the germ-cells in this altered individual, which were originally continuous with the germ-cell from which the individual itself developed, remain entirely unaffected by the action of the conditions on the body, and when they begin to develop have exactly the same properties as the germ-cell in the generation preceding.

Heredity, then, according to Weismann, is simply the property possessed by a germ to develop into exactly the same type in each successive generation. He says: "Ich stelle mir vor, dass die Vererbung darauf beruht, dass von der wirksamen Substanz des Keimes, dem Keimplasma, stets ein Minimum unverändert bleibt," &c.

As the action of conditions can give rise to no hereditary individual differences, these must be due to some other cause. This cause, Weismann says, is to be found in sexual, or, as Haeckel calls it, amphigynous, reproduction. Sexual reproduction consists in the fusion of two complementary germ-cells or of their nuclei: each of these germ-cells has a specific molecular structure, on which depend the hereditary tendencies of the organism whence the germ-cell is derived. Thus, in fertilization, two hereditary tendencies are mingled, and thus the offspring does not resemble exactly either of its parents, but combines the characters of each together.

In order that there shall be no ambiguity about his argument, Weismann precisely states what, according to his view, and, as he believes, in actual fact, occurs in monogonous reproduction, *i.e.* in parthenogenesis, where there is only one parent instead of two. If, in a species reproducing parthenogenetically, all the individuals were perfectly similar, all the descendants throughout any number of generations would continue similar, leaving aside evanescent differences due to conditions, and which are not hereditary. In such a case no selection, Weismann says, would be possible, and therefore no evolution in any direction.

"Processes of selection in the proper sense of the word, those which produce new characters by the gradual increase of characters already present, are not possible in species which reproduce asexually."

"If it were once proved that a species reproducing itself solely by parthenogenesis had been transformed into a new species, thereby it would be proved at the same time that other causes of modification exist than processes of selection, for by selection the new species could not have been formed."

But with sexual reproduction it is quite otherwise. Weismann points out that no two individuals of different generations could ever be similar where reproduction is sexual, and even the individuals of one family, born of the same two parents, would not be similar, because the various tendencies in the parents are present in different intensities at different times, though he gives no reason for this assumption.

According to Weismann, the individual hereditary differences so produced are the basis on which selection acts; and these differences thus explained, summed up or combined in different ways by selection, give a complete and satisfactory explanation of all organic evolution.

Now, let us examine this theory a little. For the sake of simplicity we will in most cases consider the effect of the supposed processes on one organ. In the first place, what ground is there for assuming that *Vermischung* would ever cause an increased development in the offspring of an organ possessed by the parents? Heredity, as understood by Weismann, is nothing more than the property in the germ-cell of developing into an individual like that from which it was derived. If each parent possessed a given organ in the same degree of development, a degree unaffected by external conditions, then both the ovum and the spermatozoon will, on this view of heredity, have the property of developing into an individual with the same organ developed to the same degree. When the two properties are combined by fertilization, the fertilized ovum ought to have the property of developing this character with still greater certainty, but why should it have the property of developing the character to a higher degree than that reached in either parent? By *Vermischung*, in its literal sense, the union of the two hereditary powers cannot have this effect. If by *Vermischung* it is meant that the offspring is intermediate between the two parents, then the mean of two equal characters is the same character again. And if this is what Weismann means by *Vermischung*, then a character developed to a certain degree in one parent, and not at all in the other, would in the offspring be developed to exactly half the degree in which it existed in

the one. And so on. But it is obvious that in this way no increase of any character could ever occur.

But of course *Vermischung* may mean something else. It may mean that the hereditary powers of ovum and spermatozoon are added together, that the result of copulation between the germ-cells is not the mean, but the sum, of the properties of both. In this case, evolution would be extremely rapid, for each child would be equal to both its parents rolled into one. If each parent, say, among cattle, had horns equally well developed, the offspring would have horns twice as big. And it is obvious that in this way no decrease could ever occur, for if one parent had an organ developed and the other had no trace of it, the offspring would have it in the same degree as the one.

Now, it seems to me that, if *Vermischung* does not mean either of these things, there is only one other meaning it can have, and that is, that the hereditary powers of the copulating germ-cells reinforce one another to some extent, but not to such an extent that the result is equal to their sum. If this be the meaning, then there can never be any decrease in a character once formed. For, if every individual of a species possesses a certain organ, let us take the hind-legs in a mammal, then if two individuals which have these organs less developed than any other individuals in the species, copulate, the offspring resulting must have hind-legs better developed than either of them. Thus the whale could never have been evolved.

It follows, therefore, that, on Weismann's theory of variation, evolution is impossible. And as acquired characters are not inherited, no other theory of variation can be discovered. Therefore evolution is impossible altogether: the extremes meet, and the Darwinian principle overstrained goes rather to prove the fixity of species than their plasticity.

J. T. CUNNINGHAM.

Mr. Howorth on the Variation of Colour in Birds.

ALLOW me to assure Mr. Howorth that I have no theory to maintain. I simply called attention (*supra*, p. 318) to an overlooked hypothesis, propounded long ago, and, so far as I know, still unrefuted. Neither have I any wish to argue the question. Indeed, controversy about it is happily almost impossible, since he admits the chief fact of which I reminded him to be what he now terms (*supra*, p. 365) "an elementary postulate"—an expression far stronger than I should venture to use; but had he before shown any disposition to recognize it, my remarks had not been written. On the contrary, he implied (*supra*, p. 294) that it was a recent discovery, as it certainly appears to have been to him. I trust he will excuse me for having pointed out its want of novelty, just as he seems to excuse Prof. Geikie for pointing out the antiquity of his views as to the former climate of Siberia; and at the same time I have to ask Mr. Howorth's pardon for demurring to some of the assertions in his last communication, especially that as to the avifauna of Siberia having been "worked out from end to end." I dare not hope to see the day when this shall be done; but then I am not of a sanguine temperament.

I take this occasion to mention that in line 3 of the second paragraph of my former letter (p. 318) the word "Russian" was omitted before "explorers and naturalists." Of course it will be understood to cover Poles, as well as all those foreigners who were employed by the Russian Government.

ALFRED NEWTON.

Magdalene College, Cambridge, February 16.

Currents and Coral Reefs.

MAY I be allowed space to call attention to a remarkable fact relating to the growth of coral reefs, which has apparently (as far as I can ascertain) had no explanation, and which might assist materially in the elucidation of some problems relating to ocean currents about which—although the broad facts are known—a great deal of doubt exists? It might also give us some idea of the flow of submarine currents, the direction of which it is very difficult to determine.

It will be observed that in all coral formations there are in some places remarkable extensions of them from the land, which is not accountable for by supposing the depths only to be shallower in those directions, and the only alternative we have then is that the food supply must come chiefly from that direction, and this supply could only be kept up by currents striking the reef at these points. To give an instance of this, I might

refer to the Bermudas as affording a good example of this action: there, will be seen a great extension of the reefs on the north-west side, and reefs are forming on the south-west—the Challenger and Argus Banks; and it should be noticed that these are on the edge of the Gulf Stream, and also that there are considerable eddy currents here which would cause a constant supply of food to be brought to these parts of the islands; whereas the conditions round the other parts of the islands are not so favourable, and consequently there is no extension of the reefs.

I have been much puzzled by the curious formations of coral reefs in the harbours of Suakim and Massaua and also round the island of Key West; and I have been unable to account for the peculiarities in the shape of the fringing reefs except by an hypothesis such as the above, in these cases the tidal currents taking the place of the ordinary currents.

DAVID WILSON-BARKER.

The Earthquake in Lancashire.

ON Sunday, February 10, at 10.40 p.m., there was felt here a shock as of a heavy falling body, which caused the windows to rattle loudly. Two or three seconds later a second thud-like shock was felt of somewhat greater intensity than the first. This was followed by gentle but distinct tremors, lasting, perhaps, twenty or thirty seconds more. The weather was calm at the time; the heavy snow-fall had just ceased; barometer rising after the considerable depression which had occurred during the day. The sounds appeared to come from the north-east, as if a heavy body had fallen outside a window having that aspect. Several other persons name the same quarter as that whence the sounds seemed to proceed, and in one instance, in a room having several aspects, there was a distinct statement that the north windows were the first to shake, then those in the south-west, thus indicating a possible line of movement. In most cases no direction was noted. Persons down-stairs thought something had fallen above, those in the upper stories rushed down to see what had happened below. Others, again, felt surrounded by the unwonted movement. A heavy slip of snow from the roof seemed to occur, as first thought, to most, then a colliery explosion—there are two coal-beds near—and finally an earthquake.

The chief physical effects observed here, beside the general vibration, were a violent shaking of windows, the rattling of glass and crockery, such as bed-room ware. Suspended gas-shades and pictures on walls swung as if with the wind. The doors of rooms and cupboards opened and shut, one or two ornaments fell from their brackets, and the floor is described as “rising.” Most of those who slept were awakened, and seemed to suffer more alarm than those who had not retired. The movements had apparently been more severely felt by them, and they describe their beds as “rocking,” and themselves being almost thrown out. None of our telephones were affected. Substantial buildings seemed less affected than those less solidly built. The various observers quoted were all in a good position to note what occurred.

T. R. H. CLUNN.

The County Asylum, Prestwich.

Can Animals count?

UNDER this heading, Sir John Lubbock, in his recent interesting book on “The Senses of Animals,” gives several instances of apparent counting in the case of insects. He says:—

“One species of *Eumenes* supplies its young with five victims, one ten, another fifteen, and one even as many as twenty-four. The number is said to be constant in each species. How, then, does the insect know when her task is fulfilled? Not by the cell being filled, for, if some be removed, she does not replace them. . . . In the genus *Eumenes*, the males are smaller than the females. . . . In some mysterious manner the mother knows whether the egg will produce a male or a female grub, and apportions the quantity of food accordingly. She does not change the species or size of her prey; but if the egg is male, she supplies five, if female, ten, victims. Does she count? Certainly this seems very like a commencement of arithmetic.”

Now, it seems to me that this can be explained in a far simpler and more probable manner than by supposing that insects have any power of counting. I think we may safely consider—

(1) That a certain *average amount* of food is required in each case.

(2) That a certain *average time* is required by the insect to collect this food.

(3) That the eggs of the insect follow one another at a *certain rate*, over which she has little or no control.

(4) That the eggs which are to produce *males*, being smaller, take less time to form, and follow at shorter intervals, than do those which are to produce *females*.

Now take the case of the *Eumenes* which provides ten victims. She makes the cell, and goes on adding caterpillars until the egg comes to maturity and is laid, and the cell finished off. She then repeats the process, laying the egg when it comes to maturity, as before; the interval between the laying of one egg and the next being long enough, on the average, to provide ten victims, or in case the egg is to produce a male, the smaller interval only allows of five being provided.

There is thus no need to credit the insect with any power of *counting*, or of knowing beforehand anything about the sex of the eggs. It is merely another instance of the perfect way in which, by the process of evolution, means are adapted to ends.

The same explanation applies to the cases mentioned on pp. 254–56 of “The Senses of Animals.” The bee laboured to fill the cell (in which a hole had been made so that the honey ran out again), until, “when she had brought the usual complement of honey, she laid her egg, and gravely sealed up the empty cell: . . . in some mysterious manner the bee feels when she has provided as much honey as her ancestors had done before her, and regards her work as accomplished.”

I should suggest that the bee merely goes on bringing honey until the *egg is ready*. She then starts another cell, and goes through the same routine until the next egg is ready, and so on; the average amount of honey collected being proportional to the interval between the laying of one egg and the next. According to the theory of evolution, this interval is just sufficient for enough food to be provided for the use of the grub.

G. A. FREEMAN.

St. Olave's Grammar School, Southwark.

Weight and Mass.

MR. ANDREW GRAY need only turn to p. 355 of the current number of *NATURE* to find an example of an engineer's dynamical terminology, and it would tend to some useful result in this interminable discussion if he would point out in what manner the language of the engineer must be modified to suit the requirements of the *precisionist*. We find on p. 355 “the working pressure is 175 pounds per square inch,” “the total weight of the engine in working order is 37 tons,” “probably having about 30 tons useful weight for adhesion,” and so on.

Let Mr. Andrew Gray point out what he considers the errors of the engineers. Ought the engineer to say, “the working pressure is 175 pounds weight (or pound weights?) per square inch,” “the total mass of the engine is 37 tons,” “probably having about 30 tons useful (? mass or weight) for adhesion.”

On former occasions in this controversy I have attempted to elicit definite expressions of opinion on the terminology of similar definite actual dynamical problems, but hitherto without success.

A. G. GREENHILL.

Woolwich, February 12.

Detonating Meteor.

MR. MAXWELL HALL's letter on this subject is of considerable interest. When the great meteor-shower of November 11–15 was traced to the orbit of the comet of 1866, it was natural to suppose that the fine fire-balls which occur about this period belong to the same series. Plainly, however, the Jamaica fire-ball recorded by Mr. Hall had a southern radiant far distant from that of the well-known Leonid shower. The same thing was noticed in the case of the shower of aërolites which fell in France on the 13th of November, 1835, the motion being south-east to north-west; and in many other instances in which fire-balls or aërolites were observed within this period the phenomena seem to agree best with a southern radiant, though the descriptions are not as full as we could desire. It thus appears highly probable that almost coincident in time with the well-known Leonid shower there is another shower, rich in fire-balls and aërolites, proceeding from a southern radiant. I hope it will be watched from southern stations in future.

W. H. S. MONCK.

Dublin, February 15.