

or Corunna, called anciently Portus Brigantinus, in Galicia." According to this the Briges would be the common ancestors of the Britons of England, France, and Spain, and the similarity of the names in these countries could be accounted for.

A. R. H.

Faunas of Oceanic Islands

IN NATURE, of February 16, I observe the reviewer of Mr. Godman's "Natural History of the Azores," makes the following statement:—"Mr. Godman appears to be the first who has, after a personal exploration of one of these oceanic groups, endeavoured to collect all that is known of its natural productions." Now, it is not with the intention of detracting from the merits of Mr. Godman's work, but merely to refresh the memory of your reviewer, that I beg to state, that I published in 1859 a small volume entitled "The Naturalist in Bermuda," which contained all that was known of the natural productions of those islands at that date. It was by no means a complete essay, but as stated in the preface "merely a prelude to a more complete publication on the same subject, which anticipated work, the result of several visits to the group, I hope to present to public notice shortly."

J. MATTHEW JONES

Institute of Natural Science, Halifax, Nova Scotia

Influence of Barometric Pressure on Ocean Currents

IN the recent discussions on the influence of barometric pressure upon ocean currents, I have not seen any allusion to the observations that have been made upon the effect of variations of barometric pressure upon the sea-level. In a memoir by M. Ch. Aimé, "Sur les variations de niveau de la Méditerranée," in the *Annales de Chimie*, tome xii., 1844, it is stated that a fall in the barometer is pretty uniformly accompanied by a rise in the sea-level to about thirteen times its amount. The Report of the British Association for 1841 contains a letter from my old friend, Mr. T. G. Bunt, of Bristol, stating that his observations upon the tide-gauge under his charge led him to conclude that a fall of one inch in the mercurial column was accompanied by an average rise of about $13\frac{1}{2}$ inches in the high-water level. And the same industrious and careful observer, in a recent "Discussion of Tide Observations at Bristol" in the *Philosophical Transactions* for 1867, gives as the mean result of twenty-one years' examination of this point, "12.772 inches of tide to one inch of mercury." I referred to Mr. Bunt's observations in a discussion at the Geological Society (March 6, 1867) on a paper by the Earl of Selkirk "On some sea-water-level marks on the coast of Sweden," pointing out that some of the discrepancies in the observations as to the sea-level of the Baltic might be attributed without improbability to variations in barometric pressure. I have since learned from Admiral Key, who served in the Baltic fleet during the Russian war, that he had been led by his own observations to a like conclusion. And I find it stated in the description of the Baltic Sea, in the English *Cyclopædia*, that its level is sometimes observed to rise, and to remain thus elevated for a time without any obvious cause, two or three feet, of which phenomenon the explanation is probably the same.

I am sorry to find that I have not succeeded in convincing Mr. Laughton of the existence of a regular undercurrent in the Strait of Gibraltar. If he will take the trouble of carefully perusing the detailed report which I have presented to the Royal Society, he will find that he is quite in error in stating that I rest my affirmation upon "one observation after several attempts made in vain." All our observations, when rightly interpreted, tended to the same conclusion. The reduction of the boat's drift almost to nothing, in the first set of experiments, when it lay in a surface-current running nearly three miles an hour, with a breeze setting in the same direction, was just as conclusive evidence that a reverse current must have been acting on the current-drag below, as was the reversal of the boat's drift in the subsequent experiment, when the surface-current was less rapid and the opposing breeze diminished its action on the boat. And our observations of the temperature and specific gravity of the 250 fathoms' stratum most unmistakably indicated on both occasions its Mediterranean derivation.

I should like to know what is the precise minimum of movement which is held by Physical Geographers to constitute a current. There seems to me a great deal of confusion upon this point. The existence of an underflow of polar water towards the Equator cannot now be a matter of question. Commander Chinn has recently obtained with the Miller-Casella thermo-

meters a temperature of $33\frac{1}{2}^{\circ}$ at a depth of 2,306 fathoms nearly under the equator. What is the rate of this movement is a point as yet undetermined. But the rate of the northerly flow of warm surface-water between Scandinavia and Iceland, which is usually attributed to the Gulf Stream, but which I regard as the complement of the southward flow of deep polar water in a vertical oceanic circulation, is estimated by Admiral Irminger at from $1\frac{1}{2}$ to $2\frac{1}{2}$ miles per day. Is this, in the language of physical geography, a current?

WILLIAM B. CARPENTER

University of London, April 10

The "Times" Review of Darwin's "Descent of Man"

THE British public are deeply indebted to the *Times* Reviewer for his very comforting and reassuring remarks on Mr. Darwin's "Descent of Man," in which he has so well exposed the "utterly unsupported hypotheses," the "unsubstantial presumptions," the "cursory investigations," of that "reckless" and "unscientific" writer. It is a great satisfaction to find that Mr. Darwin's odious conclusion that the genealogy of the Talbots, and the Howards, and the Percys must be traced back beyond the Conqueror to an Anthropomorphous Ape, and beyond the ape to an Acephalous Mollusk, rests on no logical foundation whatever. The Reviewer well suggests that anything so odious in idea, so immoral in its apparent tendency, and so different from what we have been accustomed to believe, cannot possibly be true. One is so glad indeed to be free once and for ever from the mischievous influence of such "unpractical," "disintegrating speculations," that it seems worth while trying, if space can be found for the experiment, to elicit from the good nature of the Reviewer, or of those who think with him, a little clearer explanation here and there, before the subject is finally consigned to a well-merited oblivion.

Mr. Darwin is invited in one passage, "if he wishes to corroborate his hypothesis, to commence by experimenting on some superior kind of Ascidian, and see whether, by patient selection, he can induce any of them to split themselves in half, and abandon their permanent support for a vagrant oceanic existence." Now, it is a fact that among Corals or Polypes, which are not far removed from Ascidians, these interesting experiments are actually exhibited; for the *cæspitose* Corals, by what is called fissiparity, do split themselves in half, thus forming two complete individuals where only one grew before, and the Corals of the genus *Fungia* are fixed when very young, but subsequently break their pedicels and become free. The whole group of Zoophytes, recent and fossil, connects together marvellously different forms by an almost infinite series of wonderfully minute links. The study of such a group is therefore no doubt dangerous, if not decidedly pernicious, as tending to gloss over "the enormous and painful improbability" of Mr. Darwin's speculations. For if upon examination it seemed likely, or almost certain, that different genera of Polypes were connected with one another by descent, some rash enthusiast might think a similar conclusion not impossible in the order Primates. Fortunately, one is estopped from suggesting that in fact some genera of Polypes may be connected by descent, for fear of incurring the sharp reproach to which Mr. Darwin has so frequently laid himself open, of "conjugating the potential mood." Hitherto in most departments of thought and inquiry, probable evidence has been allowed to count for something, and most men are content to believe themselves to be the sons of their reputed fathers upon a mixture of evidence and authority, which, by the very nature of the case, can never rise to absolute demonstration. The Reviewer has done good service to society by showing the untrustworthy character of the foundation on which all our genealogies are built. It would be well in future if some auxiliary verb, expressive of doubt and uncertainty, could be combined with our patronymics.

Mr. Darwin, it appears, has "a facile method of observing superficial resemblances." For instance he surprises the apprehension of the vulgar by exhibiting the curious likeness between the embryos of a man and a dog. As every one of course knows how he looked when he was still in his mother's womb and less than an inch long, that stage in a man's career when he is only too like an embryo puppy, might have been shrouded under a delicate reserve. If, in place of this absurd "superficial resemblance," Mr. Darwin could have pointed out similarities between man and the lower animals in regard to minute structures of bone and muscle, or in the organs of sense or speech, his argument might have been deemed a little more scientific.

Persons who have read his book say that he does dwell with considerable force upon these very matters, but it is easy to see from the Reviewer's tone that they are mistaken, and that such investigations have been sacrificed to a glance or two at things on the surface. This is the more to be grieved and wondered at, because in his monograph on the Fossil Cirripedes and in his work on the Fertilisation of Orchids, Mr. Darwin showed an uncommon aptitude at "a thoroughly scientific *clairvoyance*."

The Reviewer thinks it perfectly reasonable that the hand of a man and the foot of a horse, the flipper of a seal and the wing of a bat, should have all been formed upon the same general plan, without any connection by a common ancestry. It would be extremely gratifying to an inquiring mind, if he would explain upon this reasonable plan, the vast succession of creatures unveiled by geological research. Why have innumerable species been created and then destroyed? When did the creation begin, and when did it end? What causes, or if there were no causes properly so called, what caprice brought about the extinction of the mammoth, and led to the introduction of the modern species of elephant? Has the creative power been at last exhausted, or do sudden creations still occur, only in a shy sort of way, when no one is looking on? The Reviewer very sensibly censures Mr. Darwin and his followers for not specifying the year B.C. when the process of evolution first began. It is with the less diffidence, therefore, that a question is propounded above as to the date of the creation. The solution of this point of chronology will be awaited by many with extreme impatience, as different nations give very different accounts, and the Hebrews, who have a very ancient record, are by no means at one with themselves in the Hebrew and Greek editions of it. The number of years required for the process of evolution is confessedly indefinite, and as the whole hypothesis must, therefore, be destitute of any scientific value, it is no doubt quite fair on the Reviewer's part, to represent an indefinite number of years as equivalent to "infinite time." But the steps required for the process are also an indefinite number, and on this point he is less clear than elsewhere, for, referring to the old sophism respecting Achilles and the tortoise, he tells us, from Sir Isaac Newton, that "quantities ultimately coincide which may be proved to approach each other indefinitely, *within a finite time*." From this it would seem that, if Darwinians could be content with the boundaries of geological time, the genealogies of men and apes might ultimately coincide. To avoid this miserable and preposterous conclusion, we are told that the solution of the sophism by Diogenes, "is the only true one," *solvitur ambulando*. We are further obligingly informed that this solution is identical with Newton's. And as Mr. Darwin cannot transform one species into another under our eyes, the eminently unpractical character of his speculations is triumphantly exhibited. It will be very impertinent if any one suggests that the instantaneous creation of a species has never yet been witnessed, and that those who believe in such occurrences ought, on the *solvitur ambulando* principle, to favour the world with, at least, one such exhibition. Captious persons may find fault with the Reviewer's opinion that the poetic faculty has received no development since Homer, and the religious sentiment none since the book of Genesis. They may call to mind that Moses and Socrates, and St. Paul and Luther, were guilty, like Mr. Darwin, of laying before popular audiences dangerous and "disintegrating" speculations; they may fancy that truth is worth discovering, even when it seems to involve some contradiction to our pride and some loss of comfort to our finer feelings, but such persons must be very captious, and the Reviewer will, doubtless, know how to deal with them.

Torquay, April 15

THOMAS R. R. STEBBING

Sexual Selection

In the first volume of "The Descent of Man," at page 396, Mr. Darwin says, referring to butterflies, that "the lower surface (of the wings) generally affords to entomologists the most useful character for detecting the affinities of the various species." I think, also, that this lower surface might afford another link in the chain of argument by which Mr. Darwin supports his theory of Sexual Selection. Thus, for example, to speak of British species only, in the cabbage butterflies, the under surface of the wings is alike in both sexes of *Pieris Brassicae*. The black spots, however, which appear on both surfaces of the fore wing of the female vanish from the upper surface of that of the male, probably because the female has some dislike to them. There is no difference in food-plant, habit, or need of protection here;

the only explanation seems to be a whim of the female or a whim of nature, and we have lately discarded all thought of nature being freakish. In *P. Rapae* and *P. Napi* a similar difference prevails, though less constant and in a degree less marked. In the allied *Anthocharis Cardamines* the under surface of both sexes is alike, notwithstanding the vast difference of their upper surfaces. When these butterflies alight and close their wings, the under surfaces of the *hind wings* are alone visible, and these are, apparently, the parts of the insect modified for the sake of protection. The simple yellow in *Brassicæ* and *Rapae*, the green-veined yellow in *Napi*, the green marbling in *Cardamines*, of the under sides of the hind wings, are well fitted to conceal those insects as they settle on the wild flowers which they prefer.

Again in *Hipparchia Janira* the light brown patch so conspicuous on the upper surface of the fore wing of the female vanishes from that of the male; and in *H. Titonus* and *H. hyperanthus* a tendency to decrease the quantity of light colour on the upper surface of the male butterfly prevails. So is it also with one of the Hair Streaks, *Thecla Betula*, the under surface still remaining alike in both sexes of these different species. In this case the female butterflies would seem to wish their partners to be of a dusker hue than it is granted to themselves to be. The differences mentioned above are so slight that Mr. Darwin says at page 317, "With those (butterflies) which are plain-coloured, as the meadow-browns (*Hipparchia*) the sexes are alike." But it will be admitted that though these differences are slight they are yet important, as showing a tendency, more or less marked, to follow the rule which Mr. Darwin has laid down; and every sign of such a tendency strengthens his case.

In *Apatura Iris* the under surface of both sexes is alike, though the male has his upper surface glorified with purple for the delight of his plain brown wife. In the blues, *Polyommatus Alexis*, *P. Corydon*, *P. Adonis*, and *P. Egon*, the under surface of both sexes is also alike, though in the males the blue and in the females the brown of the upper surface forms the background of the spotty design. The blue blood is very strong in these butterflies, and will show itself sometimes even in the females; who, if powerless over their own decoration, have at least succeeded in bringing out the innate splendour of their handsome husbands. With the blues, as with the cabbage butterflies, the under surface of the hind-wings seems specially adapted for protective purposes; every butterfly-hunter knows how difficult it is to distinguish the common blue when it is sitting, shut up, on a scabious flower. It is the same with the small copper butterfly, which has its under surface dotted very similarly. But burnished copper and dazzling blue are not colours for protection, surely. We may give the under surface to Mr. Wallace, but we must yield the upper surface to Mr. Darwin.

At page 399, speaking of the ghost moth (*Hepialus humuli*) and others of the moth kind, Mr. Darwin says, "It is difficult to conjecture what the meaning can be of these differences between the sexes of darkness or lightness; but we can hardly suppose that they are the results of mere variability, with sexually-limited inheritance, independently of any benefit thus derived." The female ghost moth follows Mr. Darwin's rule, that females are most conservative of the features of kinship. In her colouring she closely resembles the other *Hepialide*. And the male, notwithstanding his shining shroud, keeps to the same sober under-colouring as his mate. Now *H. humuli* is more nocturnal in its habits than any of the other species in the genus *Hepialus*; I have caught *H. hectus* and *H. lupulinus* flying in bright sunshine, but I have never seen the ghost moth until dusk was far advanced. May it not be that sexual selection has come into play here by the female preferring the *whitest* male, he being the most distinguished when all colour has faded into dimness? She could not decide between differing patterns of gold and amber at that hour, but a snow-white surface would then be quite visible. The fact mentioned at page 402, that "in the Shetland Islands males (of *H. humuli*) are frequently found which closely resemble the females" (I have seen similarly varied males in Peterhead collections), would seem to confirm this theory; for the twilight of the north, at the season when the ghost-moth abounds, is so bereft of dusk that whiteness would not be needed to render the males visible.

It is possible that those acquainted with the habits of the other moths, of which Mr. Darwin speaks, may be able to reconcile their appearance with the rules of Sexual Selection which he has laid down so clearly and illustrated so fully in his last great work.

GEORGE FRASEK

169, Camden Road, London, N.W.