Bernoulli's numbers, and on points connected with definite integrals.—Prof. Wolstenholme's papers are concerned with series and loci, and treat also of epicycloids and hypocloids.—Mr. T. Cotterill gives a short paper on an algebraical form and the geometry of its dual connection with a polygon, plane or spherical.—An analogous theorem relating to polyhedra is discussed by Prof. Clifford in this same volume.—M. Hermite contributes two short notes, one on circular functions, the other on unicursal curves.—Mr. J. J. Walker writes on the invariant conditions of multiple-concurrence of two conics, and Mr. R. B. Hayward on an extension of the term *Area* to any closed circuit in space.—From this analysis it will be seen that there is considerable variety in the contents of the volume. It is not necessary here to give any detailed account of the papers, as notices of them have appeared from time to time in our columns.

LETTERS TO THE EDITOR

The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Flowers of the Primrose destroyed by Birds

I HOPE that you will permit me to make a few final remarks on the destruction of primrose flowers by birds. But first I must return my best thanks to your correspondents, as well as to some gentlemen who have written direct to me, and to whom I have not had time to send separate answers. Secondly, I must plead guilty to the high crime of inaccuracy. As the stalks from which the flowers had been cut were shrivelled, I mistook, in a manner now inexplicable to me, the base of the ruptured or removed ovarium for the summit; a remnant of the shrivelled placenta being mistaken for the base of the pistil. I have now looked more carefully, and find that on twelve stalks only three had any remnant of the ovarium left. I have also examined sixteen bits of the calyx which had been cut off by a caged bullfinch, presently to be noticed, and in fifteen of these not only had the ovarium been torn into fragments or quite destroyed, but all the ovules had been devoured, excepting sometimes one or two. In several cases the calyx had been split open longitudinally. The ovarium was in the same state in thirteen small portions of the calyx lying on the ground near a wild cowslip plant. It is therefore clear that the ovules are the chief attraction; but the birds in removing by pressure the ovules could not fail to squeeze out the nectar at the open end, as occurred when I squeezed similar bits between my fingers. The birds thus get a dainty morsel, namely, young ovules with sweet sauce. I still think that the nectar is, in part, the attraction, as caged bullfinches and canary birds much like sugar; but more especially because Mr. C. J. Monro has sent me some flowers from a cherry-tree near Barnet, which during several years has been attacked; and he finds many of the flowers, both those on the tree and on the ground, with rather large ragged holes in the calyx, like, but much larger than, those often made by humble bees when they rob flowers in an illegitimate manner. Now the inside of the flower of the cherry, round the ovarium, is bedewed (if protected from the visits of insects) with drops of nectar, which sometimes collect so as almost to fill up the bottom of the flower. In the case of the cherry I cannot doubt that this is the attraction, for I examined the ovarium of ten flowers, and although they had all been scored by the bird's beak, and in four instances punctured, the ovule had in no case been devoured.

To return to the primroses: from the accounts received, it seems that the flowers are cut off in the manner described by me, near Preston in Lancashire, in North Hampshire, Devonshire, and Ireland, as well as in Kent. In several other places, not worth specifying, where primroses are abundant, they have not been thus attacked; and this may possibly be due to the proper enemy, namely, as I now suspect, the bullfinch, not being a common bird. In my former letter I remarked that if the habit of cutting off the flowers proved to be a widely extended one, we should have to consider it as inherited or instinctive; as it is not likely that each bird should discover during its individual lifetime the exact spot where the nectar, and, as I must now add, the ovules, lie concealed, or should learn to bite off the flower so skilfully at the proper point. That the habit is instinctive, Prof. Frankland has given me interesting evidence. When he read my letter he happened to have in the room a bunch of cowslip flowers and a caged bullfinch, to whom he immediately gave some of the flowers, and afterwards many primrose flowers. The latter were cut off in exactly the same manner, and quite as neatly, as by the wild birds near here. I know that this is the case by having examined the cut-off portions. The bird worked so quickly that he easily destroyed twenty flowers in three minutes; a single wild pair would therefore cause great havoc. Prof. Frankland informs me that his bird pressed the cut-off portions of the calyx in its beak, and gradually worked them out on one side, and then dropped them. Thus the ovules were removed, and the nectar necessarily squeezed out. A canary bird to whom some cowslip and primrose flowers were given attacked all parts indiscriminately, and ate up the corolla, calyx, and stalks. A lady also informs me that her canary and siskin always attack primrose and cowslip flowers, if kept in the same room. They generally first make a ragged hole through the calyx opposite the ovarium, and remove the ovules, as I found to be the case with flowers which were sent to me; but the ovules had not been so well removed as by the bullfinch, and the nectar could not be obtained by this method of attack.

But now comes the interesting point: the caged bullfinch just referred to was caught in 1872 near Ventnor, in the Isle of Wight, soon after it had left the nest, by which time the primroses would have been out of flower, and since then, as I hear from Prof. Frankland, it had never seen a primrose or cowslip flower. Nevertheless, as soon as this bird, now nearly two years old, saw these flowers, some machinery in its brain was set into action, which instantly told it in an unerring manner how and where to bite off and press the flowers, so as to gain the hidden prize. We are reminded by this little fact of Mr. Spalding's admirable observations on the instinctive actions of chickens when their eyes were uncovered, after having been blind-folded from the moment of being hatched.

Prof. Frankland seems to have been much struck with the behaviour of his bullfinch, and remarks in his letter that "it had all the precision of a chemical reaction; the result of putting a primrose within its reach can be almost as certainly predicted as that of putting a plate of iron into a solution of sulphate of copper."

CHARLES DARWIN

Down, Beckenham, Kent, May 7

P.S.—This letter was printed before I saw your last number, and I am glad to find that some of my statements are confirmed, more especially with respect to bullfinches. During the last fortnight not one primrose has been attacked in the little wood where shortly before there was such havoc. I imagined that the pair of bullfinches, which I saw there earlier in the season, had wandered away; but yesterday evening (May 10) it occurred to me that the flowers produced late in the season might fail to secrete nectar, or that the recent cold weather might have produced this effect. Accordingly, in the afternoon I gathered fifteen flowers from as many distinct plants, and kept them in water in my room for seventeen hours. Earlier in the season I treated some flowers in this same manner, and found the tube of the corolla full of nectar; but now only one of the flowers contained a very small quantity of nectar, another showing a

mere trace of it. And the flowers being no longer cut off by the birds supports my belief that the nectar is one chief attraction to them; the ovules without the sauce not being worth the gathering. I may add that as the primrose is a dimorphic plant, these non-nectariferous flowers would be sterile, for they would not be visited by insects.—C.D.

Mr. Spencer and *à priori* Axioms

I QUITE agree with Mr. Spencer that argument between us will not be to much purpose; but it should be noted that my principal "exemplification of unconsciously-formed preconceptions" was of Mr. Spencer's own choosing, namely, Newton's "Second Law of Motion," which, if I understand him aright, may now be described as "a consciously-formed hypothesis concerning the relation between weight (force?) and motion." Only demurring to the word "hypothesis," and leaving it to Mr. Spencer to reconcile this with his former declaration that the law in question is an "immediate corollary" of one of these unconsciously-formed preconceptions, it seems to me there is little left to argue about. ROBT. B. HAYWARD

Harrow, May 8

MR. SPENCER does not state his arithmetical illustration very exactly. He implies that there is a certain truth which the savage is incapable of understanding concerning which the schoolboy makes a mistake, but that there is present in the civilised adult a consciousness of its logical necessity. It does not appear distinctly what that truth is.

The most obvious interpretation of what is printed is, that Mr. Spencer refers to the local value of figures in the Arabic system of notation: this is probably not what is meant.

Two other interpretations suggest themselves. The sum of seven and five is the same number whatever be the things to which the seven and five refer; or else the more particular statement that the sum of seven and five is the same as the sum of ten and two. It is not apparent that either of these is intended.

To say that seven and five make twelve without implying something about twelve other than the statement that it is seven and five, seems a proposition so purely verbal that it is difficult to see how the recognition or non-recognition of it illustrates the grounds of belief in physical laws.

NOT A METAPHYSICIAN

The Glacial Period

IN the many kind and favourable reviews of my book, "The Naturalist in Nicaragua," exception has been generally taken to my speculations on the extent and effects of the ice of the glacial period. The subject is a large one, and too little of my time can be given to scientific inquiry to allow me to hope that I can deal with it in detail for some years to come; but as it appears that I have not expressed my views with sufficient clearness and have been misunderstood by some of my critics, I shall be glad of an opportunity to state them with distinctness and brevity.

1. At the present sea-level, the ice extended, in the northern hemisphere, from the Pole to lat. 39° in America, to about the valley of the Thames in England, to lat. 50° in central Europe, and to lat. 52° in north-western Asia. Along the high lands of America it reached to the tropics, and in Central America all the land lying over 2,000 ft. above the sea supported glaciers. I do not contend that the present low lands of tropical America were ever covered with ice, and it is on the mountain chains of that continent alone that I believe it nearly reached to the equator.

2. The ice was thickest over the American continent, not because it was coldest there, but because the great evaporating area of the Pacific lay to the south-west of it and the counter trade-wind swept across it and precipitated the moisture with which it was laden. Siberia was equally cold, but the upper moisture-bearing currents of air were intercepted by the Himalayas, the Kuen Lun, and the Altai Mountains. It was thickest in America for the same reason that it is thicker on the summits of the Pyrenees than on similar heights on the Caucasus, and thicker on the southern than on the northern slope of the Himalayas, not because of greater cold, but of greater precipitation.

3. The immense accumulation of ice in the extreme north of America and Europe must have overflown and filled the polar basin even if it had not independently collected there; but the precipitated moisture would not have frozen on the continents if the climate had not been much colder then than now; and the surface of the Arctic Ocean must have been frozen over, and as capable of sustaining accumulations of snow as the solid land itself, even if that ocean was not displaced by the ice flowing into it from the northern extremities of the continents.

4. Probably the ice was not thickest at the Pole, but formed a ridge of varying height at unequal distances from it; for, as we have seen, it would not be thickest where it was coldest, but where there was most precipitation, and the south-west winds would part with their moisture long before they reached the Pole.

5. Whilst we can follow on the land the marks left by the ice of the glacial period, and map out its former boundaries, we can only speculate on its extent over the areas now covered by the sea. We have, however, some evidence. The Hebrides and the extreme north-east of Scotland were overflown by ice that came from the north-west, and the bed of the North Atlantic must have been filled so far at least, or to about lat. 59°; and taking into account the much greater quantity of ice lying on America than on Europe, it is not an extreme supposition that on the western side of the Atlantic the bed of the ocean was occupied by ice to lat. 45°.

6. One of the principal effects of this great advance and accumulation of ice, not yet taken into consideration by geologists, was an interruption to the drainage of all countries whose rivers flowed northwards. The great plain of Siberia was, I believe, occupied by an immense lake caused by the blocking up of the whole of the watershed to the north. In western Europe this interference with the drainage of the land took place, even if we do not accept the theory of an ice-cap, but hold with some geologists that the ice descended only from existing chains of mountains. All the rivers of northern Germany must have been dammed back by the ice descending from the Scandinavian mountains. One of the most important changes was effected in the German Ocean. Its northern half was filled with ice, from the mountains of Norway and Sweden, from Scotland and northern England. As we know that at this time the Straits of Dover did not exist, it is evident that the southern portion of the bed of the German Ocean must have been filled by a great fresh-water lake, varying in extent during the advance and retreat of the ice, into which flowed all the water of the melting ice, and all the rivers that now run into the same area.

7. There is no satisfactory evidence of the intercalation of a warm period between two glacial ones, though doubtless there was more than one retreat of the ice, during which a temperate climate prevailed in regions glaciated before and afterwards. The intermingling of the remains of northern and southern mammalia in the gravels of south-eastern England arose, probably, as explained by Sir Charles Lyell, by a northern and a southern fauna having migrated to the district at different seasons of the year.

When the German Ocean was blocked up to the north by ice, a great river must have run to the south through what are now the Straits of Dover and the English Channel, receiving into one stream the waters of the Rhine, the Thames, the Humber, and the Somme. How far that river ran southward would depend upon the relative heights of the land and the sea. It must have run into a comparatively warm ocean, for the effects of the warm currents of water coming from the tropics, instead of as at the present time entering the polar basin, would be confined to and intensified in more southern latitudes, and they would then, as now, be deflected upon the western coast of Europe. Up this river the hippopotamus and the southern species of rhinoceros and elephant may have come in summer and autumn, whilst the mammoth, the woolly rhinoceros, and the musk ox came from the north in winter.

8. The theory of the damming-up of many rivers throws much light on the difficult question of the formation of the high and low level gravels and the loess. The lake occupying the area of the German Ocean must have stood much higher in spring and early summer than it did later on in the year and in winter; and the levels of the lower parts of the rivers running into it must have been affected by its rise and fall. If we can suppose that the hippopotamus only came up the river when it was low in the latter part of summer, or in the autumn, we can understand how its remains are only found in the low-level gravels of the Thames and the Somme; though it is also possible that they may belong to a later and milder period when the ice had retired so far back that the great lake partly drained to the north around Scotland.

9. The glacial period probably existed in both hemispheres at the same time. First, because we can trace the evidence of the existence of ice along the high lands of America into the northern tropics until it nearly inosculates with that coming down