cumulative evidence as to the *orderly change,* our knowledge of which as applied to animals and plants constitutes organic evolution.

But here agreement ends. Observers, experimenters and philosophers have each different points of view. Observers in the field, assuming the facts of heredity and variation, the former inherent in the chromosomes, the latter in large degree at least promoted by double parentage and unequal division of chromosomes in maturation of germ-cells, find natural selection a universal factor in life adaptations; moreover, that barrier-isolation with its consequent segregation and preservation from mass-breeding is an efficient cause of the moulding of species as they are. That species, as they exist in nature, are *real "species,"* not products of species-makers, whether "lumpers or splitters" (two slang names for incompetents) must be admitted.

That forms equally distinct to all appearance may be developed in a short time, in the breeding-pen or greenhouse, may be admitted. These are produced in the same way—by a chosen variation being placed under a new and non-competitive environment, the basis of selection being wholly changed, and the product carefully segregated. It does not compete with the mass, nor can it interbreed with it. These imitation species, or "creations," to use the florist's term, have not entered the gauntlet of life. They have not endured and mostly would not endure. In the open they would be swamped by mass breeding, by lack of prepotence—or in most cases by lack of competitive vitality.

It is probable that every species in nature is purged by natural selection and retained through segregation. Every one we know has, more or less clearly, a relation to geographic (or in rare cases to physiological) barriers. Most closely related species will interbreed (plants especially so) when opportunity favors. Yet hybrid individuals are relatively few, a few dozens recorded among birds and fishes, while not a single species known can be reasonably supported to have arisen from hybridism.

Most experiments on "species" have been made along the selection of unusual examples or with hybridization of existing species. It is natural that one who has not realized the cumulative evidence, which shows the relation of distinctness of species to geographic isolation ("räumliche Sonderung"), should, in contemplation of "mutation" and "Mendelism" as species-formers, be "agnostic" regarding the whole matter of causes of evolution. For hybridism or mutation are rarely or never the basis of a species in nature. The experiments of de Vries with a garden flower, presumably a hybrid, are not typical of the ways of "wild nature." Several writers, especially on botany, calmly ascribe the division of genera into species to "mutation." We have yet to hear of any single species which could be, with any probability, regarded as having arisen by "mutation" or by "Mendelian hybridism." The experimenters can not afford to ignore the students of "things as they are," and who "strive to interpret what really exists," the men whom Huxley termed "hodmen of science."

The conception of Darwin that the accumulation of favorable variations may of itself and without segregation produce new species within the territory of the parent stock is as yet unproved and seems improbable. We must note two general facts. First, the features which distinguish species or subspecies are not as a rule matters of survival importance. Selection will, as a rule, speedily eliminate injurious qualities. But one existing species is (under like stress) just as well adapted as another.

Second, closely related species never occur within the same limits (a few cases attributed to reinvasion excepted). Nor are they as a rule widely separated, being on different sides of some barrier, mountain, sea, desert or other feature, not wholly insurmountable but rarely crossed. This feature of "island life" and "mountain life" is well known to all actual students of geographical distribution.

The philosophers of evolution must depend on observers and experimenters. In one sense their conclusions are negligible, for these are temporary and variable. A theory or working hypothesis becomes a part of science when its rival hypotheses have ceased to work. Organic evolution by this means has become a part of science. The extension of our knowledge of the factors that lie behind it has now become, in a sense, more important than the theory itself.

As to these causal factors, it may be noted that the broader the view the less the accuracy in detail. The "hodmen of science" may console themselves with the words of Linnaeus, "The tyro makes classes, the master makes species"; or better with Darwin's warning that "no one should discuss species at all who has not minutely compared and described many of them."

DAVID STARR JORDAN

STANFORD UNIVERSITY

SOLAR VARIATION AND THE WEATHER

I AM strongly interested in the papers of Professors Marvin and Kimball which recently appeared in the *Monthly Weather Review* for July. Being about to go upon an expedition to select sites and arrange a solar radiation observing station in the eastern hemisphere, under the joint auspices of the National Geographic Society and the Smithsonian Institution, I am compelled to forego any extensive study and rejoinder. Yet I would be sorry if a silence of seven months should convince scientific men that no rebuttal on our part is possible and that the insignificance of solar variation as an agent affecting weather must be conceded. Therefore, I venture to note a few points which my hasty examination of the papers thus far suggests.

(1) I by no means agree with Professor Marvin that "it is futile to hope to establish any scientific basis for weather forecasting on supposed changes of solar constant before we know that the constant does change from day to day, and if it does, how much." For while neither Professor Marvin nor certain other authorities are yet ready to admit that our observations have demonstrated either the real variability of the sun or its magnitude, the adoption of solar variability as a working hypothesis has already yielded in the hands of Clayton, Arctowski, Nansen, Walker and others meteorological correlations which are now actually being used with a moderate measure of success in the official long-range forecasting of Argentina and in the private long-range forecasting of Clayton. Progress in this line is to be expected quite as much as in others.

(2) Professor Marvin compares, on page 292 of his article, the scatter of three groups of data: (a) direct pyrheliometer readings; (b) approximate solar constants, which he computes by a method of his own and calls "hybrid solar constants"; (c) our solar constants obtained after the method of Langley. He expresses perplexity because the scatter of (a) exceeds that of (b), and this again exceeds that of (c), and intimates that this line must be pursued further with more data in search of an explanation for this paradox. Surely it would have been astonishing if it had been otherwise. The data (a) are affected by every change of atmospheric transparency and humidity. In Professor Marvin's "hybrid solar constants" these atmospheric sources of fluctuations are partially eliminated. In the Langley method, which gives what I may in contradistinction call "thoroughbred solar constants," these atmospheric sources of fluctuation are approximately completely eliminated. No wonder the scatter becomes smaller!

Indeed at this point I can not but express my surprise that Professor Marvin sees such a hopeful solar variation prospect in pyrheliometer measurements alone, unaccompanied by measurements adapted to accurately evaluate and remove atmospheric sources of fluctuation. If one were observing on the moon, the pyrheliometer alone would be a useful instrument to detect solar changes, but not here.

(3) Both Professor Marvin and Professor Kimball admit some possibility that our results have discovered (a) solar variations of considerable duration for which they use the term "secular," and (b) occasional briefer changes accompanying the passage of sunspots across the center of the solar disk. These possibilities Professor Marvin reserves to discuss later. He expressly states that his present paper (although filling eighteen quarto pages) is designed merely to discuss the possible reality of day-to-day solar changes. Nevertheless, he devotes three pages to disclosing, as he believes, a yearly period in the Mount Wilson and Chile observations accompanied by higher values in midsummer. Since the opposite, as he finds, occurs at Harqua Hala, he obligingly accuses us of fudging the Harqua Hala results by statistically determined corrections, in such a manner as to force them to agree with Montezuma. I shall reserve discussion and reply to a more extensive publication.

It is of interest here to point out that on the showing of his own curve the yearly effect he points out on Mount Wilson should have produced but four tenths per cent. range during all the months observed, namely, May to November. Of these, May and November seldom appear. Marvin's range, June to October, is but one fourth per cent.

In Mr. Clayton's studies of these old data, which Professor Marvin seeks to cast into disrepute by the hypothesis of a yearly periodic fluctuation, Clayton in part compares the weather conditions of identically named months of different years, *i.e.,* July with July, et cetera. One hardly sees how even Marvin finds room to criticize that. In the other part of his discussion, Clayton groups the results into high, medium and low values, using all the data of four years. His range from high to low is 5 per cent. or over twelve times the applicable part of Marvin's supposed annual correction.

Out of this latter work of Clayton's comes the extraordinary diagram connecting solar variation with the temperature of Buenos Aires for twenty days after, which I first published in 1920 and again as Figure 1 of Smithsonian Miscellaneous Collections No. 2825. I have repeatedly challenged Professor Marvin and Kimball, both privately and publicly, to explain this result and say why it is not a starting point for a new departure in forecasting. So far they have made no response of any kind. However, Mr. Clayton and Mr. Hoxmark, as shown in Smithsonian Miscellaneous Collections Nos. 2826 and 2827, have reduced the method to practice with results which, while not ideal, already show sufficient prevision to be worth money to men of affairs. One wishes that financial means were available to push Mr. Clayton's line of investigations, which seems to promise much.

(4) Professor Marvin clothes his statistical methods with powers which I am far from conceding. Having shown by his diagram (Figure 6) that the "probable" variation or measure of average scatter of the solar constant in the year 1919 does not exceed 0.6 per cent., he proposes to determine whether that part of it due to real solar changes may not be shown to be still less. Discrimination depends on the consideration that the relative scatter of pyrheliometer values at different air-masses is differently affected by solar and atmospheric causes.

It is well recognized: (a) On many of the best days, changes of transparency materially affect the comparability of pyrheliometer values. (b) This source of error is eliminated in our "short method," so that we observed with good results on many days when the "long method" was inapplicable. (c) Hence, many of the days used by Professor Marvin which were only fit for short methods are not to be regarded even as "best days." (d) Atmospheric dustiness and humidity alter greatly during a year and produce entirely different kinds of effects on different spectral rays. (e) On these and other accounts, Bouguer's formula is inapplicable to pyrheliometry in general and more particularly to some of that used by Marvin. (f) The basic assumption of Professor Marvin (p. 290, lines 30 to 33) nevertheless depends on the applicability of Bouguer's formula thereto. How wrong it is may be seen by comparing, for example, January 6 and January 19, 1919. They show a difference at air-mass unity of 12 per cent. and only 21 per cent. at air-mass four. According to Marvin's assumption, these numbers should be as 1 to 4. (g) Formulae relating to the precision of measurements depend on treating errors as differentials. How can one justify treating quantities like 12 or 21 per cent., or even the halves of them, as differentials?

Notwithstanding these weaknesses and others, Professor Marvin would have us believe that his statistical discussions of pages 290 to 293 have eliminated atmospheric effects from fluctuations ranging 10 to 20 per cent. so thoroughly as to warrant derogatory inferences as to the reality of a solar variation whose average is known to be not exceeding the order of 0.6 per cent. during the period considered. Statistical methods are useful, but they can not work miracles. If one starts with figures he must end with figures of some sort, but their significance may be zero.

(5) Professor Kimball, after tricking the eye with circles, admits apparent day-to-day correlations between Montezuma and Harqua Hala, but regards them as so small as to be meaningless, and like Professor Marvin attributes them to irresponsible fudging of Harqua Hala results. He eliminates the important big correlation of a secular character in 1922 by his grouping. If it be admitted as real, it, at least, might seem of interest.

(6) No one is more conscious than myself that the solar constant data are still imperfect. We are working with all our little force, not only to make new and better determinations, but to rectify as far as possible those published in preliminary fashion hitherto. Old values, as I have elsewhere remarked, are poor, newer ones better, and future ones we hope will be better still. Successive improvements show on Professor Marvin's diagrams. I do not believe, however, as he does, that the total elimination, if it were possible, of the present existing errors will remove the day-to-day variations of Montezuma results to a considerable extent. We are dissatisfied with summer conditions at Harqua Hala, and have just removed to a new station on Table Mountain, California, 2,000 feet higher, and with much better sky conditions. We are making great improvements in procedure at both stations. These should speak for themselves. In the meantime, I can not but wonder whether if Professor Marvin had used as much pains to search for useful correlations between our published values and weather conditions as he has used to discredit our results of 1905 to 1920, we might not have been further along.

C. G. Abbot,
Assistant Secretary

Smithsonian Institution

THE KANSAS CITY MEETING OF THE AMERICAN ASSOCIATION

Arrangements for the approaching Kansas City meeting (December 28 to January 2 next) are well advanced. It will be a great success.

The usual reduced railway rates have been secured from the regional passenger associations. The certificate plan will again apply for the United States and for eastern Canada. Persons going to the meeting, whether members of the association or not, should purchase one-way tickets, securing a certificate for the American Association for the Advancement of Science and Associated Societies. (A receipt is not what is needed.) After validation at the meeting the certificate will entitle the bearer to purchase a return ticket at half the regular fare.

The Hotel Muehlebach (12th St. and Baltimore Ave.) is to be the general headquarters for the meet-