

PREFACE

The first chapter does some of the work of a Preface, in explaining what the book does and does not set out to accomplish, so I can be brief here. It is not a textbook, nor an introduction to an established field. It is a personal look at the evolution of life, and in particular at the logic of natural selection and the level in the hierarchy of life at which natural selection can be said to act. I happen to be an ethologist, but I hope preoccupations with animal behaviour will not be too noticeable. The intended scope of the book is wider.

The readers for whom I am mainly writing are my professional colleagues, evolutionary biologists, ethologists and sociobiologists, ecologists, and philosophers and humanists interested in evolutionary science, including, of course, graduate and undergraduate students in all these disciplines. Therefore, although this book is in some ways the sequel to my previous book, *The Selfish Gene*, it assumes that the reader has professional knowledge of evolutionary biology and its technical terms. On the other hand it is possible to enjoy a professional book as a spectator, even if not a participant in the profession. Some laypeople who read this book in draft have been kind enough, or polite enough, to claim to have liked it. It would give me great satisfaction to believe them, and I have added a glossary of technical terms which I hope may help. I have also tried to make the book as near as possible to being enjoyable to read. The resulting tone may possibly irritate some serious professionals. I very much hope not, because serious professionals are the primary audience to whom I wish to speak. It is as impossible to please everybody in literary style as it is in any other matter of taste, and

styles that give the most positive pleasure to some people are often the most annoying to others.

Certainly the tone of the book is not conciliatory or apologetic—such is not the way of an advocate that sincerely believes in his case—and I must pack all apology into the Preface. Some of the earlier chapters reply to criticisms of my previous book, which might recur in response to the present one. I am sorry that this is necessary, and I am sorry if a note of exasperation creeps in from time to time. I trust, at least, that my exasperation remains good humoured. It is necessary to point to past misunderstandings and try to forestall their repetition, but I would not wish to give an aggrieved impression that misunderstanding has been widespread. It has been confined to numerically very limited quarters, but in some cases rather vocal ones. I am grateful to my critics for forcing me to think again about how to express difficult matters more clearly.

I apologize to readers who may find a favourite and relevant work missing from the bibliography. There are those capable of comprehensively and exhaustively surveying the literature of a large field, but I have never been able to understand how they manage it. I know that the examples I have cited are a small subset of those that could have been cited, and are sometimes the writings or recommendations of my friends. If the result appears biased, well, of course it is biased, and I am sorry. I think nearly everybody must be somewhat biased in this kind of way.

A book inevitably reflects the current preoccupations of the author, and these preoccupations are likely to have been among the topics of his most recent papers. When those papers are so recent that it would be an artificial contrivance to change the words, I have not hesitated to reproduce a paragraph almost verbatim here and there. These paragraphs, which will be found in Chapters 4, 5, 6, and 14, are an integral part of the message of this

book, and to omit them would be just as artificial as to make gratuitous changes in their wording.

The opening sentence of Chapter 1 describes the book as a work of unabashed advocacy but, well, perhaps I am just a little bit abashed! Wilson (1975, pp. 28–9) has rightly castigated the 'advocacy method' in any search for scientific truth, and I have therefore devoted some of my first chapter to a plea of mitigation. I certainly would not want science to adopt the legal system in which professional advocates make the best case they can for a position, even if they believe it to be false. I believe deeply in the view of life that this book advocates, and have done so, at least in part, for a long time, certainly since the time of my first published paper, in which I characterized adaptations as favouring 'the survival of the animal's genes...' (Dawkins 1968). This belief—that if adaptations are to be treated as 'for the good of' something, that something is the gene—was the fundamental assumption of my previous book. The present book goes further. To dramatize it a bit, it attempts to free the selfish gene from the individual organism which has been its conceptual prison. The phenotypic effects of a gene are the tools by which it levers itself into the next generation, and these tools may 'extend' far outside the body in which the gene sits, even reaching deep into the nervous systems of other organisms. Since it is not a factual position I am advocating, but a way of seeing facts, I wanted to warn the reader not to expect 'evidence' in the normal sense of the word. I announced that the book was a work of advocacy, because I was anxious not to disappoint the reader, not to lead her on under false pretences and waste her time.

The linguistic experiment of the last sentence reminds me that I wish I had had the courage to instruct the computer to feminize personal pronouns at random throughout the text. This is not only because I admire the current awareness of the masculine

bias in our language. Whenever I write I have a particular imaginary reader in mind (different imaginary readers oversee and 'filter' the same passage in numerous successive revisions) and at least half my imaginary readers are, like at least half my friends, female. Unfortunately it is still true in English that the unexpectedness of a feminine pronoun, where a neutral meaning is intended, seriously distracts the attention of most readers, of either sex. I believe the experiment of the previous paragraph will substantiate this. With regret, therefore, I have followed the standard convention in this book.

For me, writing is almost a social activity, and I am grateful to the many friends who have, sometimes unwittingly, participated through discussion, argument, and moral support. I cannot thank them all by name. Marian Stamp Dawkins has not only provided sensitive and knowledgeable criticism of the whole book in several drafts. She has also kept me going by believing in the project even through the times when I lost my own confidence. Alan Grafen and Mark Ridley, officially my graduate students, really, in their different ways, my mentors and guides through difficult theoretical territory, have influenced the book immeasurably. In the first draft their names seemed to creep in on almost every page, and it was only the pardonable grumblings of a referee that compelled me to banish to the Preface my acknowledgment of debt to them. Cathy Kennedy manages to combine close friendship for me with deep sympathy for my bitterest critics. This has put her in a unique position to advise me, especially over the earlier chapters which attempt to reply to criticism. I fear that she will still not like the tone of these chapters, but such improvement as there may be is largely due to her influence and I am very grateful to her.

I was privileged to have the first draft criticized in its entirety by John Maynard Smith, David C. Smith, John Krebs, Paul Harvey,

and Ric Charnov, and the final draft owes much to all of them. In all cases I acted on their advice, even if I did not always take it. Others kindly criticized chapters in their own special fields: Michael Hansell the chapter on artefacts, Pauline Lawrence that on parasites, Egbert Leigh that on fitness, Anthony Hallam the section on punctuated equilibria, W. Ford Doolittle that on selfish DNA, and Diane De Steven botanical sections. The book was finished at Oxford, but begun during a visit to the University of Florida at Gainesville on a sabbatical leave kindly granted by the University of Oxford and the Warden and Fellows of New College. I am grateful to my many Floridan friends for giving me such a pleasant atmosphere in which to work, especially Jane Brockmann, who also provided helpful criticism of preliminary drafts, and Donna Gillis, who also did much of the typing. I benefited, too, from a month's exposure to tropical biology as the grateful guest of the Smithsonian Institution in Panama during the writing of the book. Finally, it is a pleasure once again to thank Michael Rodgers, formerly of Oxford University Press and now of W. H. Freeman and Company, a 'K-selected' editor who really believes in his books and is their tireless advocate.

RICHARD DAWKINS

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