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Foreword

by Mary Midgley

It is surprisingly hard to discuss human evolution fairly and without rancour. Time has not done as much for us here as we might have hoped. The issues which Darwin laid open – but certainly did not invent – still, even today, seem to many people intolerably threatening. Can we find unbiassed ground from which to approach them? Looking back, we can now see much that was wrong about the tribal approach of Darwin's contemporaries. Some treated their religion as a science, others treated their science as a religion. Many who accepted Darwin joyfully did not understand him at all. Everyone tended to find what they were already looking for. Today the tribal divisions have shifted: different groups, different ideals, different hatreds are involved. But the animus is scarcely less, and – what is really surprising – the name of Darwin is still in use as a tribal rallying-cry. Can it still be right to treat him as a war-leader, for or against whom we should take sides? What is to be done about all this?

Georg Breuer's book is a solid attempt to do the first thing needful – to keep one's head and one's temper, get rid of irrelevancies and sort out the different issues involved in the current form of the dispute. His subject is the recent vigorous attempt to apply Darwinist principles to mankind, which is known as Sociobiology. This movement has been met with a small tornado of controversy in America which, on the usual principles of intellectual weather, is now reaching Europe. On those same principles, the tornado has picked up a surface layer of controversial grime which obscures the original complexity of the issue. Breuer begins by putting this aside and expounding the theory itself, clearly and sympathetically, for non-specialists. He then considers – also with great sympathy – the

general moral, political and psychological objections that have been brought against it. He accepts some of them, but suggests that others are excessive or misplaced. He also brings some very interesting criticisms of his own. On the whole he concludes that sociobiological thinking can indeed be useful, and is certainly not illicit. It is, however, a tool with a much more limited use than its champions have sometimes suggested.

This conclusion is, of course, not very surprising; it is the sort of verdict that one might expect to reach when moral and political charges are being brought against what seems, on the face of it, to be a branch of physical science. But it is not so easy to get the details right, particularly on a matter like this on which all of us are more or less engaged. Breuer thinks – I am sure rightly – that the tone or what some of its critics call the ‘atmosphere’ of sociobiology has been faulty as well as its excessive claims, and is involved in those claims. By over-simplifying human affairs, by constant brashness of language and occasional brutality of examples, sociobiologists have sometimes allowed the admission of hereditary elements in human behaviour to look like the acceptance of moral and emotional meanness. There has too, at times, been a triumphantly reductive tone, even a defiant quasi-religious exaltation of dubious entities such as the ‘selfish gene’. This sort of fault can be a serious nuisance, even when it does not really form part of the theory that it deforms, because it can discredit scientific work which is itself harmless and, if harmless, then useful.

That it might be useful is a dominant consideration with Breuer, and in that he is surely right. We do not have so full and satisfactory an understanding of human conduct that we can afford to throw away unexamined what might be a useful tool for it, just because we do not like the wrapping or the manners of the postman who delivers it. It is essential to see how deep the damage really goes. Breuer seems to me to do this very well, carefully distinguishing first what sociobiologists actually say from what they are merely suspected of meaning or implying, and then sorting out, within what they do say, the central theses from more optional and discardable elements. In its wide and minimal definition, Sociobiology sounds inoffensive enough. Its best-known exponent defines it as simply ‘the systematic study of the biological basis of all social behaviour’ (E. O. Wilson, *Sociobiology* p. 3–4). To apply this to man, however, is to accept that

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human behaviour has such a biological basis. And even this apparently modest thesis has been widely resisted on political grounds. Left-wing critics have denounced it as illicitly deterministic, as implying an immoral acceptance of the status quo, and as amounting to racism and fascism. Breuer takes this argument apart link by link, and succeeds, I think, in making it clear that the alarm is unfounded, and is inconsistent with any intelligible left-wing position. He rightly points out how much this denunciation of physical determinism, and in particular of the biological sciences, would have surprised Marx and Engels. Determinism is the tool of every scientist. The difficult task of reconciling it with free-will has not been noticeable more neglected in the biological than in the social sciences. About racism, Breuer speaks from the informed position of an Austrian who experienced himself the intellectual climate of the pre-Nazi time and Hitler's advent to power, and who had to leave his country after its annexation by Germany to avoid political and racial persecution. In general, as he justly remarks, it is never relevant to cite the *misuses* that have been made of doctrines as evidence against them. The large question of whether human behaviour has some genetic causes is a factual one, to be settled by the evidence. Moral questions about what we should then do about it are separate.

A total absence of genetic causes is in fact so hard to argue for, or even conceive, that thoughtful opponents in the social sciences usually prefer now to say, more subtly, that these causes do indeed exist, for man as for other animals, but that in man nothing can be known about them. Human life, then, can be studied only by the social sciences; it is entirely opaque to biology. Thus a rather precarious truce was for some time maintained, by which social scientists agreed not to deny the reality of human evolution, so long as nobody attempted to make any intellectual use of it. That truce was radically disturbed by the far-reaching ethological discoveries of Konrad Lorenz and Niko Tinbergen, which showed parallels between animal and human behaviour much more profound and important than had previously been suspected. Many social scientists have welcomed these insights and have used them admirably, showing that co-operation with biology is entirely possible – as indeed one might expect. Others, however, closed ranks and denounced the whole development. To them, sociobiology comes as a deeper shock. It renders the truce so far maintained even more

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awkward and unconvincing, because it is carried on in terms far more familiar to the social sciences (particularly in America) and has been so promoted as to catch the attention of a wide scientific public. Yet it seems to contain much that is alarming.

There are two ways out of this predicament. One is to retreat into a general scepticism about evolution itself. To some extent this can indeed be done. Apart from the pathological phenomenon of 'creationism', there seem to be good scientific reasons for thinking that 'neo-Darwinists' (or Haldanists, as they should perhaps be called) have indeed been far too confident in recent years about our knowledge of the actual mechanisms of selection. But this is a minor matter. However little we may know about these mechanisms, selection itself is not in doubt, and would not be even if some mechanism for the inheritance of acquired characteristics did eventually turn up as well. (And the discovery of such a mechanism would of course make biological issues even more central to the discussion.) There is therefore really no choice but to take the other way and examine the issues which sociobiology lays before us properly, one at a time and without prejudice. If this is done, it should be possible to spot fairly quickly what are its actual strengths and faults in method. Breuer is an admirable guide here, and particularly in the shrewd attention which he gives to the topic of what sociobiologists do *not* say. Their fascination with small, particular behaviour-patterns makes them, as he remarks, singularly blind to certain large and crucial emotional patterns that are characteristically human. But because these *are* characteristically human, there should, one would suppose, be nothing objectionable about admitting that the capacity for them is a part of our genetically determined human constitution. On this topic, as on many hereabouts, it is a mistake to take sides too quickly. Breuer sets an excellent example in avoiding the tribal feuds which have dogged this controversy, and in treating this most difficult issue fairly. Yet he has zest and enthusiasm enough to carry through it even those who are inclined to groan at the very name of sociobiology. Because he is not a specialist, he can get the issues in proportion, and so convey the real fascination of the topic. This is a book to learn from, and also one to enjoy.

Sociobiology – between the Two Cultures

If man is a descendant of animal ancestors, human behaviour must have evolved from animal behaviour. Of course, this proposition should not be taken as a basis for far-reaching speculations. Man, after all, is unique on earth and basically different from even those species which biologically are most closely related to him. Yet in spite of all these differences our descent cannot be denied. Biochemically we are more similar to chimpanzees than a dog is to a fox. Every one of us has certain bones, tendons and other organs that have no recognizable function in the human organism. Their existence can only be explained if they are considered as relics of organs that served a useful purpose in our animal ancestors a long time ago. It is therefore not absurd to assume that there might be similar relics in human behaviour as well.

Karl Marx thought of dedicating to Charles Darwin his main work, *Das Capital*. Darwin did not like the idea; apparently this was because he did not want his scientific work to be rubber-stamped politically. Nevertheless the descent of man was soon hotly debated – not only by biologists but even more in philosophical and political circles. To simplify somewhat, one could say that in such disputes it was, as a rule, the progressives who supported Darwinism whilst the conservatives violently rejected it.

Today it seems that the tables are turned. Scientists wanting to explain the evolution of human behaviour and particularly of social behaviour in Darwinistic terms are attacked from the left. Progressives of our generation denounce the attempt to unify modern biological knowledge in the new scientific branch of sociobiology as giving support to reactionary and even fascist ideas. And many scholars of anthropology, sociology, psychology and philosophy –

not necessarily with left allegiances – also voice considerable reservations about this new trend in biology.

Is such a rejection of sociobiology merely due to prejudices and misunderstandings? Is it due to the narrow-mindedness of social scientists not wanting biological ‘intruders’ in what they consider to be a field of research reserved for themselves? Can it be attributed to an over-rating of miscellaneous quotations taken out of context, which do not represent the real essence of sociobiological ideas? Or is it really true that sociobiology – and particularly its simplifying popularizations – have an *inherent* tendency to foster and support reactionary ideas?

In this book I want to show that *any one* of these questions is justified; that *none of them* can be answered with a clear and unequivocal ‘yes’ or ‘no’. Even more important in my view is the fact that the discussion on sociobiology has so far avoided what seems to me one of its most basic problems. It is what I call in this book ‘the human dimension’: the exclusively human ability to identify with *any one* of his conspecifics and feel sympathy with him. I think that basically a considerable part of the uneasiness caused by applying sociobiology to man is due to the fact that it does not take sufficient account of this most human of all human faculties. Moreover, most of its critics do not seem to realize this either; anyway, they do not spell it out clearly.

In this book I want to present a *balanced* view. I am trying to explain the essence of the arguments of supporters as well as critics of sociobiology. I do not think that this new discipline is a mere pseudo-science. It asks reasonable and relevant questions when demanding a Darwinistic explanation for the evolution of social behaviour; and these questions are amenable to scientific research. After talking with quite a number of scientists studying animal behaviour I am convinced that sociobiology has given them many a valuable impetus – even though its ideas are hotly disputed amongst biologists.

On the other hand sociobiology is a very young branch of science. It does not possess as yet a basic stock of assured and thoroughly tested knowledge. It is only just starting to delimit its field of research and to calibrate its tools. Any such new scientific enterprise must start of necessity with preliminary assumptions, hypotheses and model concepts that are still to be tested and then changed, improved

and at last perhaps adopted in the course of further research. Popular presentations that do not sufficiently emphasize this *preliminary* character of many of the ideas developed by sociobiology so far are bound to create misunderstandings and distortions.

Moreover, this field of research is of interest to each of the 'Two Cultures'. Yet natural scientists and social scientists both have their own specific and different methods of investigation and thinking arising from their different traditions. All this makes an interdisciplinary exchange of views rather difficult.

Yet even after discounting all such secondary discrepancies and the exaggerations, typical of a heated debate, there remains a hard core of basic divergence which, I think, is not due to mutual misconceptions. One main bone of contention is the question whether and to what extent it is possible to apply insights gained in the study of animal societies to man, whose societies are mainly the result of a cultural and not of a genetic evolution. Moreover there are the apprehensions of the progressives that any investigations of an inborn 'human nature' might be used – or rather abused – to vindicate an unsatisfactory status quo of human society as being 'natural'; for something that is 'natural' need not necessarily be right.

Yet man, after all, is part of nature, even if many people nowadays hardly seem to realize it. Human nature cannot be properly understood unless it is seen as part of nature as a whole, subject to the same laws of heredity and evolution. Therefore it would not appear reasonable to me to ban the question about natural inborn tendencies of man and declare it 'taboo', as it were. Neither would it be reasonable to call everybody a reactionary who is interested in such research. Such an attitude would merely result in surrendering a whole field of investigation to those who do not mind being dubbed reactionaries.

Equally, to me it would seem counter-productive to build up 'fortified positions' in the social sciences so that the concepts of sociobiology could be attacked. In the long-run, social and human scientists cannot restrict themselves to merely denouncing methodological inadequacies in the application of genetics and ethology to man. Rather, sociologists and anthropologists, psychologists and philosophers ought to strive to help scientists from the biological fields to get to grips with what is typically human. Thus many pitfalls and fallacies lurking for researchers in the human realm

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might easily be avoided. In common interdisciplinary work, with representatives of the human and social sciences, it probably would be easier to find out how human behaviour really did evolve than it would be for sociobiologists, ethologists and geneticists working in isolation.

In the course of evolution natural selection evidently put a premium on behaviour patterns that helped our pre-human and early human ancestors to adapt to the environmental conditions *of their time*. Most probably, comparative ethology and sociobiology are more likely than human and social sciences to uncover behaviour patterns that were useful to pre-humans or men of the early stone-age but which are not necessarily useful now. Whilst we cannot change our genes, the recognition and knowledge of such problems might help us to avoid trouble. We might be able to look for ways and means to ease tension resulting from the fact that we live in a totally different environment today than at the time when our specific genetic make-up was formed. We will understand man better when we know how he became man.

The idea of writing this book first occurred to me when attending the Dahlem workshop on Morality as a Biological Phenomenon in Berlin in December 1977. For a week, a panel of highly distinguished biologists, anthropologists, psychologists, social scientists and philosophers from five countries discussed problems of the evolution of human morality – and I could see for myself the extent of existing misconceptions and diverging views.

By taking part in this conference, and in two further ones on the Evolution of Social Behaviour and on Animal and Human Mind, I had the chance not only to acquaint myself with the present state of knowledge in these fields but also to talk to many of the participants and ask them questions. I am greatly indebted to the Dahlem Conferences and to their organizer Dr Silke Bernhard for giving me this unique opportunity.

At an early stage of writing this book I sent a draft of what are now sections 1.15 to 2.3 to Professor E. O. Wilson of Harvard University, one of the leading promoters of sociobiology, asking him for his comments. As the reader will see these chapters are by no means an uncritical endorsement of Wilson's views. I raised quite a number of objections and reservations. All the more I was striving to present

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his point of view in a fair and undistorted way. Wilson's reply was very encouraging and a confirmation that I had succeeded in this task. He had not found any errors in substance; there were, of course, different philosophical interpretations, but it would not be a fruitful enterprise to try to work them all out in detail. 'Your approach is reasonable,' he wrote, 'and raises issues that ought to be expressed boldly (for example, your opinion on the criteria of what constitutes evolutionary advance among major groups of social organisms).'

I can only hope that the finished volume will find a similar reception by all the scientists to whose works I refer in this book. With many of them I have had personal conversations and/or correspondence, and some of them took the time to read parts of my draft manuscript and let me know their comments. For this valuable aid I am particularly indebted to: Professor Francisco J. Ayala, University of California; Professor Patrick P. G. Bateson, University of Cambridge; Professor John T. Bonner, Princeton University; Professor Stephen T. Emlen, Cornell University; Professor Marcus W. Feldman, Stanford University; Dr Jane Goodall, Dar es Salaam; Dr Paul Harvey, University of Sussex; Professor Bert Hölldobler, Harvard University; Professor Hans Kummer, University of Zürich; Dr Charles J. Lumsden, Harvard University; Professor Hubert Markl, University of Konstanz; Professor Peter Marler, Rockefeller University; Professor Emil W. Menzel, Jr., State University of New York; Professor Charles D. Michener, University of Kansas; Professor Gordon W. Orians, University of Washington; Dr Geoffrey A. Parker, University of Liverpool; Professor Ludwig Prokop, University of Vienna; Dr Hans-Ulrich Reyer, Max-Planck-Institut für Verhaltensphysiologie, Seewiesen; Professor Donald S. Sade, Northwestern University; Dr E. Sue Savage-Rumbaugh, Yerkes Primate Research Center; Dr Robert M. Seyfarth, Rockefeller University; Professor Paul W. Sherman, University of California; Professor Robert C. Solomon, University of Texas; Professor Gunther Stent, University of California; Professor Evelyne Sullerot, formerly of the University of Paris-Nanterre; Dr Christian Welker, University of Kassel; and Dr Richard Wrangham, University of Cambridge.

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giving many valuable comments and helping me time and again in expressing my ideas in intelligible English. The interest she took in my work has been a great encouragement to me.

For the views expressed in this book and for all its shortcomings and errors I alone bear the full responsibility.

Vienna, September 1981

GEORG BREUER