. By this I do not mean to say that the logic and method of the study of human social conduct are wholly discrepant with those involved in the study of nature, which I certainly do not believe; nor do I propose to support the view expressed by those in the tradition of the *Geisteswissenschaften*, according to which any sort of generalizing social science is logically ruled out of court. But any approach to the social sciences which seeks to express their epistemology and ambitions as directly similar to those of the sciences of nature is condemned to failure in its own terms, and can only result in a limited understanding of human society.

The failure of social science, when thought of as a natural science of society, is manifest not only in the lack of an integrated corpus of abstract laws, whose circumstances of application

are precisely known, and which meet with the acceptance of a 'professional community'; it is evident in the response of the lay public. Conceived as a project by Comte and Marx, social science was to be revelatory, to sweep away the opaque prejudices of earlier times and replace them with rational self-understanding. What appears as the 'resistance' of the lay public to the 'findings' of social science is often simply equated with the opposition that has sometimes been provoked by theories of the natural world: for example, a disinclination to accept that the world is spherical rather than flat. But that sort of resistance is aroused by scientific theories or discoveries which shake or disturb common sense (I do not want to touch here upon the opposition of vested interests to scientific ideas). The objection which lay members of society frequently have to the claims of sociology is just the opposite: that its 'findings' tell them nothing which they did not already know – or worse, dress up in technical language that which is perfectly familiar in everyday terminology. There is a disinclination among those involved in the social sciences to take this sort of protest seriously: after all, haven't the natural sciences often shown that beliefs which people took for granted, which they 'knew', were in fact mistaken? Why should we not merely say that it is the task of social science to check upon common sense, to see whether lay members of society do really know what they claim to know? I want to suggest, however, that we have to take the objection seriously, even if in the end it is not sustained: for, in some sense that is not at all easy to spell out, society is the outcome of the consciously applied skills of human agents.

The difference between society and nature is that nature is not a human product, is not created by human action. While not made by any single person, society is created and recreated afresh, if not *ex nihilo*, by the participants in every social encounter. *The production of society* is a skilled performance, sustained and 'made to happen' by human beings. It is indeed only made possible because every (competent) member of society is a practical social theorist; in sustaining any sort of encounter he or she draws upon social knowledge and theories, normally in an unforced and routine way, and the use of these practical

resources is precisely the condition of the production of the encounter at all. Such resources (which I shall later call generically 'mutual knowledge') *as such* are not corrigible in the light of the theories of social scientists, but are routinely drawn upon by them in the course of any researches they may prosecute. That is to say, a grasp of the resources used by members of society to generate social interaction is a condition of the social scientist's understanding of their conduct in just the same way as it is for those members themselves. While this is easily appreciated by an anthropologist who visits an alien culture, and who seeks to describe the conduct observed there, it is not as transparent to anyone studying conduct within a familiar cultural frame, who tends to take such mutual knowledge for granted.

Recent developments in sociology, drawing in large part upon not so recent developments in analytic philosophy and phenomenology, have been very much concerned with these matters. That such an interchange between the social sciences and philosophy should have occurred is not surprising, since what distinguishes some of the leading standpoints within these broad philosophical traditions – namely 'existential phenomenology', 'ordinary language philosophy' and the philosophy of the later Wittgenstein – is a resurgent interest in action, meaning and convention in the context of human social life. Now a concern with problems of action is certainly not alien to existing orthodoxies in the social sciences. The term 'action' itself, in the shape of the 'action frame of reference' occupies a prime place in the work of Talcott Parsons. In his earlier writings at least, Parsons specifically sought to incorporate a 'voluntaristic' frame within his approach. But Parsons (like J. S. Mill) went on to identify voluntarism with the 'internalization of values' in personality and hence with psychological motivation ('need-dispositions'). *There is no action in Parsons's 'action frame of reference'*, only behaviour which is propelled by need-dispositions or role-expectations. The stage is set, but the actors only perform according to scripts which have already been written out for them. I shall try to trace out some further implications of this later on in this study. But is it any wonder that laypeople find it hard to recognize themselves in such theories? For although Parsons's writings are in these



respects vastly more sophisticated than those of many others, we do not appear in them as skilled and knowledgeable agents, as at least to some extent masters of our own fate.

The first part of this study consists of a brief and critical Cook's tour through some prominent schools of social thought and social philosophy. There are striking, and not very widely acknowledged, points of connection between, on the more abstract level of the philosophy of being, Heidegger and the later Wittgenstein and, so far as the social sciences are concerned, the lesser figures of Schutz and Winch. There is one very substantial difference between the latter two: Schutz's philosophy remained wedded to the standpoint of the ego, and hence to the notion that we can never achieve more than a fragmentary and imperfect knowledge of the other, whose consciousness must forever remain closed to us; while for Winch, following Wittgenstein, even our knowledge of ourselves is achieved through publicly accessible semantic categories. But both insist that, in formulating descriptions of social conduct, the observing social scientist does, and must, depend upon the typifications, in Schutz's term, used by members of society themselves to describe or account for their actions; and each, in his different way, underlines the significance of reflexivity or self-awareness in human conduct. Since what they have to say is in some respects not too dissimilar, it is not very surprising that their writings have much the same sort of limitations – limitations which I think are shared by many who have written about the 'philosophy of action', especially those, like Winch, influenced above all by the later Wittgenstein. 'Post-Wittgensteinian philosophy' plants us firmly in society, emphasizing both the multifold character of language and the way it is embedded in social practices. However, it also leaves us there. The rules governing a form of life are taken as a parameter, within and with reference to which modes of conduct may be 'deciphered' and described. But two things are left obscure: how one is to set about analysing the transformation of forms of life over time; and how the rules governing one form of life are to be connected to, or expressed in terms of, those governing other forms of life. As some of Winch's critics have pointed out (Gellner, Apel, Habermas), this easily terminates in a relativism which breaks off just where some of the basic issues

which confront sociology begin: problems of institutional change and the mediation of different cultures.

It is remarkable how frequently conceptions which at least in certain important respects parallel that of 'forms of life' (language-games) appear in schools of philosophy or social theory which have little or no direct connection to Wittgenstein's *Philosophical Investigations*: 'multiple realities' (James, Schutz), 'alternate realities' (Castaneda), 'language structures' (Whorf), 'problematics' (Bachelard, Althusser), 'paradigms' (Kuhn). There are, of course, very basic differences between the philosophical standpoints which these express, and the sorts of problems which their authors have developed to try to illuminate them. Each of them in some part signals a movement along a broad front in modern philosophy away from empiricism and logical atomism in the theory of meaning; but it is not difficult to see how the emphasis upon discrete 'universes of meaning' can allow the principle of *relativity* of meaning and experience to become *relativism* caught in a vicious logical circle, and unable to deal with problems of meaning-variance. I shall try to show in the course of this study how it is possible, and important, to sustain a principle of relativity while rejecting relativism. This depends upon escaping from the tendency of some if not most of the authors just mentioned to treat universes of meaning as 'self-contained' or unmediated. Just as knowledge of the self is, from the earliest experience of the infant, acquired through knowledge of others (as G. H. Mead showed), so the learning of a language-game, the participation in a form of life, occurs in the context of learning about other forms of life that are specifically rejected or are to be distinguished from it. This is surely compatible with Wittgenstein, whatever some of his followers may have made of his ideas: a single 'culture' incorporates many types of language-game on levels of practical activity, ritual, play and art; and to become acquainted with that culture, as a growing infant or as an alien observer or visitor, is to come to grasp the mediations of these in moving between languages of representation, instrumentality, symbolism etc. In quite different contexts, Schutz talks of the 'shock' of moving between different 'realities', and Kuhn refers to the apprehension of a new 'paradigm' as a sudden 'Gestalt switch'. But although such sudden



transitions no doubt occur, the ordinary member of society quite routinely shifts between different orders of language and activity, as do scientists on the level of theoretical reflection.

Parsons argued that the most significant convergent idea in modern social thought concerns the 'internalization of values', as independently arrived at by Durkheim and Freud; I think a better case can be made for the notion of the social (and *linguistic*) foundation of reflexivity, as independently arrived at, from widely varying perspectives, by Mead, Wittgenstein and Heidegger – and, following the latter, Gadamer. Self-consciousness has always been regarded, in positivistically inclined schools of social theory, as a nuisance to be minimized; these schools endeavour to substitute external observation for 'introspection'. The specific 'unreliability' of the 'interpretation of consciousness', indeed, whether by the self or by an observer, has always been the principal rationale for the rejection of *Verstehen* by such schools. The intuitive or empathic grasp of consciousness is regarded by them merely as a possible source of *hypotheses* about human conduct (a view which is echoed even in Weber). In the tradition of the *Geisteswissenschaften* in the nineteenth and early twentieth centuries, *Verstehen* was regarded above all as a *method*, a means of studying human activity, and as such as depending upon the 'reliving' or 're-enactment' of the experiences of others. Such a view, as held by Dilthey and later in modified form by Weber, was certainly vulnerable to the strictures levelled against it by positivistic opponents, since both Dilthey and Weber, in their varying ways, wanted to claim that the 'method of understanding' yields material of an 'objective', and therefore intersubjectively verifiable, kind. But what these writers called 'understanding' is not merely a method for making sense of what others do, nor does it require an emphatic grasp of their consciousness in some mysterious or obscure fashion: *it is the very ontological condition of human life in society as such*. This is the central insight of Wittgenstein and of certain versions of existentialist phenomenology; self-understanding is connected integrally to the understanding of others. Intentionality, in the phenomenological sense, is not thus to be treated as an expression of an ineffable inner world of private mental experiences, but as *necessarily*

drawing upon the communicative categories of language, which in turn presuppose definite forms of life. Understanding what one does is only made possible by understanding, that is, being able to describe, what others do, and vice versa. It is a semantic matter, rather than a matter of empathy; and reflexivity, as the distinctive property of the human species, is intimately and integrally dependent upon the social character of language.

Language is first of all a symbolic or sign-system; but it is not simply, or even primarily, a structure of 'potential descriptions' – it is a medium of practical social activity. The organization of 'accountability', as has been made fully clear in existentialist phenomenology after Heidegger, is the fundamental condition of social life; the production of 'sense' in communicative acts is, like the production of society which it underpins, a skilled accomplishment of actors – an accomplishment that is taken for granted, yet is only achieved because it is never wholly taken for granted. Meaning in communicative acts, as it is produced by lay actors, cannot be grasped simply in terms of a lexicon, any more than it can be transcribed within frameworks of formal logic that pay no attention to context-dependence. This is surely one of the ironies of some sorts of supposedly precise 'measures' employed in the social sciences, quite properly resented by the lay public since the categories often appear foreign and imposed.

In this study, I discuss several schools of thought in social theory and social philosophy, from the phenomenology of Schutz to recent developments in hermeneutic philosophy and critical theory. I shall try to make it clear what, if anything, I have borrowed from each of these schools, and shall attempt to indicate some of their shortcomings. This essay is not, however, intended to be a work of synthesis, and while I shall specifically draw attention to several parallel currents in social thought in the contemporary period, it is not my objective to seek to show an immanent process of convergence which will finally establish a secure logical framework for sociology. There are some standpoints in contemporary social thought which I have not analysed in a detailed way, even though much of what I have to say bears directly upon them. I have in mind *functionalism*, *structuralism* and *symbolic interactionism* – labels for an array of views which are diverse, to be sure, but each of which possesses