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WILLIAM BATESON

With the recovery of Mendel's paper a new era in the study of heredity began. Bateson at once became a recognized leader in the new movement. His earlier work on variation had supplied him with a wealth of material that only waited the clue that Mendel's theory afforded, and his experimental work on discontinuous variations, that had already started before 1900, had prepared him for the acceptance and realization of the profound significance of the new theory.

The ardor with which Bateson undertook to apply, test and extend Mendel's discovery, the keenness that he brought to bear on the new work, and the complete frankness with which he discussed "unconformable cases" had a wide influence on the rapidly growing school of genetics.

He did not try to hide his contempt for second-rate work, and he was unsparing in the exposure of the pretensions of those who were satisfied with lower standards. This sometimes led to acrimonious rejoinders, but it put fear into the hearts of those who continued to use an outworn phraseology of variation and heredity that no longer had any real significance. He hit and he hit hard! If he disagreed he said so flatly, and could always give a cogent argument for his disagreement. His intellectual rectitude was beyond all praise and recognized by friend and foe alike. His courtesy and hospitality were unfailing, and he will be missed by a host of admirers, and regretted, I can not but think, by those of his opponents who found him a foeman worthy of their steel.

His own work extended the fundamental principles of genetics in many directions. The more difficult the problem the more it attracted him if it offered an opportunity for exact experimental investigation. The perseverance with which he followed every clue —"treasure your exceptions," he said—and the high standards of work that he insisted on for himself as well as for others made a deep impression on his colleagues. His death came suddenly in the midst of his labors, and students of genetics the world over have felt deeply the loss of a friend of outstanding intellect and commanding personality.

Bateson's first important contribution dealt with material collected in this country in 1883. He had seen an announcement in the Johns Hopkins University circular that Balanoglossus had been found at the Marine Station, then situated at Hampton, Virginia, and wrote to Brooks asking permission to come to the station to work on this rare and extraordinary worm. "Brooks sent me a cordial invitation to come over and try. Such leave was no little thing to give, for Balanoglossus must have been known to be one of the prizes of the station, but in professional generosity Brooks was royal and lavish." The friendly relation between Brooks and his students that had so much to do with his influence over them was soon established with Bateson. At the time Brooks was absorbed in writing his treatise on heredity. Bateson wrote later (1910): "For myself I know it was through Brooks that I first came to realize the problems which for years have been my chief interest and concern." "Variation and heredity with us had stood as axioms. For Brooks they were problems. As he talked of them the insistence of these problems became imminent and impressive."

The material collected at Hampton and in the following year at Beaufort, N. C., led to papers on the early stages of development and on the morphology of the adult worm. In a later paper on "The Ancestry of the Chordata" Bateson discussed, in guarded terms, the position of Balanoglossus in relation to the vertebrates, reaching the conclusion that the structural resemblances indicated relationship and that the unsegmented nature of the notochord and central nerve cord indicated that the ancestor was not segmented, and that the repetition seen in the body cavities and gill-slits must have had an independent origin. This question of repetition haunted Bateson for the rest of his life. His later conclusion is interesting.

> The meaning of cases of complex repetition will not be found in the search for an ancestral form which, itself presenting the same character, may be twisted into a representation of its supposed descendant. Such forms there may be, but in finding them the real problem is not even resolved a single stage; for from whence was their repetition derived? The answer to the question can only come in a fuller understanding of the laws of growth and of variation which are as yet merely terms.

At the present time, forty-three years later, this statement may still stand word for word.

In 1894 appeared "The Materials for a Study of Variation" which has recently been called Bateson's most important work. Here he brought together a great number of widely scattered cases bearing on discontinuity in variation. It is the particular use that Bateson made of this evidence that is the most interesting feature of the book. He argued that since evidence for discontinuity is to be found everywhere in animals and plants, evolution through natural selection, which he interpreted to mean by the selection of continuous variation, will not account for the origin of species. This relationship of variation to species-formation was a problem that interested Bateson intensely. He recurs to it over and over again in his later writings.

This book on discontinuity in variation appeared six years before de Vries's mutation theory, in which discontinuity in inheritance is the central theme, but Bateson seems never to have become convinced that the discontinuity shown by de Vries's mutants in Oenothera furnishes the sort of evidence for discontinuity which he himself appealed to as supplying the materials for evolution.

In the preface to "The Materials" Bateson says, referring to his earlier discussion of the phylogeny of the vertebrates, "over it all hung the suspicion that the then current morphological arguments and interpretations might not be sound." In these discussions we are continually stopped by such phrases as "if such and such a variation then took place and was favorable." Again, "the whole argument is based on such assumptions as these—assumptions which, were they found in Paley or Butler, we could not too scornfully ridicule." Bateson set himself, therefore, the task of collecting and codifying the facts of variation as "the first duty of the naturalist." He brought together a great body of evidence from the literature and from this he reached the conclusion that the *forms* of living things taken at a given moment show a discontinuous series and not a continuous series. He also argued that the forms of living things may be separated into specific groups or species, "the members of each such group being nearly alike, while they are less like the members of any other group." Assuming that the doctrine of descent is true in the main because of the difficulty of forming any alternative hypothesis as good, he then examined the theory of natural selection in the light of these conclusions. On the theory of natural selection "specific diversity of form is consequent upon diversity of environment and diversity of environment is thus the ultimate measure of diversity of specific form. But "diverse environments often shade into each other insensibly and form a continuous series, whereas the specific forms of life which are subject to them on the whole form a discontinuous series." The magnification of this difficulty furnishes the basis of Bateson's critical attitude towards Darwin's theory.

He points out that while the study of the adaptation of living things was undertaken as a test of the theory of natural selection its study ceases to help us at the exact point at which help is most needed. "We are seeking for the cause of the differences between species and species and it is precisely on the utility of specific differences that the students of adaptation are silent. For, as Darwin and many others have often pointed out, the characters which visibly differentiate species are not as a rule capital facts in the constitution of vital organs but more often they are just those features which seem to us useless and trivial . . ." "In the early days of the theory of natural selection it was hoped that with searching the direct utility of such small differences would be found, but time has been running now and the hope is unfulfilled." "Hence though the study of adaptation will always remain a fascinating branch of natural history it is not and can not be a means of directly solving the origin of species."

Bateson's general conclusion is summed up in the statement "that the discontinuity of which species is an expression has its origin not in the environment nor in any phenomenon of adaptation but in the intrinsic nature of organisms themselves manifested in the original discontinuity of variation." "The discontinuity of species results from the discontinuity of variation."

When in 1900 Mendel's paper (1865) was brought to light and confirmed by the results of de Vries, Correns and Tschermak, Bateson at once realized its importance. He was at the time himself engaged in a study of the inheritance of discontinuous variation and had become familiar with evidence that falls into line with Mendel's interpretation. He republished (1902) the English translation of Mendel's paper that had been prepared by the *Journal* of the Royal Horticultural Society (1900), and emphasized its far-reaching application. In collaboration with Miss Saunders, Bateson sent in his first report of the work to the evolution committee of the Royal Society (Dec. 17, 1901), which was published in 1902. In this report experiments of Miss Saunders with plants (Lychnis, Datura and Matthiola) and Bateson's with poultry furnished an admirable verification of "Mendel's Law" and served as a sufficient reply in themselves to an inadequate and prejudiced critique of Mendel's results that had appeared in *Biometrica.* As I have said, in the first edition of the "Principles" in 1902, Bateson took up the cudgels in defence of Mendel's work. His vigorous onslaught (based on direct familiarity with the facts in the case) on Weldon's misleading review of Mendel's work made it impossible that the importance of the new discovery should be overlooked or disregarded. "The study of variation and heredity *must* be built on statistical data, as Mendel knew long ago; but as he also perceived the ground must be prepared by specific experiment. The phenomena of heredity and variation are specific, and give loose and deceptive answers to any but specific questions. That is where our *exact* science will begin." In our sparse and apathetic community error mostly grows unheeded choking truth. That fate must not befall Mendel now."

Between the years 1902 and 1909 further reports to the evolution committee were made by Bateson and his collaborators. A large amount of exact information concerning heredity over a wide range of animals and plants appears in these reports. They have also a special interest to students of genetics. Each stage in the progress of the work that Bateson and his collaborators were carrying out at Cambridge is here set down. The reports give an insight both into the methods undertaken to study the problems and into the origin of some of the ideas at which Bateson later arrived. It is difficult to pick out any one subject as more important than another, but the work on stocks by Miss Saunders, the work of Hurst and of Bateson and Punnett on the inheritance of the shape of the comb and color of the plumage in poultry, the work on sweet peas by Bateson and Punnett contributed many important facts to the study of genetics. The explanation of the reversion that occurs when certain white races of peas are crossed, taken in connection with Cuénot's analysis of the relation of recessive whites to color determiners in mice, and the discovery of coupling and repulsion of certain characters in sweet peas (1900) (now more familiarly known as linkage) are two of the outstanding results that have had important developments in the extension of Mendelism. But in such an abundance of material it is difficult to select the more significant parts. One feature of these reports is characteristic. Nothing is glossed over for the sake of uniformity. Exceptions are reported and emphasized. Their examination whenever possible is the starting point for further study that is often illuminating. In a summary of genetic work up to 1906 (Progr. Rei. Botan.) Bateson made the following significant comment . . . "it is practically impossible to make any general statement as to which characters are dominant and which are recessive . . . It may be suggested that in the dominant type some element is present which is absent in the recessive type. The difficulty in applying such a generalization lies in the fact that not very rarely characters dominate which appear to us to be negative." As examples, the dominance of hornless cattle and of the abortive condition of the female organ in the lateral florets of barley are given. "Consequently we are almost precluded from regarding dominance as merely due to the presence of a factor which is absent in the recessive form. Not impossibly we may have to regard such negative characters as due to the presence of some inhibiting influence but in our present stage of knowledge there is no certain warrant for such an interpretation." This reserved attitude Bateson always held, returning to a discussion of it in a paper that appeared (*Jour. Genetics,* 1926) shortly after his death.

In 1913 the greatly enlarged second edition of the "Principles" appeared, summarizing Bateson's own work and that of the Cambridge School as well as that published elsewhere. This book has been for many years the reference book for students of genetics.

In the Silliman lectures given at Yale University in 1907 (published in 1913) some of the more general problems of biology were discussed in the light of the new discoveries in genetics. Bateson writes:

> On attempting a more general discussion of the bearing of the phenomena on the theory of evolution I found myself continually hindered by the consciousness that such treatment is premature and by doubt whether it were not better that the debate should for the present stand indefinitely adjourned. That species have come into existence by an evolutionary process no one seriously doubts; but few who are familiar with the facts that genetic research has revealed are now inclined to speculate as to the manner by which the process has been accomplished.

These lectures carry the reader far afield. From beginning to end the problem of species is under examination. Bateson's first-hand information relating to a wide range of subjects is nowhere more manifest than in these lectures. The difficulty of explaining the origin of species through the survival of better adapted individuals is again subjected to drastic criticism mainly on the ground that the distinctiveness of species—a view towards which Bateson strongly inclined—bears no demonstrable relation to their fitness for the particular environment in which they live. This contention, which has long been a stumbling block to selectionists and is inherent in Darwinism taken literally, has seemed less significant to others who, following Darwin, do not find the distinctiveness of species as sharply marked as Bateson postulates, and who find no real difficulty in the absence of adaptive features in those characters chosen by systematists in defining species. For on the one hand it is recognized that the characters chosen are, for the most part, arbitrarily picked out because they are constant, hence are not necessarily the characters that furnish the basis for selection of the types in question, and on the other hand it is recognized that there is no serious difficulty in accounting for the constancy of these chosen specific characters provided they are recognized as by-products of physiological factors themselves of real importance for the welfare of the individual.

In the summer of 1914 the British Association met in Australia and Bateson gave the presidential address—on heredity. Here he goes over much of the ground covered by "The Materials" and "The Problems of Genetics" but develops farther in a tentative and speculative vein some of his earlier views. He was frankly skeptical concerning the nature of the elements or *factors* of Mendelian nomenclature which he thinks are sorted out amongst the offspring by a process of cell-division in an orderly fashion. "That they are in some way directly transmitted by the material of the ovum and of the spermatozoon is obvious, but it seems to me unlikely that they are in any simple or literal sense material particles. I suspect rather that their properties depend on some phenomenon of arrangement." Thus, in a guarded way, he definitely disassociated himself from the movement then already in full swing that identifies the genetic elements with the stable materials of the chromosomes. He also states that he is entirely skeptical as to the occurrence of segregation solely in the maturation of the germ-cells which he thinks "as almost decisively" negatived by the different factors carried by the male and female organs in certain plants. Later work, however, has shown that these differences find a reasonable explanation as the result of gametic lethals, although Bateson seems never to have appreciated the genetic evidence for this conclusion. Probably the most novel feature of the Australian address is the application of the presence and absence hypothesis to the possibilities of evolution through loss. He quite frankly points out where that view leads. If, he says, we *may* have to forego the claim that variations arise by the *addition* of "factors" and if, as he believes, the evidence favors the view that all known mutant changes are due to losses, it would seem to follow that if evolution is based on such variations it is due to degradation of the original germ-materials. Curiously enough, in the same summarization he suggests the possibility of fractionation of factors which would appear as a half way step but which possibly might equally well be interpreted as any kind of a change in a gene. The evidence adduced for fractionation is, however, based on changes in visible characters that can hardly in the light of recent development be claimed as a substantial argument. The highly speculative nature of these suggestions he fully realized and adds the caution: "I do not suggest that we should come to a judgment as to what is or is not probable in these respects. As I have said already, this is no time for devising theories of evolution and I propound none."

The second part of the address delivered at Sydney goes over much the same ground that Bateson had already covered in his Herbert Spencer lecture (1912). On these two occasions only, has Bateson ventured to express his views on the biological aspects of the structure of human society. In plain language he points out the appalling lack of conscious foresight in the preservation of the human race and the effect of the modern tendency to preserve the socially unfit.

He points to the immense variability in the human stock:

> How hard it is to believe the polymorphism of man . . . How few of these could have changed parts with each other. . . . In no wild species, not even among the ants do we find any polymorphism approaching to this. I never cease to marvel that the more divergent castes of civilized humanity are capable of inter-breeding and producing fertile offspring from their crosses. Nothing but this paradoxical fact prevents us from regarding many classes even of Englishmen as distinct species in the full sense of the term. . . . The problem that confronts the political philosopher is to find a system by which these differentiated elements may continue together to form a coordinated community while each element remains substantially contented with its lot.

Bateson's conviction that many of the observed physical traits that distinguish individuals trace back to genetic differences will be applauded by students of human heredity, and his contention that a mixed population may better serve the purposes of modern civilization than a homogeneous one, may not be contested, but there may still be grave doubt as to whether these observable differences play as important a rôle in the advance or retardation of a social group as do the traditional and economic influences that determine the behavior of the group as a whole. It is noticeable that the rigorous standard that he demands in others dealing with Darwin's theory of natural selection scarcely warrants some of the bold prophecies he makes concerning the future of the human race under present conditions. "The essential difference between the ideal of democracy and those which biological observation teaches us to be sound, is this: democracy regards class distinction as evil; we perceive it to be essential." Aside from the view as to what democracy is or is not, it must be questioned, I think, whether "biological observation" has anything authoritative to say on the matter, since the fabric out of which political systems are made and transmitted from one generation to another involves mental processes about which at present biological observation has little if anything that is worth while to contribute.

In 1910 Bateson accepted the directorship of the John Innes Horticultural Institution, which became a center of research in genetics. Here in collaboration with associates and students he turned out year after year a series of important papers dealing with several difficult problems in heredity. The more significant of these contributions deal with (1) the inheritance of "rogues" in peas whose peculiarities Bateson was inclined to believe could throw light on the problem of the time of segregation of characters; (2) the variation shown by root cuttings, probably a periclinal phenomenon; (3) the inheritance of double flowers and sex characters in begonias; (4) studies on variegation; and (5) on the genetics of Primula, etc. Bateson was attracted by these problems partly because they were puzzles, partly no doubt because they did not seem to conform to the then current methods of genetic analysis and might therefore open up new fields of adventure.

Bateson brought to his work an exceptionally wide and first-hand familiarity with plants and animals. He had also an extensive knowledge of the literature of his subject at command and an ability to express himself fearlessly in classical and clear English. His personal interests extended far beyond the immediate fields of his researches. His deep interest in painting and other forms of art must have surprised his scientific friends when they discovered it for the first time, and his artist friends would no doubt have been equally surprised to have discovered his far-reaching influence on the biological science of his time.

T. H. Morgan

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THE RELATION OF MAPPING TO MODERN CIVILIZATION[1]

When the pioneers, with their covered wagons, started west from Kansas City and other points in Missouri, they felt little need for maps except those of the crudest type. They had imagination or they would not have started forth; but like every other age, they did not see much beyond the present and could not visualize what was coming. Railroads, hydroelectric developments, growth of modern towns and cities were beyond their imagination. Accordingly, when the time came for establishing boundaries and making maps, they, like men of every other age, met the present needs and were little concerned about the future. The division of land was their first concern. As the land had little value, moderately correct surveys were considered quite sufficient. In many places the points were marked temporarily, rather than for permanent use, and were lost in a few years. One of the results has been a great deal of litigation, and in the case of lands which later became of great value, such as oil fields, the cost of litigation has undoubtedly been greater than good maps would have cost if made in the first place.

In the early work the magnetic compass was much used, and this fact has been blamed for a great deal of inaccuracy. Recent investigation has shown that in most cases it was quite as much the use of careless methods that was responsible for the defects of the survey. The magnetic compass is an easy instru-

[1] Presented before Section M, American Association for the Advancement of Science, Kansas City, December 30, 1925.