of the Geological Survey Director King prophesied for the United States a future annual output of mineral products having a value of a billion dollars, and that the present production is two and one half times that amount, it must be conceded that the desirability of the federal scientific investigations of these national resources is even greater than in 1880. "It is a most conservative statement," Director Smith says, "that at no date has the general public been in closer touch with the United States Geological Survey or made larger use of the published or unpublished results of its surveys and investigations than at the present time."

UNIVERSITY AND EDUCATIONAL NEWS

A BEQUEST of $3,000,000 to Oberlin College by Charles M. Hall, the distinguished electrochemist and manufacturer of aluminum, is announced. The bequest is in the form of $2,000,000 endowment to be used for any purpose, $500,000 to be used to build an auditorium, $100,000 for the auditorium's maintenance, $200,000 to be spent for campus improvements; all property in Oberlin owned by Dr. Hall, and a valuable art collection.

THE will of Miss Grace Hoadley Dodge, for many years known for her educational and philanthropic activities in New York City, contains bequests of $1,400,000 for educational and charitable purposes, as well as a number of deferred bequests of the same character. The sum of $500,000 is bequeathed to Teachers College, Columbia University, in the founding and conduct of which she took an active part. The college will receive two deferred bequests, one of which may be large. To the National Board of the Y. W. C. A. the sum of $500,000 is left, and to the Y. W. C. A. of New York City, $200,000.

AT the meeting of the corporation of Harvard University on December 28, it was voted to establish a separate faculty for the Bussey Institution. The vote was consented to by the board of overseers, and the new body at present includes the following members: W. M. Wheeler, Ph.D.; W. E. Castle, Ph.D.; R. T. Fisher, A.B., M.F.; E. M. East, Ph.D.; C. T. Brues, S.M.; I. W. Bailey, A.B., M.F., and C. C. Little, S.D., of the Bussey Institution; G. H. Parker, S.D. and W. J. V. Osterhout, Ph.D., of the faculty of arts and sciences; and E. E. Tyzzer, A.M., M.D., of the medical school.

DR. C. E. BURKE, lately of the University of California, has been appointed instructor in the department of chemistry at the University of Vermont.

DR. HOWARD D. HASKINS, formerly associate professor of bio-chemistry in the school of medicine of Western Reserve University, has been appointed professor of bio-chemistry in the medical department of the University of Oregon.

DR. FREDERICK D. HEALD, of Philadelphia, has been appointed professor of plant pathology and pathologist, Washington State College and Experiment Station, Pullman, Washington.

DISCUSSION AND CORRESPONDENCE

BATESON'S ADDRESS, MENDELISM AND MUTATION

IN Bateson's thoughtful and stimulating address,[1] a recognized authority on evolution attempts to summarize for us recent progress in the study of that subject by analytical methods. It would be well for all engaged in some particular branch of this subject to attempt thus to lift the eyes from the scene of their individual labors and survey from time to time the entire field. An indispensable sense of proportion and perspective is thus gained. This is my excuse for commenting briefly on some of Bateson's fruitful ideas.

That evolution occurs all biologists agree. That the organisms now existing on this earth are different from those which formerly existed here no one questions. But we are still ignorant of how they came to be different. The geological record indicates that the change was gradual. The supposed ancestors of the horse,

[1] Bateson, W., Address of the President of the British Association for the Advancement of Science, SCIENCE, N. S., 40, pp. 287–302, August 28, 1914.

for example, are less and less like modern horses the more remote in geological time are the deposits in which their bones are found. But students of evolution differ in their ideas as to how gradual the progress of evolution has been and is, for no one supposes the process ended.

Within my view stands the sloping bank of a reservoir, which most visitors ascend by a flight of granite steps; but children often go up the grassy bank wherever they happen to encounter it. Either method takes one to the top, the gradual or the stepwise mode of ascent.

Evolution was thought by Darwin to occur in two ways comparable with these, the gradual and the stepwise. From Darwin's writings it would seem that he regarded the gradual as the more common and important method of evolutionary change among organisms, but it is clear that he recognized stepwise or "sport" variation as of considerable value, particularly in the production of new varieties under domestication.

But not many years after Darwin's death a question arose in the minds of certain thoughtful naturalists as to whether Darwin had rightly estimated the relative importance of these two methods of evolution. Galton, Bateson and DeVries have laid increasing emphasis on sport variations or "mutations," until these have come to be regarded by many as of overshadowing importance in evolution. The full-fledged mutation theory[2] maintains that evolution occurs *by steps alone,* that is that new species arise from old ones by single discontinuous steps, never by gradual uninterrupted change. This theory has been the guiding principle in evolutionary study in recent years. Its basic idea is that natural species are invariably discontinuous and that intergrades between them do not occur except possibly as the result of sporadic hybridization, such intermediate forms being unstable and so without significance. The attempt by Bateson[3] to classify the variations which occur within species led him to the conclusion that only such variations as are discontinuous in nature can have species-forming value, since they alone are not "swamped by crossing." This idea has been supported by the observation that among species regularly dimorphic or polymorphic, the several forms which remain distinct, notwithstanding constant intercrossing, are Mendelian alternatives, conforming with the laws of dominance and segregation. Many of the striking variations in color and form which occur among domesticated animals and plants follow these same laws so that their rediscovery and verification in 1900 was rightly regarded as strong presumptive proof of discontinuity in evolution. At about the same time DeVries brought together in his book entitled "The Mutation-Theory" a large amount of evidence favoring the idea of discontinuity in evolution most important of which was the repeatedly verified polymorphism of the seedlings produced by Lamarck's evening-primrose.

Mendelian segregation, however, does not at present offer a sufficient explanation of mutation in the evening-primroses so that provisionally we are forced to conclude with Gates[4] that mutation and segregation following hybridization are probably distinct phenomena. It also remains doubtful whether the phenomena observed among evening-primroses occur at all commonly among other plants or among animals. The so-called "mutations" which Morgan has observed in the fly *Drosophila* are certainly not of this order, but are clearly due to Mendelian factorial variation. Many with Bateson think that Mendelism affords a basis for the explanation of all evolution and confidently expect the evening-primroses sooner or later to be shown conformable with its fundamental ideas. In the

[2] The term "mutation theory" is here used in its widest sense, including not merely the ideas of De Vries concerning evolution among the evening primroses, but the general idea of discontinuity in the origin of species previously outlined by Galton and Bateson.

[3] "Materials for the Study of Variation," 1894.

[4] Gates, R. R., "Breeding Experiments which Show that Hybridization and Mutation are Independent Phenomena," *Zeits. f. ind. Abst. u. Vererbungslehre,* 11, pp. 209–279, 1914.

latest statement of his views, Bateson argues substantially as follows: Variations may be large or small. Those which are small are either not inherited or are of no consequence, being "slight differences that systematists would disregard." But large differences can not arise "by accumulation of small differences." Hence only large differences have evolutionary significance. In his own words:[5]

> We have done with the idea that Darwin came latterly to favor, that large differences can arise by accumulation of small differences. Such small differences are often mere ephemeral effects of conditions of life, and as such are not transmissible; but even small differences, when truly genetic, are factorial like the larger ones, and there is not the slightest reason for supposing that they are capable of summation.

Whether we may properly regard small differences as capable of "summation" depends upon what we mean by summation. Phillips and I[6] have shown that in the case of piebald rats the areas of white fur characterizing the race may be either increased or decreased at will and that the change takes place gradually, progressing steadily generation after generation and far transgressing the original limits of variation. The same is undoubtedly true of similar variegated patterns which mendelize among both animals and plants. Small differences which have arisen spontaneously have certainly been *aggregated* in this case. But crossing of the modified races shows that the aggregated changes have not been summated to such an extent that they constitute a single Mendelian factor, except in one case, where it seems quite possible that something of this sort has occurred. I am by no means ready to regard summation out of the question, whether by that we mean mere aggregation or fusion into a new Mendelian unit.

Bateson has further expressed the view that evolution has occurred largely, if not exclusively, by loss of Mendelian factors resulting in striking variations that breed true from their first appearance and thus render the parent species dimorphic or polymorphic. That many varieties of domesticated animals and cultivated plants have originated in this way will be admitted by any one who has studied them genetically. Darwin himself pointed out the importance of "sport" variation in producing new varieties of animals and plants under man's care and supervision and it is known that similar variations occur in wild species. But it is doubtful whether in a wild species a sport originating in this way has ever replaced the original form. Under domestication it is only the constant interposition of man that keeps the favored sports alive. Whether sport variation has had any part in the evolution of species is accordingly very doubtful. If we compare one wild species with another, we commonly find existing between them not single striking differences but numberless minute differences. Systematists usually name as diagnostic characters a few of the more striking differences, ignoring, as they are quite warranted in doing, all minor ones, the enumeration of which is for their purpose superfluous. But if one makes an intensive study of related species he finds that they differ in endless details of structure and physiological behavior extending even to differences in size of the constituent cells of the body (Conklin), or of their parts (chromosomes, chromomeres, etc.). During recent years, as the discrimination of species has become more keen, it has taken on more and more a quantitative expression. Series of specimens are measured, and specific distinctions are based on absolute and relative dimensions of parts, not on the "presence or absence" of large striking features of organization. It is easy to dispose of the work of the systematist by assuming that he does not know his business, but is it wise to do so? For other lines of evidence also indicate that the differences between species are quantitative and increase with genealogical divergence. This for example is the conclusion reached from such distinct methods of study as the examination of the forms of hemoglobin crystals in the blood of various species of animals and of the precipitin reaction of the blood.

[5] *L. c.*, p. 285.

[6] Publ. No. 195, Carnegie Institution of Washington, 1914.

But quantitative differences such as distinguish species are precisely those which do *not* Mendelize in crosses. Bateson says (p. 291):

> Of the descent of quantitative characters we still know practically nothing.

By which he probably means that we know *nothing Mendelian,* since in this address he treats Mendelism as the all-sufficient basis of evolution, and ignores a decade of intensive work in America directed toward the discovery of Mendelian factors as a basis for quantitative differences, a work participated in by many different workers, all favorably disposed toward the idea, but all unavailing. For the uniform result of a cross which involves quantitative differences is the production of an intermediate, which in turn produces intermediates only slightly more variable than the races originally crossed. Dimorphism or sharply discontinuous polymorphism is regularly wanting after size crosses. This is a fatal objection to the idea that specific differences are discontinuous in origin. One who advocates this idea has no choice at present but to ignore (as Bateson does) all evidence derived from the experimental study of this subject.

The idea that large differences can not arise by summation of small ones is rendered improbable by this evidence. For if the larger (quantitative) differences can so readily be broken down into smaller ones, it seems highly probable that the process is reversible. Indeed the experience of breeders shows that it is. The dog-breeder alluded to by Bateson who titrated his colored fluids to illustrate blood dilution in crosses was, so far as quantitative characters are concerned, employing a very apt method, notwithstanding Bateson's disapproval of it.

Even sport variations, which truly Mendelize, and which form the basis of color varieties and other fancy varieties among animals and plants, even these are capable of secondary break-down or "fractionation," as Bateson admits. In making this admission he differs from the supporters of the pure-line theory who conceive that a Mendelian factor is incapable of change, but who apparently hold the idea as an article of faith rather than one requiring proof.

Secondary break-down or modification of Mendelian factors is, however, coming to be so generally recognized that a special name is now applied to its products, that of multiple allelomorphs. Even those who hold to the conception of "pure lines" now recognize that the same sport variation ("mutation" or "locus") may assume several different conditions which viewed quantitatively form a graded series; but they insist on the *discontinuity* of the grades or forms which a Mendelizing character may assume, maintaining on *a priori* grounds that these stages can not be bridged. The perilousness of such a position is apparent from a single well-known case. The first discovered Mendelizing character in animals was albinism and it is one of the simplest and clearest cut of all Mendelian characters thus far discovered. It was not to be expected that the single step between a wholly uncolored and the normally colored condition would be or could be bridged. Yet two such intermediate stages have already been demonstrated, which are unmistakable allelomorphs of albinism, *i. e.,* which behave as alternative forms of the same genetic factor. If two such intermediate stages may arise, why may not others arise; why not a dozen, why not a thousand? Is it safe to assume that this is not possible?

Bateson urges that in cases of color variation such as that of the sweet-pea and the primrose the *large* changes came first and the smaller ones later by secondary break-down or "fractionation." The argument implies, indeed, he expressly claims, that large differences can not be built up from small ones. I do not believe that either paleontology or the history of breeding will support Bateson in this claim. On the former ground Osborn[7] holds to a gradual origin of discontinuous differences between organisms. A study of breeds of animals in comparison with their wild originals or present-day representatives shows

[7] Osborn, H. F., "The Continuous Origin of Certain Unit-characters as Observed by a Paleontoogist," *Amer. Nat.,* 46, pp. 185–278, 1912.

that variation has not occurred merely by large losses subsequently fractionated so as to form intermediates. Not merely intermediates arise but also those which transcend any known original sports. Original black races have become blacker; original yellow races have become yellower; white-spotted races have become more spotted still at the will of the breeder. Large races also become larger, and small races smaller, under the hands of the fancier. He does not limit himself to the production of intermediates.

To suggest further that all variation transcending limits previously existing is due to loss of inhibitors and so is really retrogressive is scarcely satisfying. It is a formal evasion of the difficulty but in no sense a solution of it. It belongs with the box-within-box idea of development. I agree with Bateson that variety formation within the higher animals and plants seems to be very frequently by a process of loss but I can not believe that this is the exclusive process concerned in the formation of new species or even of varieties. It needs but to carry the idea to its logical conclusion to show its absurdity. Is man merely an amœba simplified by loss of inhibitors? I can not believe so. I can not believe that the original proteid molecule has since its original synthesis only grown simpler. New radicles have undoubtedly become attached to it as side-chains replacing or supplementing old ones and changing its properties. The living substance is not merely *losing* constituents; it is also gaining new ones. Similarly organisms, morphologically and physiologically, change not merely by losses but also by gains. It is impossible to explain evolution satisfactorily by either process alone. The two go hand-in-hand and no doubt are constantly occurring among organisms. Change is universal. Mere subdivision of a species into two groups of individuals, which are prevented from intermingling, seems to be sufficient in time to make the two groups specifically distinct. Each keeps on changing in so many different ways that it would be little short of a miracle if both changed similarly and simultaneously in all respects. Direct environmental effects are insufficient to account for such organic changes, for among the best-known illustrations of divergent evolution are the animals of oceanic islands, close together and subjected to the same climatic agencies, undoubtedly descended from common ancestors at no remote period, yet having become distinct, probably through numerous spontaneous changes which isolation prevented from being ground down to a common level by inter-crossing.

These are commonplaces of evolutionary knowledge, familiar to everyone since Darwin and Wallace first called attention to them, yet we are in danger of overlooking them for the moment in our enthusiasm over a new method of attacking the obstinate problems of evolution. It may not be superfluous therefore to call renewed attention to them in this connection. Spontaneous variation is still with us and is as widespread as it was in Darwin's time. It is doubtful whether unvarying "completely homozygous" organisms occur anywhere outside the text-books. In the case of organisms known to be varying genetically there is abundant evidence that small variations are heritable no less than large ones, and we are by no means "done with the idea" that small variations are capable of summation.

With Bateson we must deplore the necessity of engaging merely in destructive criticism. It is indeed "a low kind of work." It would be so much easier, pleasanter, and more satisfying to adopt a single explanatory principle for evolution and build on this. But it would be foolish to go on building lofty superstructures of hypothesis on an insecure foundation, and the more carefully we scrutinize the mutation theory the more serious do our doubts become whether it is a secure foundation for evolutionists to build on.

W. E. Castle
Bussey Institution,
Forest Hills, Mass.,
December 12, 1914

Mastodon Tusk in Glacial Gravels

To the Editor of Science: A tusk of a proboscidean, probably *Mastodon americanus*, was