scopic photograph of a microscopic object." Funk and Wagnalls dictionary says, "Photomicrography is the art or process of making photomicrographs: opposed to microphotography."

Webster and Oxford give definitions for the two terms similar to those above quoted, but, unfortunately, give the other word as a second choice in both cases. Oxford, however, quotes Sutton and Dawson, "Dictionary of Photography," "Microphotography . . . is now used to designate the reduction of negatives to very minute size, and serves to distinguish it from the process denominated photomicrography."

Obviously to me, "photomicrographs" is the correct term to use for the numerous reproductions appearing in current scientific literature and advertisements, of all manner of photographs taken through microscopes. I deplore such misuse as is evidenced in the *Scientific Monthly,* September, 1928, page 209 (the same article uses the term "microorganism"); in SCIENCE, advertisements in various 1928 numbers; in *Industrial and Engineering Chemistry,* volume 20, number 10, advertisement on page 62; and in other places, the exact references to which I have forgotten. In the *Scientific Monthly* referred to, the misuse of "micro" as a prefix is carried to "microcinematographic photographs." May I mention a paper by R. B. Harvey and myself (*Phytopathology,* volume 11, number 3) in which the perfectly good and logical, though somewhat long word, "cinematophotomicrography," is used?

It would seem that "custom" has already permitted the misusage indicated. I protest. I wonder if it will do any good.

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## WHEN IS NORMAL NORMAL?

MUCH has been written about the concept of normality, especially in statistical and educational literature, but the terms "normal" and "abnormal" are commonly used both in those fields and in general biological terminology to denote approach to or deviation from the usual or average, without qualification as to whether they refer to the medium considered or to the causative factors involved. For this reason entirely normal reactors are frequently described as "abnormal," when in reality only the causative factors deviate from the average, and contrariwise abnormal reactors are described as "normal" because they have not shown "normal" responses to abnormal conditions.

Examples of this could be taken from almost any field of biology, but consider the case of an originally normal child whose experiences have caused it to develop certain inhibitions and behave quite differently from other children. In such a case the deviation of this child's behavior from the average behavior of children of his class is accepted as a measure of his abnormality. Suppose, though, that practically all average children when subjected to the same or similar experiences react in the same or in a similar manner. Then this child and his behavior are entirely normal when considered in the light of his past experiences, and it is only his experiences which are abnormal. Furthermore, if this child remained unaffected by the abnormal conditions he had experienced and which it had been shown would bring about a new type of behavior with average children, then, though still behaving like normal children without the same experiential background, he would be abnormal because he had not been normally affected by his unusual environment.

The same principle applies equally well, it seems to me, whether the unusual growth or other function of a tissue or organism or any other similar biological phenomenon is being considered. The medium itself may be abnormal and demonstrate appropriate abnormal behavior; again it may be entirely normal but attract attention by its response to abnormal causative factors.

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## SCIENTIFIC BOOKS

*Scientific papers of William Bateson.* Edited by R. C. PUNNETT. 2 vols. Illustrated. The Macmillan Co., N. Y.

THESE two beautifully printed volumes from the Cambridge (England) University Press contain the collected scientific papers of Bateson reprinted from various journals and books. An account of his life and work by Mrs. Bateson and his more popular writings have already been published elsewhere. These volumes contain the record of his work, as he published it from time to time, conveniently brought together in one place. Like all "collected papers," many of these have now only a historical value. They have had their effect on contemporaneous scientific thought and investigation and are chiefly valuable now for the unified picture which they present of the achievements of one of the leading scientists of our time. To a student of the history of biological science in one of its periods of most rapid progress they will be of great value.

In order to understand what these papers are about and why they were written, one should have in his mind's eye the scientific background against which they were projected. Of the two volumes, the first belongs to the pre-Mendelian period in which the nature and causes of variation were Bateson's objective; in the second volume, heredity in the light of Mendel's principles is the objective.

The earliest paper in these volumes dates from 1886. At that time Darwin's evolution theory had been generally accepted by scientists. It was conceded that different groups of animals had diverged from each other in the process of descent from a common ancestor, and efforts were being made to trace these lines of descent. Bateson's paper deals with the ancestry of the Chordata, which by arguments based on comparative morphology and embryology then much in vogue he seeks to connect with the imperfectly known group of the enteropneusta as represented in Balanoglossus. Bateson himself in common with biologists generally came later to place less confidence in theoretical lines of descent based on morphological and embryological resemblances. This study interested him in the subject of serially repeated parts, so conspicuous in the organization of chordates, a subject which comes in for more intensive and extended study in his book of 1894, "Materials for the Study of Variation."

Bateson next took up the study of variations in the form and texture of the shell of a mollusk, *Cardium edule,* "apparently correlated to the conditions of life" (1889). This investigation led to extensive travels in quest of material in the district of the Aral Sea and in Egypt. He finds by a study of shells from successive terraces of the Aral Sea that increasing salinity of the water is attended by marked changes in the character of the shell and concludes cautiously that these changes are probably a direct consequence of changes in the environment. But this does not lead him to adopt a Lamarckian explanation of evolution in general. He recognizes even in his shells the probably simultaneous action of environment and natural selection in producing racial changes. A variety of minor investigations upon the sense organs and perceptions of marine animals next engages Bateson's attention. These are recorded in the *Journal* of the Marine Biological Association in 1890. In the same year he publishes a paper on some cases of abnormal repetition of parts in crustacea and fishes, the same general subject which had been taken up in his first paper (1886) and which was to receive more extended treatment later. In the following year (1891) Bateson makes a study of "variations in floral symmetry of certain plants having irregular corollas." The underlying idea in his studies of variation throughout this period is expressed in this sentence:

> In proportion as the process of evolution shall be found to be discontinuous the necessity for supposing each structure to have been gradually modeled under the influence of natural selection is lessened, and a way is suggested in which it may be possible to escape from one cardinal difficulty in the comprehension of evolution by natural selection.

For his next subject (1892) in the study of variation, Bateson turns to insects, and investigates the relation between color of the cocoon in certain moths and that of the substratum, concerning which Poulton had described a protective resemblance. He was able to show that no such relation exists and that consequently protective resemblances as a factor in natural selection had probably been overestimated in Poulton's writings. But in the case of the color of larvae of the pepper moth (*Amphidasys betularia*) he verified the observations of Poulton that a protective resemblance to the environment does exist.

In the same year (1892) he published a paper on numerical variation in teeth, one of the parts of a projected general work on variation. As an outgrowth of this study he proposes a modification in the current idea of homology.

> The received view of homology supposes that a varying form is derived from the normal much as a man might make a wax model of the variety from a wax model of the type, by small additions to and subtractions from, the several parts. . . . But the natural process differs in one great essential from this. For in nature the body of the varying form has never *been* the body of its parent . . . but in each case the body of the offspring is made again from the beginning, just as if the wax model had gone back into the melting-pot before the new model was begun.

Another paper of this period (1892) describes some cases of variation in secondary sexual characters of insects, which statistically treated is thought to be dimorphic or polymorphic. The only one of these cases which in the light of our present knowledge would seem to be beyond question is that of the earwig (Forficula) with "low males" and "high males," which are of very different body-size and proportions, in particular with a striking difference in the length of the anal forceps. These differences have, however, since been shown in all probability to be a consequence of the intestinal fauna and flora of the insect, rather than of its genetic characteristics.

In two letters to *Nature* he attacks successfully some supposed cases of "aggressive mimicry" between flies and bumblebees. This is an incident merely in the general assault which he was preparing on the orthodox view of evolution as operating on continuous variation subjected to the uninterrupted action of natural selection.

The year 1894 saw this general offensive launched in the extensive work now out of print, "Materials for the Study of Variation." The preface and introduction of this book are reprinted, which suffice to show the motive, methods and major conclusions of Bateson's work up to this time.

He recognizes the futility of mere speculation as to the course of evolution such as he himself regretfully adopted in his paper on the ancestry of the Chordata. He is *through* with all such methods.

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 the 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.

Such a catalog Bateson attempts to supply in the body of his work. The last page of Bateson's preface is worth quoting entire since it applies with added emphasis to the study of problems of evolution and to discussions of evolution which are carried on in our own time by methods so varied and with conclusions so contradictory.

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 problem 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 the 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 showed that Natural History is a problem and no vain riddle.

On the first page I have set in all reverence the most solemn enunciation of that problem that our language knows. [All flesh is not the same flesh: but there is one kind of flesh of men, another flesh of beasts, another of fishes, and another of birds.] 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.

The underlying thought in Bateson's introduction is that specific groups are discontinuous whereas the environment is continuous. He suspects that variations which lead to the formation of species are discontinuous in nature, and catalogs in the body of his book evidence pointing in this direction. Galton had expressed a similar view, though his major attention had been given to the study of continuous variation, and after his death the Galton Laboratory had given exclusive attention to variation of this sort with the idea that it alone was responsible for evolution. Bateson says (p. 235):

To sum up, the first question which the study of variation may be expected to answer relates to the origin of that discontinuity of which species is the objective expression. Such discontinuity is not in the environment; may it not, then, be in the living thing itself?

The study of variation thus offers a means whereby we may hope to see the processes of evolution. We know much of what these processes *may* be. The deductive method has been tried with what success we know. It is time now to try if these things can not be seen as they are, and this is what variation may show us. In variation we look to see evolution rolling out before our eyes. In this we may fail wholly and must fail largely, but it is still the best chance left.

The consensus of biological opinion thirty-five years after the publication of Bateson's book would probably be that his effort had "failed largely," as he anticipated that it would, but that it had important consequences in leading to a fact-finding rather than a speculative study of evolution. Six years later De Vries began the publication of his mutation theory, Mendel's law was rediscovered, and a few years later still Johannsen's pure line and genotype concepts were made public, all being attempts to discover what the facts were about evolution as observed in progress at a particular time and place.

In the meantime Bateson himself was busy with the study of new cases of discontinuity in variation such as the color variation of flat-fishes (1894), of Chrysomelid beetles (1895), of webbed feet in Antwerp pigeons (1896), of meristic variation in crustacea (1900) and of melanism in moths (1900). And his studies are extended to successive generations so as to cover the inheritance of variations. He writes a general article for *Science Progress* (1897) on "Progress in the Study of Variation" [including heredity]. He had published in 1895 an investigation of the origin of the cultivated cineraria, which he shows to have been through hybridization of several wild species. In 1900 he discusses the inheritance of variation in the corolla of *Veronica Buxbaumii.* Two controversial papers in which he attacks sharply some of the methods and conclusions of the biometric school in the study of variation conclude Volume 1.

Volume 2 includes the papers on heredity and related subjects published by Bateson after the rediscovery of Mendel's law in 1900. This discovery furnished the key-note of all Bateson's subsequent work. He at last had found what up to this time he had been looking for. Mendel's law explained how discontinuous variations were perpetuated and why they were not swamped by crossing. He could now, with the aid of this law, not only see evolution at work, but also control its processes. This with a body of enthusiastic colleagues he now set about doing.

The first paper in Volume 2 is an introduction to an English translation of Mendel's brief but momentous paper published in 1866. The second paper is an exposition of Mendel's principles of heredity reprinted from a book of similar title published in 1902. This was an epoch-marking work which to most English-speaking readers brought the first information that a new day had dawned in the study of evolution. This message it brought in no uncertain terms. The complete failure as a generalized statement of facts of the Galton-Pearson law of ancestral heredity was pointed out, and in contrast it was shown that Mendel's law is a valid and easily verifiable principle governing the transmission of discontinuous genetic characters. The question was raised whether in last analysis all heritable variations would not be found to be discontinuous in character and subject to Mendel's law in transmission.

Next comes a partial reprint of the famous "Reports to the Evolution Committee" of the Royal Society on "The Facts of Heredity in the Light of Mendel's Discovery." In a footnote is suggested the now generally accepted terminology of generations concerned in a Mendelian cross, $P_1$ (parental), $F_1$, $F_2$, etc. (filial).

A paper on the present state of knowledge of color heredity in mice and rats (1903) summarizes the experiments made with these animals previous to and since the rediscovery of Mendel's law and shows that all are consistently Mendelian, notwithstanding the persistently maintained opposite view of the biometric school.

Later contributions deal with the inheritance of heterostylism in Primula (1905), walnut comb in fowls (1905), flower color in sweet peas and stocks (1906). An address before the International Zoological Congress held in Boston in 1907 deals with "Facts Limiting the Theory of Heredity." A paper on "The Heredity of Sex" (1908) deals with sex-linked inheritance in Abraxas first described in 1906 by Doncaster and Raynor. In the same year, "Reports to the Evolution Committee" describe experiments with poultry, sweet peas and stocks.

Subsequent papers are reprinted chiefly from the *Journal of Genetics* established by Bateson and Punnett in 1911. Their substance is known to most students of genetics. Comprehensive experiments with various plants and animals serve to extend the Mendelian principles or show their limitations. The reduplication hypothesis to explain the earlier discovered facts of coupling and repulsion is launched (1911) but partially withdrawn some years later in favor of Morgan's chromosome theory, in a paper on the "Genetics of *Primula sinensis*" (1923). The subjects of somatic segregation, of chimeras, of root-cuttings receive repeated attention. One is, in fact, amazed at the fruitfulness and the fundamental value of Bateson's investigations. A number of reviews of current publications on genetics and evolution complete Volume 2.

W. E. CASTLE

## THE AMERICAN GEOPHYSICAL UNION

THE tenth annual meetings of the American Geophysical Union and of its sections will be held in the National Academy and Research Council Building, Washington, D. C., on April 25 and 26, 1929. Following the business meeting of the general assembly of the union on the afternoon of April 26, the union will hear the five following general-interest papers presented by the Section of Oceanography, these all being either regarding work in progress or relating to work recently completed: "The Expedition of the Submarine *S-21* to the Caribbean Sea and Gulf of Mexico," by C. S. Freeman; "Oceanography and the Fisheries," by Henry B. Bigelow; "The International Ice Patrol, with Special Reference to its Economic Aspects," by Edward H. Smith; "The Cooperative