puzzles it may be that they share this superiority with organic chemists and spectrum analysts. So perhaps they need not be altogether ashamed of their little accomplishment.

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I SHARE Prof. Johnstone's inability to solve either crossword puzzles or Mendelian results (NATURE, Feb. 26, p. 319). But I do not think this should encourage him to decry the carrying out of breeding experiments by men who can properly interpret them. It seems reasonable to regard Drosophila as the sum of a number of factors arranged in a certain way, just as a crystal of alum is an aggregate of molecules orientated in a definite pattern, of atoms specifically arranged to give molecules, and of electrons to give atoms.

It is almost an axiom of scientific method that analysis must precede synthesis. The first stage toward the solution of the Drosophila problem, then, must of necessity be one of analysis; the splitting up of the entity Drosophila into genes and factors. This analysis is far from being complete yet. With the materials gained thereby we can proceed to the synthesis, namely: Why is this aggregate Drosophila, and not Pulex or Homo? An attempt to synthesise before the materials are available is as ineffectual as the attempt to make bricks without straw.

The problem Prof. Johnstone propounds is, however, the really fundamental one; and until we reach the stage of synthesis, Drosophila will not be allowed to retire into the oblivion it so well deserves.

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Atmospheric Electricity.

DR. CHREE, in his review on the above subject (NATURE, Dec. 18, 1926, p. 894), has inadvertently drawn an erroneous inference with regard to the reduction factors used by Dr. Mauchly for the *Carnegie* potential-gradient observations. The manner of obtaining these factors is described by Messrs. Ault and Mauchly on pp. 207-209 of the volume (Researches, Department of Terrestrial Magnetism, vol. 5) reviewed, and it is stated by them that the values finally adopted depend on the reduction-factor determinations made during the years 1915 to 1921, namely, during cruises IV., V., and VI. (not VI. alone, as Dr. Chree infers).

Certain methods of reduction of the ocean observational data are criticised by Dr. Chree. The fact of the matter, however, is that there is no consensus of opinion as yet on the best methods for the reduction and derivation of so variable an element as the "atmospheric potential gradient." No agreement regarding these matters exists even at observatories in the same country and under the same general administration. He who wishes may readily find fault with the methods of his co-worker. So also as regards the application of "non-cyclic corrections" in the manner advocated by Dr. Chree, there is legitimate cause for difference of opinion. No adequate physical basis for Dr. Chree's assumptions has as yet been advanced.

But even had Dr. Mauchly omitted entirely the series of observations to which Dr. Chree takes exception, his general conclusion as regards the progression of the diurnal variation of the atmospheric potential gradient, according to universal time rather than local time, would not have been affected. Others have arrived at a similar conclusion. Thus, Dr. Hoffmann in 1923 (two years after Dr. Mauchly's first announcement in the *Physical Review*, vol. 18, pp. 162 and 477) concluded that the daily extreme values of the potential gradient at Arctic and Antarctic stations, as shown by the available series, occur everywhere at about the same absolute time (*Beiträge zur Physik der freien Atmosphäre*, xi, Heft 1, 1923).

Recent observations in Arctic regions and elsewhere have confirmed in the main the validity of the conclusions by Mauchly and Hoffmann. Thus, Dr. H. U. Sverdrup, in charge of the scientific work of Amundsen's *Maud* Expedition, recently reached the following conclusion from a discussion of all available observations on meteorologically undisturbed days: "Our observations in the Arctic sea, far from land or close to the coast near the 160th meridian of east longitude, give very positive confirmation of the conclusions by Mauchly and Hoffmann."

There are land stations which, evidently because of local influences of one kind or another, show extreme values of the potential gradient at times differing an hour or more from the average times, and there are some land stations which exhibit double maxima and minima in the diurnal variation of the potential gradient. It is generally found at these latter stations that one of the maxima and one of the minima values occur near the average times of extremes of the potential gradient in undisturbed regions. Suffice it to say that all available data at hand at present show that if the hourly values of the atmospheric potential gradient be plotted according to universal time, there will be found a general agreement in phase among the curves for stations in very remote regions, such as is not exhibited if the hourly values be plotted according to local time.

As regards a possible relationship between solar activity and the atmospheric potential gradient, I beg leave to refer the interested reader to my previous articles in NATURE and in the issues of *Terrestrial Magnetism and Atmospheric Electricity*, for March and December 1924, and March 1925. In the last-cited article, p. 17, will be found an extract from the Potsdam observations, published in 1924, showing that Dr. Kähler agreed with me that the Potsdam series, owing to the severe climatic conditions to which that station is subject, is not well suited for investigations as to a possible effect of sunspottedness on the atmospheric potential gradient. But even if the Potsdam series is included in the combined available data at Eskdalemuir (Scotland), Kew (England), and Ebro (Spain), for the complete solar cycle 1913-1922, a positive or direct correlation between atmospheric potential gradient and sunspottedness is found, which amounts to $0{\cdot}54 \pm 0{\cdot}15$, if no account of drift (my t-term) arising from one cause or another is taken into account, and to $0{\cdot}78 \pm 0{\cdot}08$, if corrections be made for drift as was done in the volume reviewed by Dr. Chree.

I intend publishing elsewhere some later evidence bearing on this important matter.

It will probably be necessary to await the completion of another cycle and the accumulation of data at widely distributed stations, not subject to local disturbing influence, before all the questions arising as to the precise nature of any solar activity influence on atmospheric electricity may be definitely settled. However, renewed interest has been aroused in the problem, and it is also gratifying in this connexion that, beginning in 1928, the observational work in atmospheric electricity aboard the *Carnegie* will be resumed.

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Washington, D.C., Jan. 31.