

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 *a 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 overflowed 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 overflowed 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 freshwater 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 insculcates with that coming down

from the south, and there is no difference in the character or appearance of the moraines left on both sides of the equator. Second, because, excepting on the supposition that the ice extended, at least along some meridians, both from the south and the north nearly to the equator, at the same time, we cannot explain the distribution of those animals and plants that are found in the temperate zones of both hemispheres, separated by the whole width of the tropics, over which they cannot now pass. For example, there are more than forty flowering plants of North America and Europe which are also found in Terra del Fuego. Darwin's theory that these plants were driven to the high lands of the tropics during the glacial period, and followed the retreating ice in its retrocession, must fall to the ground if the ice did not exist in both hemispheres at the same time. (See "Origin of Species," p. 405, &c.)

10. The piling-up of water around the poles in the form of ice could not fail to affect the level of the ocean. Mr. Alfred Tylor has calculated that the accumulation of the ice in the northern hemisphere alone would abstract so much water as to lower the level of the sea 600 feet; and if, as I believe, the glacial period occurred at the same time in both hemispheres, the level of the ocean must have been lowered at least 1,000 feet.

11. The theory of the lowering of the level of the sea during the glacial period is directly opposed to the generally accepted one of a great submergence of part of England and Scotland to a depth of about 2,000 feet, when the marine shells of Moel Tryfaen and Macclesfield were deposited. The facts on which this theory of submergence is based can be otherwise explained. The shells are broken or worn, and generally mixed amongst other transparent materials. They are just where they ought to be found on the supposition that an immense body of ice coming down from northern Ireland, from Scotland, and from Cumberland and Westmoreland, filled the basin of the Irish sea, scooped out the sand with the shells that had lived and died there, and thrust them far up amongst the Welsh hills that opposed its course southward and around the great bight of which Liverpool forms the apex. Excepting some raised beaches around our coast, which were probably formed after the glacial period, and in no case reach more than 100 feet above the present level, I believe there is no evidence of the submergence of Great Britain either during or since the glacial period.

THOMAS BELT

#### Lakes with two Outfalls

THE subject of double outfalls is of some interest, if only as showing the necessity of accurate observation, and the difficulty of ascertaining the truth in matters apparently of simple fact. In NATURE, vol. ix. p. 485, Mr. W. B. Thelwall brings forward two instances of lakes with double outfalls, and states that he has passed two or three more. Now, as regards that upon the Fille Fjeld, which he describes from personal observation, I beg entirely to call in question his accuracy. I passed the locality during each of the two last summers, and my attention was drawn to the position and nature of the watershed, especially during my visit of last summer, when I had carefully inquired into the asserted existence of a natural double outfall at the Lesjeskaagen Vand. (See NATURE, vol. viii. p. 304; also Colonel Greenwood's and Mr. R. B. Hayward's letters, NATURE, vol. viii. p. 382.)

Mr. W. B. Thelwall says:—"Between Nystuen and Skogstad is a chain of lakes crossing the watershed, the highest of which (not the one marked on the Vei-cart over Norge, I think), sends its waters to the west, past Nystuen to the Sogne Fjord, at Lærdalsören, and on the east by the Lille Mjösen, and Aadalen to the Tyrifjord, and so past Drammen to the Christiania Fjord. This lake is a small one, and the double outflow is close to the high road."

Now this statement is inaccurate in all the essential details. The division of the waters is *not* between Nystuen and Skogstad, but on the other side of Nystuen between it and Maristuen. The water which passes Nystuen does *not* flow towards the west to the Sogne Fjord, but to the east towards the Lille Mjösen, as I carefully ascertained when I was staying at Nystuen. This is rendered certain, too, by the fact that the land rises to the west of Nystuen, the actual division of the waters being about 100 or 105 feet, by my aneroid barometer, above Nystuen. Moreover, having scrambled up a steep mountain close behind Nystuen, whence the view on a clear day is of the wildest character, I had a bird's-eye view of the whole district in debate, and examined it carefully with a good field-glass, with a view to detecting any

evidence of a double outflow. I came to the conclusion that the division of the waters took place in the boggy bottom of the valley to the west of Nystuen, and that it would be impossible to say exactly where it was. To the westward of this boggy place is indeed another lake, of which the waters flow to the Sogne Fjord; but this lake is several miles to the west of Nystuen, and separated from it by dry land, rising 100 feet or more above the levels of the water in the two lakes.

Whether lakes with two outflows exist or not, it is difficult to avoid feeling that Colonel Greenwood was warranted in his former incredulity upon the subject. W. STANLEY JEVONS

#### Trees Pierced by other Trees

UNDER this heading your correspondents discuss two distinct questions as if they were the same, namely the piercing of the stem of a tree by the head of another, as supposed by Mr. Murphy, and the growth of the *root* of a plant in or on another tree. Nothing can be more common than this last. Wherever soil aggregates the roots of seeds will grow as a matter of course. More than this, trees will strike roots into soil collected in their own forks, as I can show here, or down the rotten wood of their own trunks. A remarkable case of this may be seen in a yew tree in West Tisted churchyard near here. But nothing can be more opposite than the growth of the root and that of the head. The root grows to darkness; the head to the light.

Alresford, May 11

GEORGE GREENWOOD

[This correspondence must now end.—ED.]

#### The supposed Antipathy of Spiders to Chesnut Wood

SOME years back, while walking in the cloisters of New College, I remember a resident Fellow (since deceased) telling me that spiders were never known to occur in the woodwork of the roof, and attributing their absence to the chesnut timber, of which it was framed.

It has been asserted that this wood, which was formerly supposed to be that of the chesnut, really belongs to *Quercus sessiliflora*, but I do not know if that is still held to be the case.

The roof of Westminster Hall was at one time considered to have been constructed of chesnut; has any such story been heard of in connection with it? R. A. PRYOR

13 Bury Street, S.W.

#### AN EXPERIMENTAL OBSERVATION ON HAY FEVER\*

THE accompanying brief but most interesting paper was received a day or two ago. Believing that it may bring relief to those who during the coming warm weather may be attacked with hay fever, Prof. Tyndall forwards it, with his compliments, to the editor of NATURE.

From what I have observed (says Prof. Binz) of recent English publications on the subject of hay fever, I am led to suppose that English authorities are inaccurately acquainted with the discovery of Prof. Helmholtz, as far back as 1868, of the existence of uncommon low organisms in the nasal secretions in this complaint, and of the possibility of arresting their action by the local employment of quinine. I therefore purpose to republish the letter in which he originally announced these facts to myself, and to add some further observations on this topic. The letter is as follows†:—

"I have suffered, as well as I can remember, since the year 1847, from the peculiar catarrh called by the English 'hay fever,' the speciality of which consists in its attacking its victims regularly in the hay season (myself between May 20 and the end of June), that it ceases in the cooler weather, but on the other hand quickly reaches a great intensity if the patients expose themselves to heat and sunshine. An extraordinarily violent sneezing then sets

\* By Prof. Binz, of Bonn.

† Cf. Virchow's *Archiv*, vol. xli. p. 100.