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EVOLUTION AND SOCIETY

A STUDY IN VICTORIAN
SOCIAL THEORY

BY

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of Sussex*



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TO MY FATHER AND MOTHER

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PREFACE

This book attempts to investigate a phase of Victorian social thought which has an intrinsic interest as part of the history of ideas in nineteenth-century England. It would be gratifying if it should also be regarded by social scientists as a contribution to the history of their disciplines, but it is almost certainly not the kind of contribution a social scientist himself would be likely to make. The chief object has been to understand how and why theories of social evolution in Victorian England emerged and took shape as they did; it has not been to arrange the Victorian social theorists in an order of merit according to the extent to which they seem to have anticipated the methods and concepts of modern sociology and anthropology.

Nevertheless, the subsequent revolution in the methods and assumptions of the social sciences which underlies the vast differences between the preoccupations of the modern social scientist and those of his Victorian predecessors has led inevitably to a reinterpretation of the earlier revolution with which this book is primarily concerned. Its repudiation by modern British sociologists and anthropologists is a fact about evolutionary social theory of which any attempt to understand it and put it in historical perspective must take account. Questions—even historical questions—can now be asked about Victorian social theory which the Victorians themselves could not, or would not, have asked, because the concepts in terms of which they have to be put had not been made explicit, or did not seem important.

The most obvious of these questions is, why did so many Victorians conceive the major task of social science to be the study of social evolution? It is an obvious question because

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it is in this respect that the approach of modern social scientists is most at variance with that of the Victorians, and an attempt is made in this book to answer it, and to explain the hold that the idea of social evolution had upon the minds of Victorian intellectuals.

In making use, up to a point, of the more familiar concepts of the modern social sciences to ask historical questions about Victorian social theory, two difficulties in particular have to be faced.

First, there is the obvious danger of displaying *naïveté* in a field of study other than the author's own. Social scientists may find irritating the airy use here of terms such as 'functionalism', 'social system' and the like which they have either abandoned or are engaged in reconsidering and refining. An attempt has been made to keep the use of such terms to a minimum, using them where they seemed essential for brevity or precision, but not attempting to break them down by introducing distinctions not strictly necessary at this level of analysis. If the result seems to some crude and irritating this is perhaps in the nature of the case, quite apart from whatever avoidable errors and *gaucheries* there may be.

The second difficulty is more serious; as an historian, it has not been my intention to pass judgement on Victorian theories of society from the standpoint of the modern social scientist, nor am I qualified to do so. On the other hand, to employ some of the concepts and distinctions suggested by the subsequent development of the social sciences in analysing Victorian social theories often involves appearing to make an evaluation. To ask why someone did *not* think in a particular way may be taken to carry the implication that he ought to have done so.

To this again, there appeared no acceptable alternative

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and no attempt has been made to write in an absolutely neutral manner. This probably does not matter, provided it is remembered that such evaluations as there may be do not form part of the chief object in writing the book, which has been to understand and explain. The questions asked and the analyses undertaken have this as their ultimate object, and are not introduced primarily to convict the Victorians of sociological unsophistication. This is not an attempt to avoid responsibility for the evaluations made—obviously responsibility for them must be accepted, whatever the purpose in making them. But a mere attack on the Victorian social theorists would be undesirable for many reasons: it would be unnecessary; it would not be very interesting; the present writer is not particularly qualified to undertake it; and it would be historically irrelevant.

It remains true, however, that the legacy of the evolutionary anthropologists and sociologists of the nineteenth century has been largely repudiated by their twentieth-century successors, particularly in Britain. Contemporary social anthropologists do not regard it as their task to collect information about primitive peoples with a view to reconstructing the prehistory of civilization, nor do sociologists try to elicit laws of social evolution; indeed, one of the accusations made against certain influential contemporary sociological theories is that they cannot account for social change.

The repudiation of the evolutionary tradition by modern sociology and anthropology has made a reappraisal of the historical significance of that tradition inevitable. This reappraisal does not, of course, necessarily involve a rejection of the evolutionists' claim to have replaced theories of society deduced from psychological assumptions by the study—however heavily charged with preconceptions—of actual

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social phenomena in a systematic way. Modern anthropology goes back to Malinowski and Radcliffe-Brown, Durkheim and Weber, but it also, in a sense and more remotely, goes back to the evolutionists and polygenists, the travellers and cranks, who founded the Anthropological Society of London in 1863, 'with the object of promoting the study of Anthropology in a strictly scientific manner'. To some extent, the link is purely verbal, but there is also a genuine though loose connection, in their insistence on anthropology as an empirical and systematic subject, taking the world for its province and regarding nothing as too strange, remote, irrational or disgusting to be of potential importance. There was a notable change in the methods and intentions of social theory in the third quarter of the nineteenth century.

The rejection of evolutionism, however, has inevitably led to a reassessment of its historical importance. This reassessment, most elaborately expounded by Talcott Parsons, in *The Structure of Social Action*, depreciates the importance of the revolution of the mid-nineteenth century by pointing to a persisting framework of ideas—positivism—within which it took place, positivism being defined by Parsons as the belief that 'positive science constitutes man's sole possible significant cognitive relation to external reality'.

It may be asked, why bother with evolutionary positivism? Who now, as Talcott Parsons pointedly asks at the beginning of *The Structure of Social Action*, reads Spencer? And if the answer is no one, is that not as it should be? There are a number of answers to this. The history of ideas is not necessarily, like the old school history books, a record of victories. Moreover, there *was* a revolution in the mid-nineteenth century which requires explanation and which has not, apart from the hopeful use of 'Darwin' as a magic password, really received it.

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But there is also an answer more directly related to the reappraisal mentioned above which bears upon the question of what Lord Annan has called 'the curious strength of positivism in English thought'. The study of evolutionary positivism may not show why it did not disintegrate ten, twenty or even thirty years earlier, but it should help to explain how evolutionism became so strongly entrenched as the prevailing orthodoxy, and hence so hard to uproot. Even today, among educated laymen, a vague kind of evolutionary positivism often seems to play a considerable part in their unstated assumptions about the social sciences, while Weber and Durkheim are little more than names. To show how it became so strongly established would be to find out something not only about the intellectual needs and preoccupations of the Victorians but about the history of social theory.

It will be argued in this book that the seeds of modern sociological theory were to a considerable extent implicit in the doctrines which were becoming current in the eighteenth-sixties, but they were stifled by the overriding needs of an evolutionism which provided more satisfactorily what the Victorians sought in theories of society.

The Introduction (chapter 1) and chapter 2 deal in a fairly summary way with the prehistory of social evolutionism. This seemed necessary because there is no single work to which the reader can confidently be referred for such a prehistory. Chapter 2, in particular, deals with the question of the relations between English utilitarianism and embryonic social evolutionism. Parts of this story are familiar enough; others perhaps less so.

Evolutionary social theory proper is considered primarily with reference to three outstanding figures: Sir Henry Maine, Herbert Spencer and E. B. Tylor. There are good reasons for the choice of these three in particular. Maine

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was the first, and in many ways the most thorough, student of institutions by the new methods. Spencer, as well as making contributions to social theory which still arouse the interest of contemporary sociologists, was the philosopher of the whole movement, ambitiously relating his social theories to a total theory of existence. His name, more than any other, symbolized evolutionary social theory for the ordinary reader. Tylor was the chief channel through which social anthropology passed from being the concern of a few eccentrics in the early eighteen-sixties to acceptance as a profession and an academic discipline. There are other candidates for consideration—most notably J. F. McLennan and Sir John Lubbock, who are discussed in less detail. Any selection must be to some extent arbitrary, but the selection of these three—Maine, Spencer and Tylor—seems more defensible than any other.

A word must be said about the treatment of continental theorists. Evolutionary social theory was not an isolated, purely English phenomenon. It was, for a while, the orthodox social theory of most of the countries of Europe, and also of the United States. Where continental theories appear to have been relevant to the development of evolutionary social theory as a method, as in the case of Comte and Savigny, these have been discussed at some length. Where they seem to have contributed concepts and methods merely within an already established tradition of inquiry—the tradition of evolutionary social theory—they have been ignored, partly from lack of competence to deal with them, partly because their contribution is of interest only at a level of technical complexity which this book does not aspire to reach. The reader will therefore find no estimate of the contribution of such continental anthropologists as Wundt, Klemm and Waitz.

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To the establishment of evolutionary social theory, however, continental influences were profoundly important. Evolutionary social theory in England is seen, in the following pages, very largely as the outcome of a tension between English positivistic attitudes to science on the one hand and, on the other, a more profound reading of history, coming to a large extent from German romanticism, which made the older form of positivist social theory, philosophic radicalism, seem inadequate. The tension between the romantic-historical and the positivist approaches to society could only be reconciled, for the Victorians, by some theory of social evolution. This book is an attempt to understand why, and so to explain the hold which theories of social evolution had over so many Victorian intellectuals.

I wish to thank the Master and Fellows of Christ's College, Cambridge, for electing me into the Research Fellowship during the tenure of which most of the research for this book was done. I am also grateful to the Twenty-Seven Foundation for a grant to cover the costs of preparing the manuscript.

I wish to thank the Editor of *Victorian Studies* for permission to publish part of Chapter 4 which first appeared as 'Evolution and Anthropology in the 1860s: The Anthropological Society of London, 1863-71' in *Victorian Studies*, vii, December 1963.

In writing this book I have become indebted to too many colleagues to mention them all by name; I hope that they will accept this expression of my gratitude. A few of these debts, however, call for specific mention. Dr G. Kitson Clark, who supervised the book's progress from its inception, allowed me to benefit from many helpful suggestions and from his intimate knowledge of nineteenth-century England. Without his help at crucial stages the book would

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never have been completed. The criticisms and encouragement of Professor Meyer Fortes have done much to make me feel rather less gauche and uneasy in my trespasses into anthropology. To Professor Graham Hough and Mr Quentin Skinner I am indebted not only for reading the manuscript and making helpful criticisms and suggestions but for many stimulating conversations. When I attempt to estimate how different my career would have been without the generosity of Professor J. H. Plumb, which only the many others who have experienced it will be able to appreciate, I am defeated by the extent of my indebtedness; of one thing I am certain: it would not have included writing this book. To these and many others this book owes much; I alone am responsible for its errors and inadequacies. I must also thank Mrs Hazel Treleven, who typed the manuscript, and my wife for her criticisms and her forbearance.

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PREFACE TO PAPERBACK
IMPRESSION

I should first of all like to thank those critics and correspondents who have kindly sent me factual corrections. Unfortunately I cannot acknowledge each of these here separately, but I do want to take this opportunity of considering some more fundamental objections to the overall strategy of *Evolution and Society*. For an author to offer second thoughts after only three years may perhaps raise suspicions of light-mindedness. However, the gestation of a book is a lengthy process and it is only exceptionally that one can think equally intensively about all its parts at once; much in the pre-history of *Evolution and Society* prior to its publication in 1966 is, I find it salutary to recall, now a decade old. In particular I have been helped in reappraising it by the work and criticisms of Professor George Stocking.¹

Essentially I think that the criticisms I would now accept are those which show that I have not always been sufficiently rigorous in applying the methodology I professed; the latter I continue to think sound, at least in essentials. I should like, therefore, both to consider and briefly acknowledge some of these deficiencies in execution and confusions in strategy and also to reaffirm and slightly amplify the theoretical allegiance of the book.

There is first of all, however, an objection which, though in a sense one of detail, does deserve to be mentioned here since it bears directly on one of the chief claims of the book to offer an historical explanation. In a number of places,

¹ G. W. Stocking Jnr., *Race, Culture and Evolution* (Free Press, New York, 1968).

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developing historically a point originally made by Professor Talcott Parsons, I attempted a general historical explanation of social evolutionism by pointing to the fact that a wider sociological knowledge raised the spectre of relativism. I still think that this is an important point and a vital clue. I now think, however, that in some particular cases the point is overstressed or at least oversimplified. There seems to be virtually no direct evidence, for example, that Tylor was bothered by relativism. What he was bothered by, as Professor Stocking has pointed out to me, was polygenism. This, in a sense, is relativism in another form, but I think I insufficiently stressed the different forms it could take. In the case of Maine, I recognised the problem, but in an unsatisfactory way. On p. 155 I said 'This is not meant to imply that, psychologically speaking Maine felt in any danger of falling into relativism but merely that this was the logical function of his method'. Indeed it was, but I am not now very happy about the status of this kind of 'functionalism' in intellectual history.

My view of the overall strategy adopted in *Evolution and Society* is, as I have said, still essentially that put forward in the original preface. What is at issue, and where I am inclined to give ground, is the adequacy with which this strategy is carried out, which is related to the degree of clarity with which I conceived it. The fundamental point was that it was not a history of British 'social anthropology' in the Victorian period. This was partly due to a necessary modesty. To write such a history would be to provide a good deal more in various directions than *Evolution and Society* even pretends to offer. It was also, however, intended as a warning that a good deal in the way of historical explanation is likely to be missed if we approach the subject with preconceptions conditioned by a phrase like

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‘social anthropology’. In particular it seemed profitable to regard the Victorians who are usually picked out, for good reasons, as pioneers of social anthropology, as being concerned to a considerable extent with the same problems as, for example, the utilitarians of the early nineteenth century, who are not often considered in this context. To this extent *Evolution and Society* was conceived quite deliberately as a repudiation of the Whiggish way of looking at the history of the social sciences, which seemed to distort the understanding of the past by insisting on distinctions, for example between political theory and sociology, which have less application to the nineteenth century than they have had since (see p. 17).

So far so good. But in the preface I made another point which I also continue to adhere to, namely that the subsequent development of the social sciences has thrown into relief certain conceptual and methodological issues which can be, as it were, reflected back upon the past and used to ask genuinely historical questions. These two assertions seem *prima facie* incompatible. I believe, and shall try later to show, that they are not. What is undoubtedly true, however, is that to attempt to maintain both raises grave dangers of inconsistency in the actual execution of a work, and these I think I did not always avoid.

Most seriously, a critic might well ask why I selected the figures I did for special treatment and virtually ignored others. My dismissal of the Social Darwinists. (e.g. p. 115) —which I now feel sure was unjustifiably brusque, especially in the case of Bagehot—seems fishy, and a critic might object, though so far as I am aware no one has, that it runs the danger of making my denial that Darwin was a major influencing on evolutionist sociology seem only trivially true, by arbitrarily excluding from it those writers who were

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evidently influenced by him. I do not think matters are quite so bad as this, but it does seem in retrospect that my reasons for neglecting Social Darwinism look suspiciously Whiggish. Other matters seem unduly neglected for the same reason, and their exclusion suggests a certain cloudiness of aims. For in disclaiming both the scope and the possibly anachronistic limitations of a ‘history of Victorian anthropology’, I necessarily raise the question: if that is not the subject of the book, what is? The obvious short answer would be ‘Victorian social evolutionism’. The exclusion of Bagehot and Leslie Stephen, or, somewhat earlier, of authors like Bray, Hennell and R. W. Mackay, then becomes indefensible. Again, if one is going to discuss evolutionist debates of the eighteen sixties, as distinct from picking out figures the twentieth century happens to find interesting, then debates between monogenists and polygenists, and between the evolutionist and degenerationist accounts of savagery should certainly bulk larger than they do in *Evolution and Society*.¹

For the latter omissions, however, I think a slightly more respectable explanation can be offered than for the exclusion of the Social Darwinists. I concentrated not merely on social evolutionism but also—with the regrettable omissions of Stephen and Bagehot—specifically on attempts to construct a social *science*. This still seems to me legitimate as an attempt at a scholarly division of labour, though the lines of demarcation are admittedly very hard to draw and are not in fact drawn at all clearly. Nevertheless, the attempt, even if its difficulties were not at all fully understood, did make easier what does still seem to me a virtue of the book—the

¹ I do plead in some mitigation the slightly fuller treatment given to these matters in my article “The Uses of Philology in Victorian England” in R. Robson (ed) *Ideas and Institutions of Victorian Britain. Essays in honour of G. Kitson Clark* (London, 1967).

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attempt to link social evolutionism with Philosophic Radicalism, considered as an earlier version of a 'science of social relations'. This in turn, however, does have its own kind of limitation of vision. In other words there seems to have been both a profit and loss account in my use of the Philosophic Radicals; it got me out of the limited context of 'histories of anthropology' only to suggest other limitations of its own.

It is time to return, however, to the main theoretical issue, and here I remain unrepentant. I still think, that is, that my original belief that the subsequent development of the social sciences suggests (legitimate) ways of looking at Victorian social evolutionism and providing valid historical explanations of it, is compatible in principle, however difficult to combine in practice, with the anti-Whiggish principles which I also asserted. It is true that there is a good deal that I now find tiresomely cumbersome and nagging about the constant contrasting of Spencer, Maine and Tylor with more recent sociology and social anthropology, but that is a complaint about execution, not an objection in principle. Nevertheless, the principle itself may not seem immediately acceptable. I recognized the difficulty when I pointed out (p. x) that 'To ask why someone did *not* think in a particular way may be taken to carry the implication that he ought to have done so'. What can one say about this? One can, of course, admit the reproach, but claim it as a necessary evil. We do expect people to think as we do, especially when their thinking is served up to us under labels—which may well be necessary to avoid intolerable circumlocutions—like 'social anthropology', 'sociology', 'science', etc. To make it clear that this was not the case, when it was not, is an indispensable classificatory activity, a necessary preliminary to an historical explanation. Anachronistic questions are

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preferable to anachronistic assumptions. It is a vital part of this defence, of course, that the ideas with which those of the past are to be contrasted are those most likely to be current among the historian's readers.

Again, so far so good. But why should we then use these particular differences in asking for an historical *explanation*? We may properly ask why we are not like our ancestors; there seems something odder about asking why our ancestors were not like us. Can we properly ask why their ideas were not ours? Why *ours*? To ask why something is as it is, is a legitimate though imprecise question. To ask whether something is or is not like something else is also a proper question. But to ask *why* it is not presupposes that there might be some reason for supposing that it might be. There seems no *prima facie* reason for supposing that everyone, or indeed anyone, in the past might have been like us. This defence therefore will not do.¹

At this point one can fall back on another defence. This is to say that we may find that doing what *we* find it natural to expect was already, so to speak, 'on the cards'. This is what I tried to show on p. 83ff. If it is conceded that the study of the interrelation of institutions was, conceptually speaking, a 'real' option in mid-nineteenth century England, then the question why it was to such an extent sacrificed to the construction of evolutionary sequences becomes a proper and obvious historical question. This is one defence, but I am inclined to press the argument further. For I suspect that even without such evidence I should have been tempted to adopt the same strategy and I am not yet persuaded that I should have been wrong to do so.

¹ I am heavily indebted at this point to Quentin Skinner. "Meaning and Understanding in the History of Ideas". *History and Theory* VIII. No. 1, 1969.

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It still seems to me, for example, an important observation about Victorian social evolutionism that it was ‘both relativist and not relativist’ (p. 263). It is important both as a classification for us and as part of an explanation of what social evolutionism could do for Victorian intellectuals and hence of their possible motives in adopting it. Whether these were, or were relevant to, their actual motives then becomes a question to be determined by research. But, though Spencer, at least, recognized the point, it seems highly unlikely that the conceptual terms within which this research would be carried out would be arrived at simply by trying to see social evolutionism as its Victorian practitioners saw it. One needs also to be able to see it against a model of a social theory deliberately intended to be relativist: Malinowskian functionalism, for example. But the usefulness of the comparison is not affected one way or the other by whether social anthropologists now follow Malinowski, or even by whether a completely neutral social theory is possible. Any genuine conceptual distinction, once made, may be applicable to past debates, and hence may allow us to ask historical questions which we could not have asked otherwise, because it enriches our sense of the possibilities of a situation. The intellectual historian should not allow today’s conceptual maps to obliterate or to distort those of the past, which it is his job to recover and explain, but he may quite properly allow the former to enrich his sense of the potentialities of the latter. The case with conceptual distinctions seems in this respect much like that of metaphors and explanatory models generally. They can be, and often are, inappropriately applied, but this is a matter of empirical tests; no limits at all can be set *a priori* (except tautologically) to their potential historical usefulness. They are neither automatically validated nor automatically

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disqualified if they happen to be currently fashionable among ourselves, nor are they automatically discredited if they are not.

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ABBREVIATIONS

The following abbreviations have been used:

PERIODICALS AND JOURNALS

- AR* *Anthropological Review.*
ER *Edinburgh Review.*
JAS *Journal of the Anthropological Society of London.*
JES *Journal of the Ethnological Society of London.*
MAS *Memoirs Read before the Anthropological Society of
London.*
TES *Transactions of the Ethnological Society of London.*

WORKS OF HERBERT SPENCER

- P of B* *The Principles of Biology.*
P of E *The Principles of Ethics.*
P of P *The Principles of Psychology.*
P of S *The Principles of Sociology.*
S of S *The Study of Sociology.*
SS *Social Statics.*

The place of publication of all works cited is London unless otherwise stated. The customary abbreviations O.U.P. and C.U.P. have been used to denote the Oxford and Cambridge Presses. Where an edition other than the first has been used and the date of the first edition is of some importance it has been placed in brackets after the citation.