

usual causes of their failure to become developed into distinct species."

This, I repeat, is the essence of physiological selection; and any "originality" which my views upon the subject present consists in recognizing the "fundamental fact" set forth in the first of the two sentences above quoted, together with its consequence as set forth in the second. Before Mr. Catchpool published the theory in these columns, no one—with the partial exception of Mr. Belt—had perceived this factor of organic evolution; and while, for about the sixth time, repudiating the grotesque "originality" which Mr. Wallace continues to ascribe, I may conclude by observing that—personalities apart—it is to me a matter of satisfaction that he has now himself begun to perceive the existence and the importance of the factor in question.¹

GEORGE J. ROMANES.

Oxford, December 1.

The Tornado.

IN NATURE, vol. xlii. p. 612, there appears a notice of my book on "The Tornado," from "H. F. B." I must thank so high an authority for noticing the book; all I ask for is a full, free, and fair discussion of the facts presented. May I call attention to one or two points which may not be clearly understood?

(1) My object in writing the book was to bring together all the facts known regarding tornadoes, and to give a brief *résumé* of theories, as far as possible, showing the gradual development during the past fifty years.

(2) I have nowhere touched one of Prof. Ferrel's mathematical discussions of the problem. In some cases I have tried to show that there may be an interpretation of certain physical phenomena not exactly in accord with his own views.

(3) I have not denied a single thermodynamic principle. The quotations given by "H. F. B." are quite plain when the book is read as a whole. These do not refer to thermodynamic principles at all, but rather to the experiments made by Prof. Espy nearly fifty years ago, and whose results I have tried myself to check. I am sure that anyone who carefully peruses the book will be satisfied that there have been read into it statements or inferences which are not there.

(4) Above all, I have not advanced any visionary electric speculations regarding the generation of tornadoes. Rather I have tried to avoid that very thing. In the very quotation made by "H. F. B.," from p. 76, occur these words:—"It has been my purpose for many years to avoid, as much as possible, all speculations in considering air motions and the causes of atmospheric phenomena. This is especially pertinent when we consider electric action in the atmosphere. It is very difficult to believe that electricity has nothing to do with our thunderstorms."

It is a significant fact that "H. F. B." begins his quotation with this last clause, and from it tries to show that I have adopted an electric hypothesis. I cannot quite see why he did not begin where he should have done. I have not given my

¹ It is, perhaps, desirable to add, as already stated elsewhere, that I entertain no doubt at all touching the unconscious or unintentional nature of the "adoption." Nevertheless, I may further add, the adoption itself is so manifest, that several eminent men of science wrote to me on the subject when first his work on "Darwinism" appeared. Among the mildest of their comments are:—"Mr. Wallace has treated you very badly. After having set up a caricature of your theory, he adopts the theory itself, pure and simple." But of more importance is Mr. Gulick's opinion, seeing that he was the first to conceive, though the last to publish, the theory of physiological selection. As soon as he had read "Darwinism," he wrote me from Japan a long letter, the substance of which may be gathered from the first two sentences, as follows:—"Mr. Wallace has most certainly adopted the fundamental principles of our theory. He takes our principles, which in the previous chapter he has combated; but he makes such disjointed use of them that I am not willing to recognize his statement as an intelligible exposition of our theory." More recently he sent to the *American Journal of Science* a paper, which he summarizes thus:—"Mr. Wallace's criticism of the theory of physiological selection is unsatisfactory: (1) because he accepts the fundamental principles of that theory on pp. 173-79, in that he maintains that with-out the cross-infertility the incipient species there considered would be swamped; (2) because he assumes that physiological selection pertains simply to the infertility of first crosses, and has nothing to do with the infertility of mongrels or hybrids; (3) because he assumes that infertility between first crosses is of rare occurrence between species of the same genus, ignoring the fact that, in many species of plants, the pollen of the species is prepotent on the stigma of the same species when it has to compete with the pollen of other species of the same genus; (4) because he not only ignores Mr. Romanes' statement that cross-infertility often affects 'a whole race or strain,' but gratuitously assumes that the theory of physiological selection excludes this 'racial incompatibility' (which Mr. Romanes maintains is the 'more probable form'), and bases his computation on the assumption that the cross-infertility cannot be associated with any other form of segregation."

name to any hypothesis at all, but have advanced a few facts which I hope may ultimately help us to build up the true view of tornadic phenomena.

The familiar lecture experiment of forming a cloud in a receiver by a stroke of the air-pump is given by "H. F. B." as an illustration of dynamic cooling. It would be quite interesting if some one would compute the amount of work done by the air in this case, premising that the stroke of the pump is made quickly enough to form a vacuum into which the air from the receiver rushes. Tyndall says: "Mere rarefaction is not of itself sufficient to produce a lowering of the mean temperature of a mass of air." It is quite well known that the work done in this experiment is not that of driving out a piston, but is rather the very slight work needed to impart a motion to the molecules in the receiver—in other words, to drive out these particles from the receiver.

As to whether a dense and cold stratum of air in motion can overrun a warmer stratum, I have to say that this question has been negatively settled in this country. While such a condition might be possible in quiescent air, and has been observed in balloon voyages, yet in these cases there was no disturbance of the atmospheric equilibrium. In balloon voyages I have myself found a distribution of temperature in a vertical direction, which, according to theory, should have given rise to a violent tornado, but there was no marked disturbance. I have faith to believe that in the near future there is to be a marshalling of facts which shall establish true views of storm-generation; and even now there are many intelligent men who have grown restive under the present pure theories and mathematical analyses of atmospheric phenomena. To my mind, the Dr. Hann agitation has done a great deal to open the eyes of orthodox meteorologists, and even "H. F. B." seems to be in a little doubt as to the final outcome of his views in relation to orthodox meteorology. It seems to me all persons who are studying storm-generation and movements are realizing the absolute need of a solid groundwork of fact on which to base our views, and this is the great point that I have been contending for these many years.

Washington, D.C., November 18.

H. A. HAZEN.

PROF. HAZEN can alone speak as to what views he intended to advocate; a reviewer can only take count of such as are expressed or implied in the work he reviews, and the present writer is unable to see in the above letter any evidence that the quotations given from Prof. Hazen's work were not fairly representative of his text, or that they fail to justify the comments upon them. That he is not alone in his inference that Prof. Hazen "appears to regard as inapplicable to the movements of the atmosphere, those laws of thermodynamics that are based on the results of Joule's labours," is shown in the following extract from Prof. J. Hann's paper in the September number of the *Meteorologische Zeitschrift*:—"Da Herr Hazen so ziemlich alle Grundlagen, auf welchen man die Meteorologie in neuerer Zeit mit Sicherheit weiter ausbauen zu können vermeint, leugnet oder in Zweifel zieht, darunter selbst allgemein anerkannte physikalische Gesetze, wie z. B. die adiabatische Temperatur-Änderungen in feuchter Luft, so scheint es zunächst allerdings überflüssig, sich mit ihm in eine Kontroverse einzulassen, da der Boden für eine Verständigung gänzlich fehlt." The passage referred to in this remark is one of those quoted in the present writer's review.

H. F. B.

Araucaria Cones.

I AM drawn to add, with your permission, a few words to what has already been written on the subject of Araucaria cones, by noticing that all your correspondents speak of trees situated in the south of the British Isles, or, at least, not further north than Cambridge, whereas it may be of equal, if not greater, interest to the Duke of Argyll and others to know something of the behaviour of the Araucaria in the north of Scotland. It also seems to me unfortunate that many of the correspondents have omitted to mention the most interesting point concerning the fruiting of the Araucaria, viz. the monœcious or dioecious character of the trees they describe. Loudon, in his well-known book on the "Trees and Shrubs of Great Britain," pronounces the Araucaria to be dioecious. At that time knowledge could only be gained of it on its native hills. In the "Manual of Coniferae," published by J. Veitch and Sons, is figured a branch from the monœcious tree at Bicton with both pollen and ovule-bearing catkins. It would be interesting to learn if many of the