CAMBRIDGE LIBRARY COLLECTION

Books of enduring scholarly value

Life Sciences

Until the nineteenth century, the various subjects now known as the life sciences were regarded either as arcane studies which had little impact on ordinary daily life, or as a genteel hobby for the leisured classes. The increasing academic rigour and systematisation brought to the study of botany, zoology and other disciplines, and their adoption in university curricula, are reflected in the books reissued in this series.

Materials for the Study of Variation

Building on the work of Darwin and Mendel, the biologist William Bateson (1861–1926) was the first scientist to combine the study of variation, heredity and evolution, and to use the term 'genetics'. This book was first published in 1894 after many years of experimental and theoretical work – particularly in the embryology of the acorn worm genus *Balanoglossus* – which had been guided by the principle that embryonic developmental stages replay the evolutionary transitions of adult forms of an organism's ancestors. Bateson was the first to challenge this theory, which made him unpopular among the scientific establishment of the time, but he was proved right. Organising his material by anatomical sections, Bateson explores speciation, phylogeny and discontinuous and continuous variation among a wide range of species, including vertebrates, invertebrates and plants. This pioneering work offers great insight into how the study of genetics and inheritance itself evolved.

Materials for the Study of Variation

Treated with Especial Regard to Discontinuity in the Origin of Species

William Bateson

CAMBRIDGE UNIVERSITY PRESS

CAMBRIDGE UNIVERSITY PRESS

Cambridge, New York, Melbourne, Madrid, Cape Town,
Singapore, São Paolo, Delhi, Mexico City

Published in the United States of America by Cambridge University Press, New York

www.cambridge.org
Information on this title: www.cambridge.org/9781108053129

This edition first published 1894
This digitally printed version 2012

ISBN 978-1-108-05312-9 Paperback

MATERIALS

FOR THE

STUDY OF VARIATION.

MATERIALS

FOR THE

STUDY OF VARIATION

TREATED WITH ESPECIAL REGARD TO

DISCONTINUITY

IN THE

ORIGIN OF SPECIES.

BY

WILLIAM BATESON, M.A.
FELLOW OF ST JOHN'S COLLEGE, CAMBRIDGE

London:
MACMILLAN AND CO.
AND NEW YORK.
1894

Cambridge:
PRINTED BY C. J. CLAY, M.A., AND SONS,
AT THE UNIVERSITY PRESS.

PREFACE.

THIS book is offered as a contribution to the study of the problem of Species. The reasons that have led to its production are as follows.

Some years ago it was my fortune to be engaged in an investigation of the anatomy and development of *Balanoglossus*. At the close of that investigation it became necessary to analyze the meaning of the facts obtained, and especially to shew their bearing upon those questions of relationship and descent which modern morphology has attempted to answer. To this task I set myself as I best might, using the common methods of morphological argument and interpretation, and working all the facts into a scheme which should be as consistent as I could make it.

But the value of this and of all such schemes, by which each form is duly ushered to its place, rests wholly on the hypothesis that the methods of argument are sound. Over it all hung the suspicion that they were not sound. This suspicion seemed at that time so strong that in preface to what I had to say I felt obliged to refer to it, and to state explicitly that the analysis was undertaken in pursuance of the current methods of morphological criticism, and without prejudging the question of possible or even probable error in those methods.

Any one who has had to do such work must have felt the same thing. In these discussions we are continually stopped by such phrases as, "if such and such a variation then took place and was favourable," or, "we may easily suppose circumstances in which such and such a variation if it occurred might be beneficial," and the like. The whole argument is based on such assumptions as these—assumptions which, were they found in the arguments of Paley or of Butler, we could not too scornfully ridicule. "If," say we with much circumlocution, "the course of Nature followed the

lines we have suggested, then, in short, it did." That is the sum of our argument.

Were we all agreed in our assumptions and as to the canons of interpretation, there might be some excuse, but we are not agreed. Out of the same facts of anatomy and development men of equal ability and repute have brought the most opposite conclusions. To take for instance the question of the ancestry of Chordata, the problem on which I was myself engaged, even if we neglect fanciful suggestions, there remain two wholly incompatible views as to the lines of Vertebrate descent, each well supported and upheld by many. From the same facts opposite conclusions are drawn. Facts of the same kind will take us no further. The issue turns not on the facts but on the assumptions. Surely we can do better than this. Need we waste more effort in these vain and sophistical disputes?

If facts of the old kind will not help, let us seek facts of a new kind. That the time has come for some new departure most naturalists are now I believe beginning to recognize. For the reasons set forth in the Introduction I suggest that for this new start the Study of Variation offers the best chance. If we had before us the facts of Variation there would be a body of evidence to which in these matters of doubt we could appeal. We should no longer say "*if* Variation take place in such a way," or "*if* such a variation were possible;" we should on the contrary be able to say "since Variation *does*, or at least *may* take place in such a way," "since such and such a Variation *is* possible," and we should be expected to quote a case or cases of such occurrence as an observed fact.

To collect and codify the facts of Variation is, I submit, the first duty of the naturalist. This work should be undertaken if only to rid our science of that excessive burden of contradictory assumptions by which it is now oppressed. Whatever be our views of Descent, Variation is the common basis of them all. As the first step towards the systematic study of Variation we need a compact catalogue of the known facts, a list which shall contain as far as possible all cases of Variation observed. To carry out such a project in any completeness may be impossible; but were the plan to find favour, there is I think no reason why in time a considerable approach to completeness should not be made.

Difficulty has hitherto arisen from the fact that Variation is not studied for its own sake. Each observer has some other object in view, and we are fortunate if he is good enough to mention in passing the variations he has happened to see in following his own ends. From the nature of the case these observations must at first be sporadic, and, as each standing alone seems to have little value, in the end they are unheeded and lost. If there were any central collection of facts to which such observations might from time to time be added, and thus brought into relation with cognate observations, their value would at once appear and be preserved. To make a nucleus for such a collection is the object of the present work.

The subject treated in this first instalment has been chosen for the reasons given in the text. Reference to facts that could not be included in this section of the evidence has as far as possible been avoided, but occasionally such reference was necessary, especially in the Introduction.

It was my original purpose to have published the facts without comment. This course would have been the most logical and the safest, but with hesitation it was decided to add something of the nature of analysis. I do this chiefly for two reasons. First, in starting a method one is almost compelled to shew the way in which it is to be applied. If it is hoped that others may interest themselves in the facts, it is necessary to shew how and why their interest is asked. In the old time the facts of Nature were beautiful in themselves and needed not the rouge of speculation to quicken their charm, but that was long ago, before Modern Science was born.

Besides this, to avoid the taint of theory in morphology is impossible, however much it may be wished. The whole science is riddled with theory. Not a specimen can be described without the use of a terminology coloured by theory, implying the acceptance of some one or other theory of homologies. If only to avoid misconception matters of theory must be spoken of.

It seemed at first also that the meaning of the facts was so clear that all would read it alike; but from opportunities that have occurred for the discussion of these matters I have found that it is not so, and reluctantly I have therefore made such comments as may serve to bring out the chief significance of the

phenomena, pointing out what they shew and what they do not shew, having regard always to deficiencies in the evidence.

That this is a dangerous course I am aware. But in any discussion of a problem in the light of insufficient knowledge the real danger is not that a particular conclusion may be wrong, for that is a transient fault, but rather that the facts themselves may be so distorted as to be valueless to others when the conclusions that they were used to shew have been discarded. This danger I have sought indifferently to avoid by printing the facts as far as possible apart from all comment, knowing well how temporary the worth of these comments is likely to be. I have thus tried to avoid general statements and have kept the descriptions to particular cases, unless the number of similar cases is great and an inclusive description is enough.

Each separate paragraph relating a fact has been as far as possible isolated and made to stand alone; so that if any one may hereafter care to go on with the work he will be able to cut out the discarded comments and rearrange the facts in any order preferred, inserting new facts as they come to hand. Most of these facts are numbered for reference. The numbers are distributed on no strict system, but are put in where likely to be useful.

For almost every fact stated or mentioned one reference at least is given. When this is not the case the fact is either notorious, or else the result of my own observation. In collecting evidence I have freely used the collections of former writers, especially those of Geoffroy St Hilaire, Ahlfeld, and Wenzel Gruber, but unless the contrary is stated, each passage referred to has been seen in its original place. By this system I hope I have avoided evidence corrupted by repetition. Several well known conceptions, notably that of the presence of order in abnormality, first formulated by Isidore Geoffroy St Hilaire, have been developed and exhibited in their relation to recent views.

The professed morphologist will note that many of the statements are made on authority unfamiliar to him. I have spared no pains to verify the facts wherever possible, and no case has been admitted without remark if there was reason to doubt its authenticity. So long as skilled zoologists continue to neglect all forms that are abnormal the student of Variation must turn to other sources.

This neglect of the Study of Variation may be attributed in

great measure to the unfortunate circumstance that Natural History has come to be used as a vehicle for elementary education, a purpose to which it is unsuited. From the conditions of the case when very large classes are brought together it becomes necessary that the instruction should be organized, scheduled, and reduced to diagram and system. Facts are valued in proportion as they lend themselves to such orderly treatment; on the rest small store is set. By this method the pupil learns to think our schemes of Nature sufficient, turning for inspiration to books, and supposing that by following his primer he may master it all. In a specimen he sees what he has been told to see and no more, rarely learning the habit of spontaneous observation, the one lesson that the study of Natural History is best fitted to teach.

Such a system reacts on the teacher. In time he comes to forget that the caricature of Nature shewn to his pupils is like no real thing. The perspective and atmosphere that belong to live nature confuse him no more. Two cases may be given in illustration. Few animals are dissected more often than the Crayfish and the Cockroach. Each of these frequently presents a striking departure from the normal (see Nos. 83 and 625) in external characters, but these variations have been long unheeded by pupil and by teacher; for though Desmarest and Brisout published the facts so long ago as 1848, their observations failed to get that *visa* of the text-books without which no fact can travel far.

It is especially strange that while few take much heed of the modes of Variation or of the visible facts of Descent, every one is interested in the *causes* of Variation and the nature of "Heredity," a subject of extreme and peculiar difficulty. In the absence of special knowledge these things are discussed with enthusiasm, even by the public at large.

But if we are to make way with this problem special knowledge is the first need. We must know what special evidence each group of animals and plants can give, and this specialists alone can tell us. It is therefore impossible for one person to make any adequate gathering of the facts. If it is to be done it must be done by many. At one time I thought that a number of persons might perhaps be induced thus to combine; but though I hope hereafter some such organized collection may be made, it is perhaps necessary that the first trial should be single-handed.

As I have thus been obliged to speak of many things of which I have no proper knowledge each section must inevitably seem meagre to those who have made its subject their special study, and I fear that many mistakes must have been made. To any one who may be willing to help to set these errors right, I offer thanks in advance, "humbly acknowledging a work of such concernment unto truth did well deserve the conjunction of many heads."

In the course of the work I have had help from so many that I cannot here give separate thanks to each. For valuable criticisms, given especially in connexion with the introductory pages, I am indebted to Mr F. Darwin, Dr C. S. Sherrington, Dr D. MacAlister, Mr W. Heape, Mr G. F. Stout, Dr A. A. Kanthack and particularly to Mr J. J. Lister. I have to thank the authorities of the British Museum, of the Museum of the Royal College of Surgeons, of the Musée d'Histoire Naturelle in Paris, and of the Museums of Leyden, Oxford, Rouen, Newcastle-upon-Tyne, of the École Vétérinaire at Alfort, and of the Dental Hospital for the great kindness that they have shewn me in granting facilities for the study of their collections. In particular I must thank Mr Oldfield Thomas of the British Museum for much help and advice in connexion with the subject of Teeth. I am also greatly obliged to Messrs Godman and Salvin for opportunities of examining and drawing specimens in their collections. To many others who have been good enough to lend specimens or to advise in particular cases my obligations are acknowledged in the text, but I must especially express my gratitude to Dr Kraatz of Berlin, to Dr L. von Heyden of Frankfurt, and to M. H. Gadeau de Kerville of Rouen for the large numbers of valuable insects with which they entrusted me.

My best thanks are due to Dr A. M. Norman for many useful suggestions, for the loan of specimens and for the kindly interest he has taken in my work.

My friend Mr H. H. Brindley has very kindly given me much assistance in determining and verifying several points that have arisen, and I am particularly indebted to him for permission to give an account of his very interesting and as yet unpublished observations on the variation and regeneration of the tarsus in Cockroaches.

Through the help of Dr David Sharp I have been enabled to introduce much valuable evidence relating to Insects, a subject of

which without his assistance I could scarcely have spoken. It is impossible for me adequately to express my obligation to Dr Sharp for his constant kindness, for the many suggestions he has given me, and for the generosity with which he has put his time and skill at my service.

It is with especial pleasure that I take this opportunity of offering my thanks to Professor Alfred Newton for the encouragement and sympathy he has given me now for many years.

As many of the subjects treated involve matters of interpretation it should be explicitly declared that though help has been given by so many, no responsibility for opinions attaches to anyone but myself unless the contrary is stated.

The blocks for Figs. 18, 19, 25, 133, 161 and 185 (from *Proc. Zool. Soc.*) were very kindly given by the Zoological Society of London; that for Fig. 28 (from *Trans. Path. Soc.*) by the Pathological Society; and for Fig. 140 which is from the *Descent of Man* I am obliged to the kindness of Mr F. Darwin. Figs. 5 B, 5 C, and 77 were supplied by the proprietors of Newman's *British Butterflies*, and Figs. 5 A, 82 and 84 by the proprietors of the *Entomologist*. The sources of other figures are acknowledged under each. Those not thus acknowledged have been made from specimens or from my own drawings or models by Mr M. P. Parker, with the exception of a few specially drawn for me by Mr Edwin Wilson.

The work was, as I have said, begun in the earnest hope that some may be led thereby to follow the serious study of Variation, and so make sure a base for the attack on the problems of Evolution. Those who reject the particular inferences, positive and negative, here drawn from that study, must not in haste reject the method, for that is right beyond all question.

That the first result of the study is to bring confusion and vagueness into places where we had believed order established may to some be disappointing, but it is best we deceive ourselves no longer. That the problems of Natural History are not easy but very hard is a platitude in everybody's mouth. Yet in these days there are many who do not fear to speak of these things with certainty, with an ease and an assurance that in far simpler problems of chemistry or of physics would not be endured. For men of this stamp to solve difficulties may be easy, but to feel

difficulties is hard. Though the problem is all unsolved and the old questions stand unanswered, there are those who have taken on themselves the responsibility of giving to the ignorant, as a gospel, in the name of Science, the rough guesses of yesterday that tomorrow should forget. Truly they have put a sword in the hand of a child.

If the Study of Variation can serve no other end it may make us remember that we are still at the beginning, that the complexity of the problem of Specific Difference is hardly less now than it was when Darwin first shewed that Natural History is a problem and no vain riddle.

On the first page I have set in all reverence the most solemn enuntiation of that problem that our language knows. The priest and the poet have tried to solve it, each in his turn, and have failed. If the naturalist is to succeed he must go very slowly, making good each step. He must be content to work with the simplest cases, getting from them such truths as he can, learning to value partial truth though he cheat no one into mistaking it for absolute or universal truth; remembering the greatness of his calling, and taking heed that after him will come Time, that "author of authors," whose inseparable property it is ever more and more to discover the truth, who will not be deprived of his due.

St John's College, Cambridge.
29 *December*, 1893.

CONTENTS.

INTRODUCTION.

PART I.

MERISTIC VARIATION.

CORRIGENDA.

p. 23, line 5. For "and that in" read "and in."

p. 27, line 29. For "appear" read "appears."

p. 37, line 18. For "their" read "the."

p. 54. Note 2. For "xxviii" read "xx."

p. 55. *Parra* is now known not to have affinities with the Rallidæ.

p. 141. In description of Fig. 15 insert "After Solger."

p. 151, line 2 and p. 153, Note. For "W. B." read "G. B."

p. 198. For "Pinnipediæ" read "Pinnipedia." For "*Dent.*" read "*Deut.*"

p. 212. In description of Fig. 40 delete "p^1 of the left side is in symmetry with two teeth on the right side." The figure is correct.

p. 281, 15th line from bottom. Delete "and perhaps all."

p. 382. For "W. H. Benham" read "W. B. Benham."

p. 429. For "Banyul's" read "Banyuls."

p. 473, 4th and 6th lines from bottom. For "*Tornaria*" read "*Balanoglossus.*"

p. 526. Delete the heading "(1) *Clear cases of Extra Parts in Secondary Symmetry.*"

Note to p. 461, Note 718. As to union of eyes in Bees, see further, Dittrich, *Zeit. f. Ent.*, Breslau, 1891, xvi. p. 21, and Cook, A. J., *Proc. Amer. Ass.*, 1891, p. 327.

Note to p. 468, Note 2. In connexion with Giard's observation the following fact should be given. Since this Chapter was printed I have had an opportunity of examining a sample of Flounders taken in the shallow water off Bournemouth. Of 23 specimens seen alive, all but about half a dozen were more or less blotched with shades of brown on the "blind" side. In five the brown was more extensive than the white. The eyes and dorsal fins were normal. The fishmonger who shewed them to me said that the Flounders in that place were generally thus blotched, and that those seen were a fair sample. In estimating the significance of Cunningham's experiment (p. 467) this fact should be remembered.