likely to be misleading. This too only represents an arithmetical result, and signifies that as in the series of twenty-two observations there is an average departure of ± 5.2 inches from the mean of all the measurements, the probability is that this mean will be within ± 0.94 inch of the truth, so far as those measurements are to be trusted.

For these reasons I am quite unable to follow the arguments by which it is sought to connect the amounts of these two "probable errors," or to see how they can in any way indicate "the relative probability of this particular variation being the result of a periodic law, and of its being a mere fortuitous series of variations from a constant average."

Neither does there appear to be any justification for assuming that the relative probability of the truth of two hypotheses is represented by the inverse ratios of the probable errors of results derived from them. Still less is there any ground for saying that because the particular series of quantities under discussion relates to a period of twenty-two years, the relative probability just alluded to is thereby increased to the twenty-second power of that ratio, or from about $1\frac{1}{2}$ to 1, to 655 to 1. It is no doubt true that if the probability of an event occurring once be represented by the fraction $\frac{1}{x}$, the probability of its recurring n times in succession

will be represented by $\left(\frac{1}{x}\right)^n$; but I fail to see how this affects the question at issue.

RICHARD STRACHEY.

July 11, 1887.

Is Cold the Cause of Anticyclones?

IN a review of Loomis's papers in this volume of NATURE, p. 2, occur the following sentences:—"While all, or nearly all, of the high pressure of anticyclones may be accounted for by the very low temperatures which overspread the same region at the same time along with the resulting upper currents concentrating upon them from adjoining cyclonic regions, it is quite different with the low pressures of cyclones. In the case of cyclones the problem is complicated by the strong winds, the copious precipitation, and the ascending currents, which affect the results in ways which no physicist has yet been able to explain."

This induction of Loomis's, that anticyclones are largely the result of cold, which the reviewer here repeats, is in entire opposition to the deductive views of Ferrel, and I think the discrepancy is to be found in the method used by Loomis in drawing his inductions. In order to investigate the cause of anticyclones, Loomis selected only decided areas of high pressure, and as a consequence his data were almost entirely confined to the winter months, when the temperature accompanying anticyclones is always low. If, however, he had selected more moderate anticyclones, he would have found that in summer anticyclones in the United States are sometimes accompanied by intense heat (90° F. or more). This is especially so in periods of drought. Under these conditions the approach of a cyclone with rain brings a most refreshing cooling. Furthermore, Hahn, attacking the problem by a different method, has obtained results apparently directly opposed to this induction of Loomis. Hahn made a careful study and comparison of the observations obtained last autumn and winter on the Sonnblick and at adjacent mountain and valley stations (see *Meteorologische Zeitschrift*, February and April 1887). One of the most marked results found was that at heights exceeding 1000 metres above sea-level there was an increase of temperature during anticyclones, while a decreased temperature was only found in valleys and near the general level of the earth's surface. Hahn's average results show that the temperature on the Sonnblick, height 3090 metres, rose from an average of $-16^{\circ}4$ C. at the average barometric pressure of 509.1 mm. to $-7^{\circ}7$ C. at the barometric pressure of 529.3 mm.; while at the same time the average temperature at Schafberg, height 1776 metres, rose from $-9^{\circ}0$ C. to $+1^{\circ}4$ C.; but on the contrary at Zell-a-See, height 754 metres, the average temperature fell from $-5^{\circ}9$ C. to $-8^{\circ}9$ C. These results, which show that the larger portion of the atmosphere is warmed instead of cooled within the area of an anticyclone, seems entirely destructive of Loomis's hypothesis that the cooling of the air near the earth's surface is the chief cause of the anticyclone. Hahn's results, however, indicate that the cooling of the air near the earth's surface does increase the pressure somewhat. Thus in October 1886 the barometric minimum occurred at all the stations, both mountain

and valley, on the 17th; while the barometric maximum occurred at all of the stations on the 30th. When the difference in pressure between the mountain stations at the time of barometric minimum was compared with the difference in pressure between the same stations at the time of barometric maximum, it was found almost exactly the same; but the difference in pressure between the valley stations and the mountain stations was about 5 mm. greater at the time of maximum pressure than at the time of minimum pressure. Hahn refers this greater range of pressure at the valley stations to the decreased temperature at valley stations during anticyclones, but this still leaves a range of pressure of nearly 20 mm., which the decreased temperature entirely fails to explain. These results of Hahn are in entire accord with the results obtained at Blue Hill Observatory (640 feet above sea), as compared with Mount Washington and with stations near sea-level, which indicate that the cooling in anticyclones is almost entirely confined to within a few hundred feet of the earth's surface (see *Science*, vol. viii. pp. 233 and 281).

In the light of these facts, it seems more reasonable to assume that the warmth found on mountains and the cold in valleys, accompanying anticyclones, are the result, rather than the cause, of anticyclones. Such researches as those of Langley on the solar heat, and of Hahn on the distribution of temperature pressure, &c., in different planes of the atmosphere, indicate that the effect of the sun's heat on the atmosphere is far more complex than some of our text-books on meteorology would have us believe; and instead of the cause of anticyclones being as simple as the reviewer of Loomis's article states, it seems probable that we shall understand the phenomenon of the anticyclone only when we master the problem of the cyclone.

H. HELM CLAYTON.

Blue Hill Observatory, Boston, Mass., U.S., June 28.

Physiological Selection.

I AM perhaps in a position to contribute something practical to the discussion upon Mr. G. J. Romanes's proposal of physiological selection as an improvement upon Darwin's natural selection.

I failed to meet with Mr. Romanes's paper in the Journal of the Linnean Society, and I confess that I did not gain a very clear idea of what he meant by physiological selection, until I read his article in the *Nineteenth Century* for January. His main difficulty appears to be the intercrossing with parent stock, which he thinks would prevent the survival of any varieties naturally selected to become species. Now, my Australian bush experience of the habits of animals and birds satisfies me that this difficulty is mainly, if not entirely, imaginary, and that Nature amply provides against the supposed intercrossing. Any person who has observed the habits of semi-domesticated stock, such as horses and cattle, can scarcely fail to know that migration is a general practice of one sex, and a frequent one of the other. The old always hunt the young males and strangers entirely away to form herds and families of their own, and thus the supposed intercrossing is by one sex at least effectually obviated. But it is also frequently the case that young fillies and heifers, at the same season, take to wandering, for less evident reasons, far from their accustomed haunts, scores of miles, after which they will stop, and attach themselves to another herd and locality as tenaciously as their parents remain in theirs. This of course further tends to prevent intercrossing with parent stocks.

I cannot but think that Mr. Romanes's anxiety to find a solution of his difficulty has led him into serious mistakes, which vitiate his treatment of the subject. For instance, he says (page 59, *Nineteenth Century* for January), "The hypothesis of physiological selection sets out with an attempted proof of the inadequacy of natural selection, considered as a theory of the origin of species." I was out walking yesterday when I read this, and I wrote in the margin, "The theory of natural selection is one, not of the origin of species at all, but of the preservation of particular varieties." On reaching home, I referred to the "Origin of Species" (4th edition, 1866), and was certainly pleased to find that I had adopted Darwin's precise words repeated in several places (see pages 71, 91, 123, &c.). At page 91 he says:—"Some writers have misapprehended or objected to the term natural selection. Some have imagined that natural selection induces variability; whereas it implies only the preservation of such varieties as occur, and are beneficial under

the conditions of life." Was not this a prophet? Yea, I say unto you and more than a prophet! Of course if the conditions of life are unfavourable, the incipient variations cannot become species. But surely it is obvious that in *variation* is the real *origin* of species. Variations must occur before the selection of some of them in preference to others. To consider the theory of natural selection as a theory of the origin of species, is therefore clearly an error. In his "Origin of Species" Darwin certainly expounded variation, and I might have ventured to think that as the book deals more largely with the subsequent selection of a few varieties to survive as species at the expense of many extinguished, a more exact title for it would have been "The Evolution of Species." But what says the great master? See page 71:—"Owing to this struggle for life, any *variation*, however slight . . . will tend to the *preservation* of that *individual*, and will generally be inherited by its offspring. . . . I have called this principle by which each slight *variation*, if useful, is *preserved*, by the term natural selection, in order to mark its relation to man's power of selection." And who will not recognize the wisdom of his selection of the term? It has been before observed that the "*Ascent of Man*" would seem a more accurate title than the "*Descent of Man*." But I have no doubt that his reasons for preferring the latter were equally cogent.

But Mr. Romanes proceeds:—"This proof is drawn from three distinct heads of evidence. (1) The inutility to species of a large proportional number of their specific characters. (2) The general fact of sterility between allied species, which admittedly cannot be explained by natural selection, and therefore has hitherto never been explained. (3) The swamping influence of even useful variations of free intercrossing with the parent form." I have advanced, I think, ample reasons why No. 3 may be regarded as imaginary, and which therefore reduce the value of No. 2 to a minimum. No. 1 depends entirely upon the definition of "*utility*." Has this word any real significance outside human interests and considerations? The idea of utility, if extended to Nature's operations, may, it seems to me, apply to the interests of any other variation than the one whose specific characters are in question, which may therefore be, without compunction or regret, sacrificed to the most fit, as we know that innumerable species have been extinguished in the interest of those that supplanted them. But utility to Nature may be the extinction of one variation and the preservation of another. As Mr. Romanes's whole paper is built upon what I have already quoted from it, I need scarcely follow it any further. With your permission, however, I have another remark to make.

Mr. Romanes seems to me to have been much exercised by the consideration of the intercrossing with parent forms, and, not knowing of the simple solution given above, to have cleverly invented his physiological selection to escape from the dilemma. Of course Nature is not clever, but simple in its operations. I was always much impressed with what appeared to me a greater difficulty, which might be thought to have a clearer title to be called "physiological selection." I allude to a general tendency in the (human at least) sexes to prefer a mate with opposite characteristics, with the apparent result of insuring mediocrity in the progeny. Thus, as a general rule, the tall prefer the short; the dark, the fair; the wise, the silly; &c., and *vice versa*. Variation is, on the other hand, apparently insured to a large extent by the differences between parents, but still it would seem that the tendency should, *ceteris paribus*, be inevitably towards a mean in the progeny. The general migration, however, as above indicated, of young males and females, gives plainly ample opportunity for the preservation of viable variations, besides others which experience and care will doubtless discover.

Melbourne, April 11.

H. K. RUSDEN.

Weight, Mass, and Force.

APPLICATIONS of the data previously given, in the extract from the American journal, to the dynamical principles of varied motion are easily provided for Mr. Hayward. Take the following: "Determine the weight of the greatest train the Strong locomotive can take up a 96-foot grade from rest at one station to stop at the next station a mile off in four minutes, taking the brake power as a resistance of 400 lbs. to a ton."

The main points at issue, however, are whether the language of the engineer, and in fact the usage of our own and other languages, is scientifically correct or incorrect in its use of the

words *weight* and *weighing*; and whether the mathematician is to be allowed to restrict the word *weight* to the subsidiary sense of force of attraction by the earth.

It is of great importance that this question of dynamical terminology should be thoroughly thrashed out now, before Mr. Hayward's Committee on Dynamics, of the Association for the Improvement of Geometrical Teaching, prepare their final report on the subject.

A. G. GREENHILL.

Woolwich, July 11.

The Sky-coloured Clouds.

ON the evenings of June 14, 18, and 19 there was a feeble re-appearance in Sark of the sky-coloured clouds, as I may call them in default of a better name, which were so brilliant in the twilights of the last two summers. Though the display this month has been comparatively faint, it has been unmistakably of the same character. I have seen nothing of these clouds since the 19th in travelling in the Channel Islands and through France.

Geneva, June 29.

T. W. BACKHOUSE.

P.S.—*Chamounix*, July 13.—I have seen one more display—a brilliant one seen from this neighbourhood on the 6th inst.—

T. W. B.

The Migrations of Pre-Glacial Man.

THE question raised by "Glaciator" has been treated by me in a paper entitled "The Faunas of the Ffynnon Beuno Caves and of the Norfolk Forest Bed" in the *Geological Magazine* for March 1887. I there stated that, "Although man probably reached this country from the east, it seems to me equally clear that he must also have arrived here with the reindeer from some northern source during the advance of glacial conditions." Though the Norfolk Forest Bed fauna contains abundant remains of deer and of other animals suitable as food for man, it is curious that so far no implements or other traces of man have been found there. The Forest Bed contains in the main the fauna of an eastern area, as the river on the banks of which the animals roamed flowed from the south-east. If pre-glacial man arrived in this country from the east or south, we should therefore expect to find evidences of this in the Forest Bed. On the other hand, wherever the remains of northern animals, such as the reindeer, mammoth, and rhinoceros, occur in any abundance, there we almost invariably find traces of man. Now that we know that man arrived in this country before the climax of the Ice age, as proved by the explorations carried on for several years at the Ffynnon Beuno Caves (amply confirmed also by this year's researches), it seems but natural to infer that man arrived in this country with the northern animals as they were compelled to migrate southwards by the gradually advancing glacial conditions, and that he kept mainly with the reindeer near the edge of the advancing ice.

HENRY HICKS.

ABSTRACT OF THE RESULTS OF THE INVESTIGATION OF THE CHARLESTON EARTHQUAKE.¹

I.

THE amount of information now in possession of the United States Geological Survey, relating to the Charleston earthquake, is probably larger than any of similar nature ever before collected relating to any one earthquake. The number of localities reported exceeds 1600. The sources of information are as follow: (1) we are deeply indebted to the U.S. Signal Service for furnishing us the reports of their observers; and (2) equally so to the Lighthouse Board, which has obtained and forwarded to us the reports of keepers of all lighthouses from Massachusetts to Louisiana, and upon the great lakes; (3) to the Western Union Telegraph Company, which instructed its Division superintendents to collate and transmit many valuable reports; (4) to the associated Press, which has given us access to the full despatches (with transcripts thereof) which were sent over the wires

¹ Paper read before the National Academy of Sciences at Washington, on April 19, 1887, by C. E. Dutton, U.S.A., and Everett Hayden, U.S.N., U.S. Geological Survey.