with many modern speculations on the cause of earthquakes, which ascribe these tremors to the slipping of rocks. My unpublished inquiry indicates that the true cause is very different. I regret that I am not yet able to give the chain of reasoning by which this result is established, but I may say that it is shown that one common cause underlies earthquakes, volcanoes, formation of mountains and islands, the elevation of plateaus, the feeble attractions of mountains noticed in geodetic operations, and the formation of great sea waves which frequently accompany violent earthquakes. All these phenomena are proved to be intimately connected, and I have shown that they depend upon a single cause, and that the earth's crust is underlaid by a fluid substratum in which the forces arise that disturb the crust.

It is nearly always assumed that changes in the earth's crust are due to secular cooling, but is that really so? When the truth comes to be known, I think it will be found that we have all been working on a false premise; a misleading hypothesis. In *Astronomische Nachrichten*, 4104, I have shown that rigidity prevents circulation, and, therefore, secular cooling would be confined almost entirely to the surface layers. Fisher and others have shown that the shrinkage due to the cooling of the crust is quite inadequate to account for the mountain folds observed upon the earth, which my researches show to depend on an entirely different cause.

Dr. Thomson is quite right in pronouncing against radium as a cause of volcanic action. The Hon. R. J. Strutt, of Cambridge, has shown that radium is very abundant in the rocks of the earth's crust, such as granite. If, therefore, we imagine radium to be the source of volcanic outbreaks, we should expect abundant eruptions to occur in all countries underlaid with granite—the United States, Europe, Asia, Africa, Australia, Brazil—which is contrary to observation. The well-known distribution of volcanoes invalidates the radium theory completely.

The Hon. R. J. Strutt, from his radium investigations, concludes that the internal temperature of the moon exceeds that of the earth. The observed low temperature of the lunar surface, however, contradicts this hypothesis, and thus we must be very cautious about ascribing too much to radium. The best experimental evidence available is that radium is a temporary form of matter, the energy of which must be renewed from other sources at intervals of 20,000 years, and thus it may play only an inappreciable part in the physics of the universe. So far, there is no evidence that it is an important cosmical agency.

The great forces which have most profoundly modified the world will be found to be familiar ones, which are overlooked mainly because they are so simple and so near at hand.

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August 16, 1906.

THE NATURE OF EVOLUTION

ON returning from Central America I find Dr. E. A. Ortmann's paper in SCIENCE of April 27 under the heading 'Dr. O. F. Cook's Conception of Evolution.' Lest the use of this label deceive any possible patrons of the genuine preparation, it may be desirable to point out that the most important ingredients have been omitted, so that the peculiar virtues of my evolutionary eye-water are entirely lost!

To suppose that progress in evolutionary knowledge can be made by the arbitrary limitation and redefinition of terms would imply, of course, a very shallow and merely metaphysical apprehension of the concrete data of the subject. Nevertheless, conceptions of evolution have to be communicated through the medium of language, and language has to be explicit if it is to convey definitely outlined ideas. When there is a practical reason for doing so, a term may be used in a special sense, subject only to the obvious desirability that linguistic changes, whether of new words or of modified meanings, be kept down to the lowest possible limits which will serve the purposes of clear exposition for the subject in hand.

The word evolution is often used as the name of the whole study of development—a branch of biology which includes the consideration of all the attendant factors or groups of phenomena. This generalized use is often convenient and wholly unobjectionable, but as soon as the question of the causes of evolution is raised the word obtains a much more explicit sense, serving then to designate the concrete physiological process in which the characters of species are changed. To insist that the progressive transformation of species be called variation, and not evolution, introduces a merely gratuitous confusion of words, since it removes both these terms, variation and evolution, from their primary significations.

The essential idea of variation is in its application to differences caused by the environment, that is, to transverse contemporaneous displacements among the individual members of a species, and not to the progressive, chronologically extended, longitudinal changes which represent the evolution of the species as a whole. These are two distinct modes of organic motion. To call them both variation does not prove that they are the same; it only facilitates such an assumption and tempts the unwary to take it for granted that anything which can modify or displace individual organisms in the transverse direction of variation, can also cause species to move in the longitudinal direction of evolution.

The kinetic conception avoids the verbal pit-fall and finds fundamental differences between the transverse contemporaneous variation of individuals and the longitudinal succession or gradual modification of form or structure in the species as a whole. Other forms of expression become necessary in order that the two kinds of phenomena formerly covered by the variation blanket can be compared and contrasted.

At such points the interests of general literature and of professional science often diverge widely. Specialists who are unwilling to use the word evolution in a definite physiological sense would have preferred some more technical means of designating this process of change in species. It might have been called, for example, symbasic prostholysis, in allusion to the fact that it is accomplished through the association of organisms into interbreeding groups rather than as a result of the environmental influences which induce variations. The species, and not the individual, is the unit of evolution; there are as many evolutions as there are segregated groups of organisms.

'The whole process of development of the organic world, from its beginning to its end,' which Dr. Ortmann prefers to call evolution, is a merely historical conception and not a biological process at all, except as it is made up of the separate evolutions of the millions of species of which the 'organic world' is composed. What is to be gained of clearness of thought or of expression by calling the general aggregate evolution, while denying this name to the specific units of development, is not easy to perceive. Dr. Ortmann would scarcely have thought to beguile us with the hollow formula that species change by variation and that variation therefore causes evolution. But why otherwise should it have appeared so astonishing to find the word evolution used in a particular as well as in a general sense? It is necessary here to fully reciprocate with Dr. Ortmann and 'positively decline to accept' his conception of evolution, if, as now appears, it is something which takes place in the organic world at large, but does not appear in the component species.

The jury must decide who has meditated the greater violence to the English language. It is certainly Dr. Ortmann who proposes the greater restriction of the word evolution, for he would permit its use only in the general and indefinite sense, as applying to the organic cosmogony as a whole, while I would recognize in addition a definite physiological meaning, when questions of evolutionary causes are being discussed.

The conception of spontaneous change in the characters of species may not be correct, but it is at least a conception, and it permits evolution to be thought of as a phenomenon separate and distinct from accidents of environment which may intensify the normal inequalities of individuals (variation), as well as from accidents of geographical distribution by which groups of individuals may be subdivided (speciation). All these are evolutionary matters in the general sense already alluded to, but underneath all the multiplicity of more or less pertinent data and speculations is this process of change in species. It may be denied, as in the mutation hypothesis of Professor de Vries, that there is such an evolutionary motion of specific groups, but all will be ready to admit that if such progressive changes of species take place they represent the real center and essence of the subject of evolution, the physiological process of which it is of so much importance to know the conditions and causes.

The fact that evolutionary literature has become so vast a congeries of speculations should not make us forget what it is all about. Certainly it affords no sufficient reason for avoiding the use of the word evolution in describing a conception in which a continuous modification of the specific type is treated as a normal condition and requisite of organic existence.

After writing the above I have come upon a further article by Dr. Ortmann in SCIENCE of June 22, in which he appears reconciled to the new term speciation, in spite of the hoary antiquity and other objectionable features of the idea which led to the suggestion. This is very gratifying. But at the same time it becomes even more obvious than before that the title of Dr. Ortmann's previous article was misleading, for in this last review of developmental theories he leaves out of account altogether the very conception he has so recently claimed to discuss, a conception which, whether old or new, true or false, is radically diverse from any of the alternatives treated. The distinction of speciation from evolution has been taken, evidently, as the whole 'conception,' whereas it is only an incidental feature. The mistake is due, no doubt, to my continued failure to give the kinetic point of view an adequate presentation, but it may be that the discussion has now reached a stage where the distinctions can be outlined more clearly than before.

Without denying the general literary sense in which anything which has even a remote bearing or influence on evolution may be considered a factor, we may return once more to the kernel of the whole matter, the question of the true, actuating causes of evolution. The differences between the alternative interpretations may then be definitely located.

It is evident that Dr. Ortmann is discussing a generalized abstraction compounded out of the four factors or groups of phenomena, variation, inheritance, adjustment and speciation. The kinetic conception, on the other hand, treats evolution as a concrete process, carried forward through two factors which are very different from the other four, since they are resident in species and do not depend upon environmental influences. Dr. Ortmann's unwillingness to recognize evolution as a concrete process can now be understood, for the factors upon which he relies are incapable of explaining such a process, as a brief examination will show.

Inheritance, to take the oldest idea first, is a general condition of organic existence, but it has no evolutionary implication. If there were no inheritance there would be, of course, no evolution in the biological sense, but this is no indication that inheritance causes evolutionary progress. Many writers have consistently denied that inheritance causes, or tends to cause, evolution. They hold, on the contrary, that like would produce like indefinitely unless acted upon by disturbing agencies of the environment. Adaptation or adjustment to environment, whether by natural selection or otherwise, is not a cause of evolution, but rather a result, a meeting by evolutionary processes of requirements imposed by external conditions. Speciation, or the diversification of segregated groups of organisms, is also clearly an evolutionary result instead of a cause. Even variation, in the sense in which the word appears to be used by Dr. Ortmann, to indicate the effects of external influences upon organisms, has not been shown to have any connection with evolution, notwithstanding the persistent faith of the apostles of mechanical causation, such as Cope, Hyatt, Dall and Dr. Ortmann himself. In short, it does not appear that the true, efficient causes of evolutionary motion are to be found in the phenomena covered by these four terms, in the senses in which they are employed by Dr. Ortmann.

The conception of which Dr. Ortmann announced a discussion, but has not really considered, definitely abandons these supposed causes of evolution as inadequate and irrelevant and would elevate to primary importance two considerations generally ignored entirely, or given very subsidiary attention. These are (1) *heterism,* the normal diversity of the individuals of which species are composed, and (2) *symbasis,* the free interweaving of the lines of descent of these normally diverse individuals.

The progressive transformation of species is made possible by these two factors, and it has not been shown that any of the others are to be reckoned as direct or actuating causes, notwithstanding the vast amount of attention devoted in the last half century to the many static doctrines under which evolution has been ascribed to one or another form of environmental influence.

It may yet be ascertained, perhaps, that the environment does in some way exert actuating influences upon evolution, but it is not too much to say that up to this time all theories of environmental causation remain purely speculative. Heterism and symbasis, on the other hand, though long neglected as evolutionary causes, are thoroughly established facts of obvious implication. Individual diversity persists in spite of uniformity of conditions, and interbreeding is everywhere coincident with evolutionary progress. Even on purely mathematical grounds it becomes apparent that the resultant of the continuous interweaving of diverse lines of descent must be a progressive transformation of type.

Dr. Ortmann points out that de Vries has confused speciation with variation, but might be charged in turn with having confused evolution with variation, just as so many other writers have confused evolution with speciation. Why so many attempts at leaving Hamlet out of the play? Each is a testimony of the surviving strength of the old pre-evolutionary idea that species are normally constant, uniform and stationary, so that evolution would need to be caused and conducted by external agencies of the environment. Though supported by no facts, the doctrine of environmental causation is still being advocated in many quarters in a manner strongly reminiscent of the defense of special creation, by Owen and Agassiz. The kinetic conception of evolution is in respect of causality as different from environmental evolution as that is from special creation, for it holds that species are not made by the environment, but that their development goes forward as a manifestation of qualities inherent in their very constitution.

The progressive modification of specific groups of interbreeding organisms is as truly a phenomenon, as much of a fact, as any of our so-called factors, natural selection, adaptation, variation, heterism, isolation, speciation, etc., which help to make up the evolutionary drama. Evolution, in the kinetic version, is not only the title of the play, but the name of the principal rôle. It is no longer restricted to the dialogue of the subordinate players, like a mere ghostly abstraction. The actions and relations of the various attendant circumstances continue to give us very important aid in understanding the workings of evolution, but they are no longer allowed to explain it away into a nebulous compound of definitions. Some of the persons are of the immediate family of evolution, but others have no direct relationship at all, though they may appear often on the stage and perform important parts. Thus natural selection is the father of adaptation, but is related to evolution only in the indirect, restraining capacity of guide and counselor. Evolution and isolation are parents of speciation, but are related only by this marriage, and had no previous consanguinity. Environmental variation is at most only an uncle of evolution, not the direct progenitor. The remaining minor factors constitute the retainers, servants and domestic animals of the evolutionary household, but this does not give them places in the genealogy of evolutionary causes.

Dr. Ortmann is annoyed by incidental changes in familiar lines and stage directions, which he does not hesitate to charge to carelessness and ignorance, forgetting, for the time, that the whole play is being recast, and that the merits of the new rendering are to be judged by its conformity with the facts of nature, rather than by reference to the traditions of evolutionary literature.

O. F. COOK.

WASHINGTON,
July 18, 1906.

TEMPERATURE CORRECTIONS OF SUGAR POLARIZATION.

TO THE EDITOR OF SCIENCE: There has come to me a belated copy of SCIENCE (April 20) containing Dr. Wiechmann's review of my work on the polariscope, in which he discusses my treatment of the subject of temperature corrections of sugar polarizations. As Dr. Wiechmann seems to have quite misunderstood what I have stated concerning temperature corrections, in view of the great importance of the subject I have ventured to bring it again before your readers. Dr. Wiechmann takes a quotation from my book (p. 44) as to the fact that the values of temperature influence are well established [by Andrews, Wiley and Schönrock for instance] as a statement endorsing the use of temperature corrections in raw sugar polarizations. He quite overlooks the statement (on the same page, I think; I have no copy at hand) that such corrections can be *quite fallacious* if proper conditions are not observed; and yet further (p. 97?), under 'Errors of Commercial Polarizations,' where I say, that owing to other inherent errors of raw sugar polarizations it is doubtful whether application of such corrections brings any nearer approach to the true saccharimetric value; and hence, such corrections are questionable in raw products at least.

The present status of the case, as I understand it, is this:

It is well established that temperature change exerts an influence on sugar polarizations made according to standard method.

The quantitative value of such influence, when pure sugar is polarized, is known within narrow limits of error.

Owing to obscure compensatory errors, not yet possible of measurement and inherent in raw sugar polarizations, the correction of temperature influence is inadvisable as generally leading to an exaggerated sugar value. Further, application of temperature correction values gives quite fallacious results if the same constant temperature of solutions and apparatus is not maintained.

As the total errors or raw-sugar polarizations apparently come nearest to balance at 20° C. this temperature has been adopted as a rigid standard by the International Sugar Commission.

The fact that the International Commission has adopted a rigid temperature standard shows that the influence of temperature is recognized. It follows that polarizations made at temperatures other than 20°, as necessarily here in the tropics where the afternoon temperature is now from 28 to 30°, that some correction should be made for temperature influence, not to the standard, of 17.5°, but to 20°. The well-known case cited by Dr. Wiechmann simply emphasizes that 'temperature corrections' may be applied with quite fallacious results, without in any way casting doubt on the 'alleged' influence of temperature on the specific rotation of sucrose which obviously is but a small part of the influence of temperature on sugar polarizations.

Here might be raised the interesting and subtle question whether the sugar values of the saccharimeter standardized at 20° are identical with those of the instrument standardized at 17.5° when *raw* sugars are polarized.

In the whole discussion, what are facts of experiments in temperature influence on pure sugar polarizations must be carefully differentiated from what is the most consistent and fairest way to estimate the sugar value of a commercial product, by the indications of a method which at its best is subject to errors as