"The [illegible]ologist's Road to Reality"

THE MIND & THE EYE

AGNE[illegible]BER

CAMBRID[illegible]Y PRESS

THE MIND AND THE EYE

A STUDY OF THE BIOLOGIST'S STANDPOINT

BY

AGNES ARBER

M.A., D.Sc., F.R.S.
F.L.S.

CAMBRIDGE
AT THE UNIVERSITY PRESS
1954

PUBLISHED BY
THE SYNDICS OF THE CAMBRIDGE UNIVERSITY PRESS

London Office: Bentley House, N.W.1
American Branch: New York

Agents for Canada, India, and Pakistan: Macmillan

Printed in Great Britain at the University Press, Cambridge
(Brooke Crutchley, University Printer)

THE MIND AND THE EYE

To
D.S.R. and P.C.R.
sai quel che si tace

PREFACE

IN the course of a period, extending over half a century, in which my concern has been with research in plant morphology, I have found my mind dwelling more and more upon the nature of scientific thought, and its relation to other intellectual activities. Such ponderings have led me gradually to realize how little I, as a biologist, could actually justify, or even, indeed, understand, the nature of the basic assumptions and modes of argument which, in accordance with scientific tradition, I was taking simply as 'given'. For the last twenty years I have been attempting to clarify my ideas on these subjects, with the aid of such reading in metaphysics as is within the compass of the amateur. In *The Natural Philosophy of Plant Form*, published by the Cambridge University Press in 1950, I have touched upon certain aspects of the botanist's attitude to his work. The present book offers a more generalized analysis of the biologist's approach to his own subject and to philosophy. Of the defects and limitations of this study I am profoundly conscious; but my hope is that its very inadequacies may stimulate others to cast an illumination, more powerful than my rushlight, upon the biologist's road to reality.

AGNES ARBER

CAMBRIDGE
22 *May* 1953

ACKNOWLEDGEMENTS

I WISH to express my gratitude to those authors and publishers who have allowed me to use quotations from the following books: J. Clark and A. Geikie (1910), *Physical Science in the Time of Nero* (Macmillan and Co. Ltd.); F. M. Cornford (1935), *Plato's Theory of Knowledge*, and (1937) *Plato's Cosmology* (Routledge and Kegan Paul, Ltd.); E. R. Dodds (1923), *Select Passages Illustrating Neoplatonism* (S.P.C.K.); Pfeiffer's *Meister Eckhart* (1949), translated by C. de B. Evans (John M. Watkins); F. Galton (1889), *Natural Inheritance* (Macmillan and Co. Ltd.); B. Jowett (1871), *The Dialogues of Plato* (Clarendon Press, Oxford); C. Singer (1917), 'The scientific views and visions of Saint Hildegard', in *Studies in the History of Science*, vol. I (Clarendon Press, Oxford); D. W. Singer (1950), *Giordano Bruno* (Henry Schuman, Inc., New York); N. Kemp Smith (1923), *A Commentary to Kant's 'Critique of Pure Reason'*, and (1933), *Immanuel Kant's Critique of Pure Reason* (Macmillan and Co. Ltd.); A. Wolf (1910), *Spinoza's Short Treatise* (A. and C. Black, Ltd.).

I am also indebted to Henry Schuman, Inc., and to Professor Ashley Montagu and Dr George Sarton, for permission to incorporate in Chapter IV the substance of an article which I contributed to *Studies and Essays offered to George Sarton* (1946).

It is now forty years since the Cambridge University Press published my first book, and I should like to take this opportunity of offering my tribute to the Syndics and the Staff for the inexhaustible kindness and skill with which they smooth their authors' way.

A.A.

CONTENTS

PART I

THE NATURE OF BIOLOGICAL RESEARCH

PART II

THE BASES OF BIOLOGICAL THINKING

PART I

THE NATURE OF BIOLOGICAL RESEARCH

PART I

INTRODUCTION

A BIOLOGIST, asked to formulate the aim of his studies in the broadest and most general terms, might answer that he wanted to know, and above all *to understand*, the form, structure and behaviour, of living things; the chemical and physical factors in their functioning; their development, reproduction, and genetics; their relation to the non-living environment and to one another; and their race history. He would, of course, make the reservation that his acquaintance with most of the labyrinthine detail of this enormous field could not be more than second-hand, derivative, and sketchy; his first-hand knowledge could relate only to a strictly limited region. For the purpose of offering some contribution of his own to science, still further limitation would be demanded, and he would have to choose, out of the area on which his interest mainly concentrated, some place where he felt there was a certain obscurity—concerning facts, or their interpretation, or both—which he could hope to enlighten. He might be fortunate enough to turn his attention to some point where either a clear-cut question could at once be put to Nature, or where, if he could achieve a more exact description of the facts, some individual inquiry might eventually emerge and formulate itself. This brings into view the research worker's first objective—the choice either of a specific question, or of some problematic area of biological thought from which, under the stimulus of factual study, he can hope that a distinct question will gradually crystallize. The finding of a genuine problem is indeed of fundamental importance. The mind languishes when faced with an enigma which lacks content, but, confronted by a problem worthy of its steel, it often displays powers which were previously quite unsuspected.[1]

When the researcher has arrived at a definite question, he

[1] Cf. Meyerson, É. (1931), vol. II, pp. 615–17.

hardly ever finds the facts, which he needs for the solution of his problem, ready to his hand; in order that they may be facts *for him*, he has to acquire them for himself, from his own standpoint. The second stage is thus the observational or experimental search for relevant data, and an attempt to organize them. After this comes the third stage—the endeavour to interpret the marshalled facts. The data may prove to fit neatly into the existing framework of scientific explanation, or they may demand a reconstruction of this framework before they can find their place in it.

If the programme has been carried successfully through these three stages, some sort of solution of the initial problem will have been reached, and the biologist comes to the fourth phase of his work—the attempt to test the validity of his solution; in other words he has to do what is commonly described as 'proving' its truth.

The first four phases just enumerated have had only an internal reference, but, in the next, the researcher has to turn his gaze outwards; for he has now to consider how to communicate the course of his investigations, and the conclusions to which he has come, to his fellow-students, who cannot know what it is all about, except through the medium of his skill with pen, and sometimes with pencil.

In these days of specialization, the biologist who hopes to add anything to science is bound to confine his attention during the greater part of his working life to these five stages. Nevertheless, most workers occasionally, and those of a contemplative temperament more frequently, are conscious of an urge to pass on to a further stage—the sixth in the sequence we are tracing. In this the biologist stands back from the individual jobs to which he has set his hand, in order to see them in the context of thought in general; to criticize their presuppositions and the mode of thinking which they employ; and to discover how the intellectual and sensory elements, which they include, are interconnected. This urge is more likely to supervene late in life, since the contemplative spirit then gathers a force that it could not collect in earlier years, when the mind, in its fresh receptivity, concen-

trated its attention upon the delightful detail of the factual multiplicity of living things. This terminal stage of biological thinking may take more than one form. The worker may attempt to realize his own individual findings in relation to those far-reaching problems which are common to the various fields of thought; his activities at this level will then extend into the territory of philosophy. On the other hand, if it is the visual rather than the purely intellectual aspect of his problems which enthrals him, and affords him a glimpse of wider horizons, he may discover that his reaction to his experiences is, in the long run, that of the artist rather than the philosopher. Vesalius is a cogent example of a great biologist, who did not think in terms of intellectual concepts, but was essentially a creative artist-naturalist.[1]

The pattern of the present book is so disposed as to follow, in general outline, the ascending scale suggested by the six stages here recognized. The chief stress will be laid upon the ultimate grade, in which, in the writer's personal view, the previous phases all find their end and their justification.

[1] On the position of Vesalius in biology, see Singer, C. (1925); Nordenskiöld, N. E. (1950); and Singer, C., and Rabin, C. (1946).

CHAPTER I

THE BIOLOGIST AND HIS PROBLEM

SINCE the first step in biological research involves the decision as to the question on which to concentrate, the researcher is at once put upon his mettle, for the full recognition and appreciation of a problem may task him even more severely than its solution. It is undoubtedly true that the "difficulty in most scientific work lies in framing the questions rather than in finding the answers";[1] an unerring instinct for the valid problem is likely, indeed, to be the ripened harvest of the scientific life, rather than its first-fruits. In practice, however, the paradox, that the thinker needs to have reached the end before he can make an effective beginning, must be ignored; for full preparedness for the start never comes until the time for starting has long gone by. So the biologist must begin without tarrying for full equipment, making shift from the first to pose his own question as best he may. It is true that the difficulty may be side-tracked temporarily, since the young worker, during his apprenticeship, is often supplied with a ready-made problem by his supervisor. Such tutelage is inevitable, if the student is to be put in possession of the traditional mode of approach and the existing technique. As a strictly ephemeral phase, it should not undermine originality—it may, on the contrary, assist it, for no man can create a new offshoot from tradition, or break with it to advantage, unless he knows precisely *what it is* that he is developing or discarding. The fact that spoon-feeding with imposed problems sometimes lingers on into later years, owing to the exigencies of team-work, may seriously impede intrinsic development; for each biologist ought to be able to say to himself, like Descartes, that his intention is to build upon a foundation that is all his own.[2] It was Descartes, again, with his sturdy

[1] Boycott, A. E. (1929), p. 95.

[2] "de bastir dans un fons qui est tout a moy" [Descartes, R.] (1637), p. 16; (1947), p. 15. Translation, Haldane, E. S., and Ross, G. R. T. (1911–12), vol. 1, p. 90.

individualism, who held that the only help that can be given to a researcher is to defray his needful scientific expenses, and then to ensure that no one filches his leisure.[1]

When we consider problems in the light of history, we find that they are, in one aspect, suprapersonal; for what particular biological questions excite special interest at any given period is a sign of the general intellectual focus of that period; contemporary topics and those of the past are thus seen through quite different spectacles. One becomes acutely conscious of this divergence in looking over old volumes of research journals; they are filled, in the main, with articles in which the authors tackle, with enthusiasm, problems which have now simply lost their interest for us. It is not by any means always that these problems have since found their answers, and that they have been built into the structure of the science; often they have been shelved rather than solved, yet the notion of pursuing them evokes in us now nothing but ennui. This feeling may have a sound basis, for a problem put aside in one period must have the right interval of dormancy before awakening, freshened, to an unforced solution, when the time is naturally ripe. In the history, for instance, of such a subject as psychology, which is relatively modern as a self-conscious and autonomous science, we can see how many starts have been made in different directions, and have afterwards been set aside in favour of other lines.[2] It seems safe to prophesy that future generations will, from a more advanced standpoint, return to reap a harvest from some of these now forsaken beginnings. That there is a time for everything, and a season for every purpose under heaven, is indeed continually exemplified in the history of science; the general intellectual atmosphere of any given moment has an effect upon this history which is compulsive to a humiliating degree. In every period certain classes of beliefs and ideas have been actively distasteful, and even workers of some independence of mind are found to have shrunk

[1] " . . . fournir des frais des experiences dont il auroit besoin, et du reste empescher que son loisir ne luy fust osté par l'importunité de personne" [Descartes, R.] (1637), p. 73; (1947), p. 73. Translation, Haldane, E. S., and Ross, G. R. T. (1911–12), vol. I, p. 127.

[2] Cf. the history of a century of psychological study in Flugel, J. C. (1933).

from them as if they were tabooed. In the literature of biology one may occasionally detect a hint of an untrodden and unauthorized way, which held out a prospect of a fresh view-point; but the man, who had glimpsed it, too often proceeded to turn his back upon it, reverting to the familiar beaten paths, where he could absorb confidence from the reassuring society of his fellow-workers. The tyranny of the *Zeitgeist* is obvious enough to us in the scientific writing of fifty years ago, whereas we are less aware of it in that of this year, because we are always too much bedazzled by contemporaneity to judge the present fairly; half a century hence, biologists will, no doubt, find as much tedium in the general approach of to-day as we do in that of a time which is to us equally long past. It is sad that it should be so, for we lose a great deal if we discard the conceptions of past generations indiscriminately, because the swing of the pendulum of fashion makes us react against them.

In every field of biological research, the intellectual climate and the general beliefs of the time cannot but be especially and rightfully influential where the beginner is concerned. He will best 'find himself' in his work if he embarks upon a task which he can realize as a part, even if a minimal part, of some attempt much larger than itself, which is a tributary of the main thought-stream of the period. If, for instance, he is a taxonomist, aiming only at dealing with the affinities of a single form, he will make the best job of his attempt by viewing it as a microcosm of the comprehensive subject of relationship in the whole living world, as seen on the background of existing classificatory ideas. Later on, he may profoundly modify, or discard these ideas, but meanwhile the natural thing will be to begin by putting them to the test of use, so that, when he departs from them, it will be for well-knit reasons.

The work of the taxonomist, like that of the morphologist, is sometimes slighted as being purely 'descriptive', and hence of no theoretic interest; but this criticism is based on an inadequate notion of what the term *description* should mean in biology. By rights it should come into quite a different category from mere mechanical representation in words. To begin with, *what* is to be

described is not fortuitous, but demands preliminary selection, involving theoretical implications; otherwise the observer would be lost in a chaotic nightmare of phenomena clamouring on all sides for his attention. Moreover, every description exists on a background of biological theory, to which it is intimately related—whether this relationship is expressed or merely understood. In so far as description includes this element of relatedness, it may be held to be contributing its mite towards testing the assumption that there is coherence between the different parts within Nature.[1] Work that is seemingly a descriptive study of the most pedestrian type may thus, on closer scrutiny, be seen to have also some theoretic function. At a higher level, the ideal of scientific research appears to be most nearly approached when delicate and detailed practical investigation is employed in a conscious attempt to solve theoretic questions of the broadest kind. Conspicuous examples of such work are seen in the still young study of what is called 'fine structure'—the realm of organization between individual molecules and microscopic structures[2]—and in the related 'chemical geography' within the cell.[3] Here we have a factual analysis of perceptual data obtained by a highly evolved technique, associated with conceptual synthesis of an abstract type. The study of form is thus raised to a plane above, but including, pure empiricism.

One of the factors determining the choice of a researcher's problem is whether the trend of his mind is towards the descriptive, or the more definitely theoretical. In science, almost as much as in art, tastes have to be reckoned with; for the researcher's choice of a field cannot be satisfying unless both feeling and intellect converge upon it. No one, indeed, can reach a creative solution of a problem which he does not approach *con amore*. Scientific men, of a non-analytic turn, when catechized as to *why* they do their work, often give elaborately altruistic reasons for their devoted concentration; but those who are more introspective confess that, in reality, they do it simply because they like it, and (in the Cornish sense) it 'belongs' to them to do

[1] See Chapter VII, pp. 82–8, of the present book.
[2] Cf., for example, Picken, L. E. R. (1950).
[3] Bradfield, J. R. G. (1950), p. 81.