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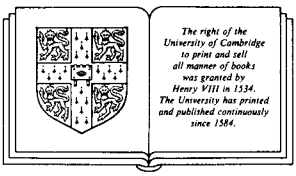
THE MIND AND THE EYE

A STUDY OF THE BIOLOGIST'S STANDPOINT

AGNES ARBER, FRS, FLS

With an Introduction by P. R. Bell

Department of Botany and Microbiology
University College London



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

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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

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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.

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INTRODUCTION

The republication of Agnes Arber's *The Mind and the Eye* at a time when so much of the stress in biology is on the molecular and genetic may seem anachronistic. This would be far from the truth. Advances in biology continue to depend on what Goethe (in *The Theory of Vision*) called 'das denkende Auge'. Agnes Arber's aim in her classic work was to illustrate this theme by an analysis of the essential steps in biological research. Her examples may appear dated, but the principles she recognizes are not. It is Agnes Arber's insight into the fundamental aspects of research, arising from many years of distinguished work in the field of plant morphology, which gives *The Mind and the Eye* its enduring quality.

Particularly timely is Agnes Arber's treatment of description in biology. 'Merely descriptive' is often used dismissively in relation to morphological and anatomical work. Frequently the implied rebuke is deserved, but description remains an irreplaceable element in all biological research. This is particularly evident where new techniques are opening up wholly new areas of observation. The problem of the causes of the two phase changes in life cycle of the land plants ('alternation of generations' in the older terminology), for example, depends for its solution upon knowledge of the intimate details of gametogenesis and sporogenesis. Electron microscopy has made this possible, and this has led to descriptive work of the highest order. But what is the 'highest order'? Firstly, of course, accuracy. Secondly, the satisfactoriness with which the discoveries are related to the structure of cells generally. And thirdly the convincingness with which the interpretation of unique structures or unique conditions of familiar cell components is argued in relation to current theories of cell structure and function. Only when the researcher has constructed an intellectual framework

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of this kind can he begin to plan experiments which will be useful, 'useful' in this context meaning those experiments of which the results can be sensibly interpreted. The current studies of self-sterility in *Raphanus* (radish) provide a splendid example of how ultrastructural investigation leads to rewarding experimentation, and demonstrate the powerful role of description in an area transformed by advances in the resolving power of microscopy.*

Agnes Arber naturally has very cogent things to say about the nature of hypotheses in the biological sciences. The grander hypotheses, particularly those involving historical events, often owe much to analogy with human experiences. Darwin's theory of evolution, for example, imposes upon the natural world concepts derived by analogy with the practices of animal and plant breeders. It has been argued extensively elsewhere, for example by Popper, whether the theory of evolution, being too imprecise to subject to experimental test, can legitimately be considered a scientific theory *sensu stricto*. A question less often asked, but well worth asking, is whether the analogy between evolution and the practices of breeders is a good one. The value of an analogy is that its quality can be assessed on a continuous scale. If an analogy is present at all it is likely to have some usefulness, and it may be an important stimulus to further research. A recent example is the parallel which has been drawn between the interaction of traffic flows at a complex junction, and the inflow of nutrients and the outflow of products in a plant community. The systems of matrix algebra which have been devised to identify the controlling factors in the first may lead to the recognition of hitherto unsuspected interactions in ecology.

It is now commonly accepted that 'scientific method' is largely an illusion, and that the logical sequence it envisages by which a researcher reaches his conclusions is an ideal

* See, for example, H. G. Dickinson & D. Lewis (1973). Cytochemical and ultrastructural differences between intraspecific compatible and incompatible pollinations in *Raphanus*. *Proc. R. Soc. Lond.* **183B**, 21–36. *Idem*, The formation of the tryphine coating the pollen grains of *Raphanus*, and its properties relating to the self-incompatibility system. *Ibid.* **184B**, 149–65.

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formulation possible only after the investigation is completed. The procedures of the individual scientist are altogether more haphazard. The driving force in research is of course motivation, fuelled by the intellectual fascination afforded by certain aspects of the material with which the researcher is confronted. The analysis of these phenomena, proceeding continuously in the observer's mind, gives rise to possible relationships, which may be either causal or conceptual depending upon the nature of the research. The researcher may ultimately become convinced of their validity well before the evidence is in a form which can be presented and found satisfying by his colleagues. Agnes Arber shows how this has long been a problem with the great, quoting the saying attributed to Gauss 'I have had my results for a long time; but I do not yet know how to arrive at them'.

The presentation of evidence raises of course the question of writing, an art in which Agnes Arber herself was so accomplished. Biologists may, suggests Agnes Arber, legitimately make more use of the literary aspects of language than is customary in physics and chemistry. Instead of thinking of his composition as a sequence of words, each with a precise meaning, the biologist should perhaps pay regard to their effect as a whole, as an organism is more than the sum of its parts. Many would regard this as dangerous country, and would not follow Agnes Arber in seeing the biologist's task as set between the purely rational approach of the physical sciences and the more imaginative one of the humanities. Without denying the difficulty of expressing some biological concepts in a precise manner (explanations of phyllotaxy, for example, frequently involve concepts of 'morphogenetic fields' at the apex, a masterly cloaking of ignorance in elegant language), the ultimate aim must surely be to evolve concepts and systems capable of exact definition and, where appropriate, of measurement. Since Agnes Arber wrote, we do perhaps see electronic engineers beginning to experience the same difficulties as biologists. As the number of circuits imprinted on microchips increases, and the interactions of these circuits become ever more complex, the terminology of

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computer science is tending to become biological. In a recent discussion of the reliability of computer memories the author goes so far as to speak of half chips or even whole chips having been 'known to go bad'. Although a technical article, biological decay, a process involving many interacting systems, was evidently seen as an appropriate analogy for the failure of an intricate component within a highly developed machine.

Language inevitably raises the topic of translation. The regrettable decline of German as a scientific language has resulted in many papers from German-speaking countries which although grammatically correct, nevertheless lack clarity. Sometimes, since the writing has come more from the dictionary than from the mind, transferring the text back to German reveals the writer's intentions. While it is probably still better for scientists to write in their native tongues, it has to be conceded that the international nature of science renders this impracticable. Indeed English is tending increasingly to become the *lingua franca* of science, and those who do not publish in English run the risk of being ignored. If English is to be accepted as the international language of scientific research, then a standard vocabulary and agreed rules of syntax would seem to be essential. A parallel would be with the form of Latin which is used by taxonomists throughout the world to describe new species of plants. It would of course be an impoverished English, bereft of charm and topical nuances, but it may be the price which has to be paid for unambiguous communication between scientists in different lands. It is a dismal prospect for those who love language and fine writing, but there will always be opportunities for scientists to indulge in these delights in their own communities.

In her discussion of the 'correspondence' and 'coherence' theories of truth Agnes Arber enters waters which many practising biologists may find unfamiliar. The 'correspondence' theory probably describes the approach of most biologists, namely it is expected that what is held to be true will correspond so far as it is possible to the observed facts. Where observations

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depend upon measurements, the likelihood of error can often itself be expressed in numerical terms following appropriate statistical analysis. Where the statements are made pictorially, as by the use of electron micrographs selected from many, fellow scientists have to depend upon the presenter's assurance that they are representative. The confidence with which this can be accepted will naturally depend upon many considerations, not necessarily directly concerned with the work in question. The validity or otherwise of the claim will of course ultimately become clear as other investigators enter the field. The situation then approaches that envisaged by the 'coherence' theory of truth, according to which truth becomes established as it acquires 'systematic coherence'. Agnes Arber draws attention to the parallel with *Richtigkeit* and *Wahrheit* in Hegelean philosophy, the first corresponding to objective 'correctness', and the second to an altogether more complex notion of truth in which the bare observations, complemented by subjective elements, acquire greater significance and value. In this discussion, despite Agnes Arber's persuasiveness, many will nevertheless feel distinctly uneasy, and will not readily accept that biological science, as distinct from physics and chemistry, necessarily involves metaphysical concepts. It is doubtful whether, to take Agnes Arber's example, the structure of an organ of a plant or animal can ever be 'explained' on teleological grounds, or even that such an explanation can be regarded as a 'partial truth'. A teleological 'explanation' appears to resemble 'scientific method' in that both are products of reasoning after the process to which they relate has been completed, and although notionally satisfactory, neither is an authentic representation of events as they really happen. A physico-chemical explanation of the structure of an organ, if it could ever be achieved, might well be predictive, in that it would be possible to foretell, given changes in the controlling factors involved, how the structure of the mature organ would be modified. Attempts at this kind of analysis are to be found in D'Arcy Thompson's *On Growth and Form*, where it is shown that different physiognomies can be related by relatively simple

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changes in co-ordinates. As the myriad activities of genes and their products in differentiating cells become known it is not inconceivable that computer modelling will make it possible to portray how cell lineages generate complex organs. This of course is still very far away, and a phenomenon as familiar as the differentiation of a xylem cell has yet to be explained *in toto*. Nevertheless an approach of this kind is becoming increasingly feasible. To many biologists it would seem to be entirely in harmony with the spirit of scientific inquiry, and notions of teleology will seem to be altogether less cogent and belong to a wholly different realm.

In her discussion of biological antitheses Agnes Arber touches on the nature of 'how' and 'why' questions in biological science. One attempts to answer 'how' questions in a mechanistic way, although such explanations, as we have seen in relation to stem apices, may founder in the vaguest of concepts, since the interplay of the physico-chemical forces in the cells concerned is often largely unknown. 'Why' questions are difficult to answer, even if the intention of such questions is at all clear. It may be that, in the context of science, 'why' questions are in fact meaningless, and irrelevant to the inquiry being undertaken. Even if the cellular events culminating in a particular structure were to be fully understood, it is no explanation to say that their *raison d'être* lies in the structure concerned bestowing a selective advantage on the individual possessing it. This merely accounts for the persistence and perfection of this structure in the course of evolution. Function is therefore no explanation of form; form and function are different aspects of the same thing – a feature becoming increasingly evident as structural research enters the realm of the molecular. Current investigations of membrane structure, particularly of the photosynthetic membranes, are providing magnificent examples of how function follows ineluctably from form. The analysis of the very fast light reactions in photosynthesis shows that the electron donors and acceptors must be arranged in a defined sequence in the membrane, since otherwise the ordered transfer of the electrons could not take place. Electron microscopy of an advanced kind,

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coupled with immunocytochemical techniques and image analysis, has now revealed the molecular symmetry of the photosynthetic reaction centre itself, and also its asymmetrical placing in the membrane.* The properties of the molecules which make up the centre and the way in which they are arranged with respect to each other make it inevitable that when irradiated with light of certain wavelengths the centre behaves in a well-defined way and light energy becomes transformed into chemical energy. At molecular dimensions function follows the laws of thermodynamics, and at this level teleological 'explanations' again appear wholly anomalous.

Most biologists at the present time would probably regard themselves as belonging to that branch of natural science which deals with the organic world. They would probably regard natural philosophy as being suspect. Nevertheless, attitudes should not become too firm, and on many matters it is better to retain an open mind. The form of a flower, for example, may be assumed by a single flower, an inflorescence, or an aggregate of inflorescences. Is this adequately accounted for by natural selection? Can there be some kind of stability in the flower-like form which reflects the natural resultant of the forces of growth in the reproductive region? Evolution of the morphological expression of the reproductive stage would then be constrained, and its end products would tend towards visual similarity. The concept of a 'flower-like' form, towards the realization of which the plant strives as it enters the reproductive phase, is undeniably Platonic in nature, and yet not incapable of being re-expressed in terms of plant physiology. The attitude of the practising biologist towards natural philosophy should preferably be one of caution rather than outright rejection. Just as profitable mathematics can be done with expressions involving the irrational $\sqrt{(-1)}$, so too can good science emerge from investigations in which the thinking has been metaphysical as

* See, for example, W. Stark, W. Kühlbrandt, I. Wildhaber, E. Wehrli and K. Mühlethaler (1984). The structure of the photoreceptor unit of *Rhodopseudomonas viridis*. *EMBO J.* **3**, 777–83.

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well as physical. Following the conventions of experimental science, however, the metaphysical elements will be omitted from the published work, and the results will appear to be 'soundly' based on objective reasoning. It is fitting that it should be so since the scientist, as scientist, is concerned with the achievements of the research and the extent to which their validity is supported by experiment and reasoning, not with the tortuous paths which have led to them.

Biologists, as most scientists, become so absorbed in the phenomena they are investigating that each generation tends to forget that it is not the first to be caught up intellectually in this kind of inquiry. Throughout the history of biology there have been great minds that have stood aside and examined the nature of knowledge, and the nature of the observation, experiment and reasoning which lead to its expansion. This is a process in which there are many interacting elements, and it is a worthwhile undertaking periodically to take stock of them. *The Mind and the Eye* remains a magnificent treatment of these themes, and its republication is to be welcomed by the reflective in all disciplines. Agnes Arber's erudition, unpretentious yet awesome to many of us, never impedes the gracious flow of the writing, and we are reminded again that the attempt to understand the natural world has a long and distinguished history. The individual biologist's self-esteem will not be wounded by seeing himself as part of a scholarly tradition and realizing that he is but one of many who have turned, and will continue to turn, their intellectual energies to probing the mysteries of living organisms. In recent years the spectacular advances in molecular biology have generated such excitement that there has perhaps been a tendency for organisms to be overlooked. Biology must nevertheless remain 'organismic', and the researcher who loses the concept of organisms seriously weakens his claim to be a biologist. It does not follow that a biologist cannot happily research in the molecular domain. The situation of many of today's biologists is expressed succinctly and timelessly in Goethe's lines

Willst Du Dich am Ganzen erquicken
So muss Du das Ganze im Kleinsten erblicken.

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The same sentiments apply to Agnes Arber's careful dissection of the biologist's manner of thinking and working. Although we may argue about some of the details, we emerge from *The Mind and the Eye* with an altogether enhanced appreciation of the richness of biological science.

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